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"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

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THURSDAY, MAY 2, 1895.

THE BOOK OF THE DEAD.

The Papyrus of Ani in the British Museum. The Egyptian text with interlinear transliteration and translation, a running translation, introduction, &c. By E. A. Wallis Budge, Litt. D., Keeper of Egyptian and Assyrian Antiquities. Printed by order of the Trustees, 1895. (London: Longmans, Kegan Paul, &c.)

PERHAPS one of the most attractive and popular departments of science is that which treats of the early customs and beliefs of primitive man. Within recent years considerable attention has been directed to this subject. Not only have specialists, such as Mannhardt, Waitz, Bastian, and Tylor, to mention a few prominent names, devoted themselves to the collection and classification of material, but a great body of the reading public have followed their labours with intense interest, and have embarked on a course of original inquiry on their own account. The chief reason for this widespread study of comparative religion is to be sought in the fact that no demands are made on the student for any special training in order that he may appreciate its methods and results. Let him but have the passion of the collector and a love for his subject, and he is fully equipped for his work; all he requires beside are books that will yield reliable information concerning the folk-lore or superstition of any early or primitive race. Readers of NATURE, therefore, will be interested in hearing some account of a remarkable work, recently published by the Trustees of the British Museum, which deals with the religion of the oldest nation in the world whose records have survived to the present day.

The nation to which we refer, it is needless to say, are the Egyptians, whose civilisation on the banks of the Nile stretches back into a remote antiquity. Both the art and literature of this people were in the main the product of their religious belief in a future existence; what we possess of the former we owe to its preservation in the tomb, while a great part of the latter has come down to us in a body of religious compositions to which Egyptologists have given the comprehensive title of "The Book of the Dead." It is with "The Book of the Dead" that the work in question deals. In the year 1888 the Trustees of the British Museum acquired the largest and most perfect specimen of this composition as preserved by that class of papyri which date from the second half of the eighteenth dynasty (about B.C. 1500–1400). About four months ago the Trustees published a second edition of the *facsimile* of the papyrus, and now Dr. Wallis Budge, the Keeper of Egyptian and Assyrian Antiquities, has produced a volume dealing exhaustively with the contents of this unique document.

It would be impossible to treat at any length in a short review the many problems discussed in the work before us. We can, however, briefly indicate its general scope and contents. Dr. Budge has given a transliteration and literal translation of the hieroglyphic text, arranged interlinearly, which will be of great value to the student. This is followed by a running translation, together with a description and explanation of the various vignettes with which the papyrus is profusely illustrated—a portion of the work which will be welcomed by the general reader. Perhaps of even greater importance, however, is the Introduction. Here the author has traced in detail the history and growth of "The Book of the Dead," from its first appearance on the Pyramids of the fifth dynasty to its latest hieratic recension in the early centuries of the Christian era. From the hands of the priests of Hierapolis we follow the work to Thebes, where we first find it divided into definite sections or chapters, each with its distinctive title. Thence, through the closely allied version of the twentieth dynasty to Saïs, where each chapter received its definite place in the series, and the order there introduced continued in use down to the Greek occupation of the country. Having laid before the reader a critical digest of the external history of the work, Dr. Budge then turns to internal questions, and proceeds to summarise the chief aspects of Egyptian belief, supporting each of his theses with citations from the native literature. He treats at length of the legend of Osiris, so closely connected with the doctrine of eternal life, and thence passes to the Egyptian idea of God. This section is followed by a detailed

description of the gods of "The Book of the Dead," and of the principal geographical and mythological places mentioned therein. The practical side of Egyptian worship then engages our attention, and we see the priest performing the complicated system of ritual and ceremony that accompanied the burial of the dead; and, the ground having thus been cleared, one passes on to a consideration of the Papyrus of Ani itself. Ani, in whose honour the work was written, was chancellor of the ecclesiastical revenues and endowments of Abydos and Thebes. From the fact of his exalted official position, therefore, we may, with Dr. Budge, regard his Papyrus as "typical of the funeral book in vogue among the Theban nobles of his time."

In the course of the Introduction Dr. Budge has admirably distinguished the uses of the Egyptian word *neter*, which correspond to a transition from anthropomorphic and polytheistic ideas to a lofty monotheism. The derivation of the word is a moot point among Egyptologists, though all agree in rendering the word by "god." Its original signification, however, may be disregarded, for it does not affect the later history of the word, with which we are at present concerned. Whatever its origin, there is no doubt that the singular *neter* is often used to express an entirely different conception to that conveyed by *neteru*, its plural, the former being employed to designate a supreme god, the latter a number of powers and beings, which were held to be supernatural, but were finite and endowed with human qualities and limitations. The truth of this will be evident to any one who will read through the passages collected by Dr. Budge in support of his contention. Dr. Budge cites the similar difficulty that attaches to the interpretation of the Hebrew word *elohim*, a comparison that might be dwelt on with advantage. One point of difference, however, may here be noted. In the history of the Hebrews we can point to the exact period when the radical change from polytheism to the belief in one god took place. With the rise of the prophets in the ninth century B.C. the nation imbibed the loftier conception, and they assimilated the prophetic teaching with such effect, that, during the post-exilic collection of the national literature, all traces of their former polytheism were as far as possible obliterated. In their literature, therefore, as it has reached us, the earlier national beliefs have survived only in indirect allusions and in the form of single words. With the Egyptians, on the other hand, this change in conception can be ascribed to no particular epoch. We find the idea of a supreme god in existence as early as the fifth dynasty; yet throughout the whole period of Egyptian history there existed side by side with it the lower conception of half-human deities, and the belief in an eternal and infinite god was not considered inconsistent with legends concerning lesser deities, who could eat and drink, and, like men, grew old and died.

To this tolerance, or rather attachment, displayed by the Egyptians for their legends and traditional beliefs, students of comparative religion at the present day owe a lasting debt of gratitude. For many of the legends preserved in late papyri have been handed down unchanged from earlier times, while the earlier monuments themselves have escaped the fury of the iconoclast. We will refer to one such legend cited by Dr. Budge. In a text of the fifth dynasty, the deceased king Unas is described in the form of a god as feeding upon men and gods. He hunts the gods in the fields, and, having snared them, roasts and eats the best of them, using the old gods and goddesses for fuel; and, by thus eating their bodies and drinking the blood, he absorbs their divine nature and life into his own. Many parallels to this quaint legend might be cited from the primitive beliefs of other races.

We cannot conclude without a reference to the unpolemical spirit in which the book is written, which is perhaps the result of a scientific training in Semitic languages and literature having been brought to bear on the difficult problems of Egyptian religion. Throughout the work it is evident that one of the chief aims of Dr. Budge has been to assist the reader to understand the evidence which documents nearly 7000 years old are here made to produce, and to judge of its value for himself. To the anthropologist and the student of comparative religion we, therefore, believe the work will be equally valuable.

THE POLLINATION OF FLOWERS.

Over de Bevruchting der Bloemen in het Kempisch Gedeelte van Vlaanderen. By J. Mac Leod. With 125 Figures. (Gent: Vuylsteke, 1894.)

THIS book is prefaced with a historical introduction which traces the study of the biology of flowers from the appearance of the work of Camerarius in 1691 to the present day. Not only does the author give an account of the work of the various writers, but he also devotes a good deal of space to criticising their conclusions, and comparing them with one another. Of these criticisms, it may be noticed, that he considers that too much importance has been ascribed to the colours of flowers in attracting insect-visitors, and he adduces several facts in support of his view. From these examples it appears that there are certainly some cases in which the bright colours of flowers have not got the object of attracting insects; yet surely in the vast majority of cases, whether the development of bright colours was primarily for this object or not, the showy floral leaves act as advertisements to catch the eye of wandering insects. As the author substitutes no definite theory to account for the colouration of flowers, it seems probable that the old view will hold its ground.

The greater part of the book (about 430 pp.) is taken up with an account of the floral mechanisms of the plants found in East and West Flanders. The mechanisms of a large number of species are carefully described, and the descriptions are illustrated by many good woodcuts, in great part original, in a few cases borrowed from other authors. At the conclusion of the description of each species a list of their insect-visitors is given; these lists appear to be very complete, and will doubtless be useful for reference.

The latter part of the work is largely taken up with an endeavour to find a parallelism between the annual evolution of the various classes of plants and insects, classified according to their mutual biological relations. But the author admits that this attempt has not been successful.

The last section to which we would call attention is that which contains a description of a theory to explain why some plants are adapted for direct fertilisation, and others for crossed fertilisation. According to this theory, entomophilous plants have to make certain sacrifices in order to attract visitors in the shape of the substances needed in the formation of nectar and various perfumes, which are, to a large extent, drawn from the reserve-materials contained in the plant at the time of flowering. If these reserve-materials are present in considerable quantities, the plant will be able to produce much nectar, &c., and will attract many insects, and become adapted to crossed fertilisation. If, on the other hand, it has but little of these stores, it will be able to expend very little in attracting insects, but will have to keep the great part of its scanty stores for the maturation of its fruits and seeds. The consequence will be that the flowers of these latter plants will be but little visited by insects, and will become adapted to self-fertilisation. The author, while he admits that this theory is insufficient to explain certain observations, yet maintains that it is more general in its application than Warming's idea expressed with regard to the flora of Greenland. According to this latter author, crossed fertilisation may be considered the rule in the case of those plants which multiply rapidly by vegetative reproduction, while plants without this second method of reproducing their kind, and which must necessarily bring their seeds to maturity, are most usually adapted to self-fertilisation. It is, however, most probable that neither of these theories should be regarded as in itself giving all the determining causes for a plant becoming adapted to crossed or self-fertilisation, but as only expressing two of, it may be, many factors which are at work in moulding any given plant for one form of fertilisation or another. H. H. D.

OUR BOOK SHELF.

Emile Levier. A travers le Caucase. Notes et Impressions d'un Botaniste. 8vo. pp. 348. (Paris: Libraire Fischbacher.)

DR. LEVIER accompanied his botanical friend, Signor Stephen Sommier, on a tour through the Central Caucasus in 1890, the object being mainly to collect and study the flora of the mountains. The letters which he sent to his friends recording his impressions were published in a magazine without his knowledge, although not written for the public, and the present volume is practically a republication of the letters, edited by the author, and illustrated by numerous sketches and reproductions of photographs. Amongst the latter are several of Signor Vittorio Sella's fine pictures of Caucasian scenery, which, however, are not done justice to in the process blocks. The botanical results of the journey have been published for the most part in the *Bulletin* of the Italian Botanical Society, and only a list of the sixty-nine new species found is given in the book, such references to botany as occur in the text, though full of interest and presenting some acute generalisations, by no means preponderating over the miscellaneous observations of an intelligent tourist, and the pleasantly narrated incidents of travel. A list of thirty-seven species of lepidoptera collected by Dr. Levier is also given.

The two botanists were accompanied by an Italian peasant as hunter, cook and general assistant; and together they experienced few difficulties and no danger on their journeys through unfrequented regions for four months. After some excursions in the neighbourhood of Batum and of Tiflis, they started from Kutais for the journey across the range, going up the valley of the Rion and across the Latpari Pass into Swanetia. After traversing the valleys of Swanetia and Abkhasia, and making an excursion up the valley of the Kukurtli on the western slope of Elburz, they reached the northern plain by the valley of the Kuban. They returned to Tiflis by the coach road from Vladikavkas through the Dariel Pass heavily laden with more than ten thousand botanical specimens, the drying of which was a never-failing source of surprise and amusement to natives and Russian officials alike.

The spirit of holiday and nature-worship breathes through the whole book. Rarely, we believe, is a traveller in untrodden ways so able to appreciate to the full the delights of his surroundings as this light-hearted Swiss physician, whose high spirits and good-humour retain contagious qualities even through the pages of his book.

H. R. M.

Science Readers. By Vincent T. Murché. Books i. to iii. (London: Macmillan and Co., 1895.)

IN elementary schools where the rudiments of knowledge about properties and things are taught, these books may be introduced with advantage as reading books. The style is conversational, and every effort appears to have been made to convey the information in simple language, as well as to make it interesting.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Origin of the Cultivated Cineraria.

IN the recent discussion at the Royal Society, I used as an illustration of the amount of variation which could be brought about under artificial conditions in a limited time, the case of *Cineraria cruenta*, which I regarded as having given rise to the cultivated Cineraria.

This Mr. Bateson describes as "misleading."

I have read all he has to say, and, with the assistance of competent members of my staff, have carefully examined authentic specimens of all the species he names as having had a share in the parentage of the Cineraria.

Those species, if I understand him rightly, are four in number: *cruenta*, *aurita*, *populifolia* and *lanata*. They were all introduced into English horticulture, through Kew, between 1777 and 1780, and were figured and described by L'Héritier in his "Sertum Anglicum."

A technical discussion of the subject would necessarily take up a good deal of space, and would not be very interesting to readers of NATURE. Mr. Bateson refers to De Candolle's "Prodromus." It will be sufficient, perhaps, to say that had he studied that authority with care, he would have found that while *cruenta* is, like the modern Cineraria, herbaceous, *aurita*, *populifolia* and *lanata* are shrubby species. Further, while the modern Cineraria retains the exact foliage of *cruenta*, that of *aurita* and *populifolia* resembles the foliage of the white poplar; "folia populi albæ." Apart from the additional fact that *populifolia* has yellow flowers, I think I may confidently appeal to even the non-botanical eye as to whether the modern Cineraria exhibits anything of the white poplar character about it. As to *lanata*, its general aspect is sufficiently indicated by its specific name. It is represented by numerous specimens in No. 4 House at Kew, where Mr. Bateson may inspect it. He will probably then regret, for the sake of his reputation as a naturalist, that he committed himself to print on a subject on which he evidently possesses little objective knowledge.

I may add that in the discussion at the Royal Society, Mr. Bateson asserted to my surprise that the cultivated varieties of the *Camellia* could be distinguished by their leaves alone. I

interjected a doubt, but next day I carefully examined a large number of specimens here with a member of my staff, and we totally failed to confirm Mr. Bateson's statement.[1]

W. T. THISELTON-DYER.

Royal Gardens, Kew, April 29.

The Unit of Heat.

MR. GRIFFITHS, in a recent communication to the Royal Society, has called attention to the indefiniteness attending our present knowledge of the heat unit. In this connection I would wish to suggest—what indeed has long been present in my mind—that a unit of heat other than the present calorie is desirable. The present thermal unit is highly arbitrary, as well as most difficult of verification. This is true, whether we take the temperature at which the calorie is to be measured as 4° C. or 15° C. or as the temperature of minimum specific heat of water. The calorie owes its perpetuation to the method of mixtures—a laborious and inaccurate method of calorimetry—and dates from a period when the variations in the specific heat of water were not held of account.

If we do adhere to a specific heat of water calorie, it will be necessary to proceed as in the determination of the standard metre; obtain the more or less inaccurate measure of the primary unit in terms of some more accessible quantity.

My suggestion is that we start with an accessible unit. I think the latent heat of steam at the standard pressure has first claim. One gramme of saturated steam at 760 m.m. might be assumed to give up the unit quantity of heat in becoming water, without change of temperature. This unit might be called a therm, in order to avoid confusion with the existing unit. The specific heat of water would then stand as about 1·8 milli-therms. The larger value of the new unit commends itself as being more applicable to the problems of applied science; which, indeed, may be inferred from the fact that engineers often understand by the term calorie the kilogramme-degree.

I am aware that the change proposed is a radical one; but an appreciable change is better than a vexatious correction, and we know now that revision and change are inevitable.

In the definition of the proposed unit we replace the unreliable thermometer by one of the most trustworthy of instruments—the barometer; and our quantities of heat may be determined by the chemical balance, and, at 760 m.m., read directly upon the weights. We are sure of the purity of the material.

Trinity College, Dublin. J. JOLY.

The Study of Earthquakes in the South-East of Europe.

IN two recent notes in NATURE (vol. li. pp. 180, 468) attention has been drawn to the foundation by the Ottoman Government of a geodynamic section of the Imperial Meteorological Observatory at Constantinople. The new department has been placed under the direction of Dr. G. Agamennone, who for several years held a similar office at Rome, and who is well known to seismologists for the valuable work performed by him in Italy.

Not content with the foundation of a seismological observatory, Dr. Agamennone has also undertaken the organisation of earthquake studies throughout the Ottoman Empire, and he is anxious to extend this very important branch of his work so as to include the entire district within and bordering the eastern end of the Mediterranean. As there must be many readers of NATURE who are able, either directly or indirectly, to aid him in this attempt, I should be grateful if you would allow me to recommend it to their attention and support. Dr. Agamennone's address is "Observatoire Impériale Météorologique, Constantinople (Pera)."

That one of the most seismic regions of the globe should at last attract the organised study it deserves, and that the initiation of the requisite observations should have fallen into hands so experienced and capable, will be matters of gratification to those who are interested in the progress of seismology. No less desirable would it be that all the results of such observations should be contained in the pages of a single journal, and Dr. Agamennone's publication of a monthly seismic bulletin, of which the first two numbers have already been issued, is an additional reason for the concentration of records from the different countries concerned in the Turkish Office.

Birmingham, April 19. CHARLES DAVISON.

[1] This and the absence of variation from the feral form in the foliage of the cultivated Cineraria, are covered by the principles laid down by Darwin in "Animals and Plants under Domestication," vol. ii. pp. 17-220.

Uniformitarianism in Geology.

IN reference to Prof. Judd's excellent statement of the position of the uniformitarian, allow me to call attention to an argument which tends to show that, so far as earthquakes and volcanic eruptions are concerned, catastrophes may be of greater magnitude now than in earlier geologic times.

The violence of an explosion will depend largely on the amount of confinement and pressure to which the exploding compounds are subject, as well shown in the case of Kilauea where there is a constantly open vent and no violent eruptions as contrasted with the numerous catastrophic explosions of long dormant volcanoes whose vents had become sealed up with cores of solid lava. But it is admitted that the crust of the earth has been growing thicker during all geological time. It is therefore almost certain that, in the remoter epochs volcanic phenomena were more frequent but less violent than they have become now that the crust is thicker, and, in its lower portions, at all events, denser and more consolidated. The usual argument, that, because the interior of the earth was somewhat hotter in early times therefore volcanic phenomena were more violent, appears to me to be entirely fallacious. The liquid matter immediately below the crust would have been at the same temperature then as it is now; and if there were a more abundant supply of aqueous vapour and other gases, the thinner and more permeable crust would have allowed of their constant and comparatively easy escape.

I do not remember to have seen this consideration referred to in any discussion of the question, and I therefore submit the argument to the judgment of physical geologists.

ALFRED R. WALLACE.

Research in Education.

PROF. ARMSTRONG's trenchant indictment of the present methods of teaching science, is a little too much akin to Carlyle's fulminations against things in general—destructive but not constructive. Probably all good teachers are agreed upon the pernicious futility of the text-book and lecture-room cram system, and are in thorough accord as to the educational value of practical work; and are waiting only to learn or discover the best system of employing it. To this end destructive criticism helps but little. What is wanted is some definite scheme of work constructed by masters of practical instruction. Prof. Armstrong does certainly advocate what may be termed the "research method"; but it does not elucidate the question much, for it is difficult to understand how far he would extend this method. Would he, for instance, never mention Dalton's laws to students until, by a series of analyses, they were in a position to discover them for themselves? Or in the case of specific heat, how much information should be given before the beginners are set to investigate the phenomena alone? There are two ways of learning practically physical and chemical truths, either by repeating methods which have been explained and demonstrated, and then verifying each step by actual contact with real objects, and so acquiring real knowledge of fact and the application of theory, or by struggling to the truth by a process of trial and error. That the latter process, when successful, is the more stimulating to the intellect may be admitted, but that it is practically possible must be doubted. In introducing any new subject to the mind, surely broad outlines should be given first, and details filled in afterwards; observation requires teaching as much as any other faculty. Tyndall tells this story of Faraday. As Tyndall was about to show the latter an experiment, Faraday laid his hand on his shoulder and said, "Wait a minute; what am I to look for?" The application is plain—even Faraday felt the advantage of having the observer fore-armed.

Beginners know not what to observe, and cannot fashion experiments for themselves, and therefore it seems more rational, that students should have the recognised methods of science explained and demonstrated to them, and then be caused to repeat the necessary operations practically, numerical details being varied as in mathematical exercises. When thus equipped with sound theoretical knowledge and fair manipulative dexterity, they will be in a position to embark upon "research"; for they will then have acquired some power of observation, accuracy, and the faculty of making inferences. The "research method" *ab initio* appears like an attempt to teach a child to read before he knows his letters. I am fully conscious of my audacity in venturing into the lists, and am not ignorant of the sort of folk who "madly rush where angels fear to

tread"; but if I can elicit some definite scheme from Prof. Armstrong, I shall regard my own dialectic annihilation as a small price to pay for the ultimate gain. D. S. T. GRANT.

Chemical Laboratory, Lahore, Punjab.

A Lecture Experiment.

To show that chlorine will attack mercury, some mercury was shaken up in a covered gas jar filled with chlorine. On shaking, the sides of the jar and also the cover-glass became coated with a continuous film of mercury, as though the inside were silvered. After a short time, the film was eaten through, and patches of the white chloride produced. I have not seen this effect noticed in books, so it may be worth while to call attention to it.

C. J. WOODWARD.

Municipal Technical School, Birmingham, April 25.

VITALITY OF SEEDS.

THE duration of the vitality of seeds is perhaps the most important of the various phenomena of plant-life, especially when considered in connection with the introduction into a country of the economic plants of other countries. It is a subject that has engaged attention from very early times, and the literature relating thereto is considerable. Much of this, however, is of a traditional and unpractical character; but even if we confine ourselves to the demonstrable, or demonstrated, the subject is almost inexhaustible. There is such an infinity of variety in the behaviour of seeds under different conditions, that it is impossible in a short account, such as this must be, to do more than convey a general idea of the subject. Perhaps the best way to treat the question, apart from technicalities, is to consider the vitality of seeds under ordinary, and under extraordinary, conditions. In the development and germination of seeds, there is, in a sense, usually a period of gestation and a period of incubation, as in oviparous organisms of the animal kingdom; and the duration of these periods is within definable limits, under ordinary conditions, though seeds do not exhibit the same fixity of time in regard to development and vitality as eggs. The embryo of a seed is the result of the impregnation of the female ovum in the ovary or young seed-vessel, by the male element, generated in the anthers; and in the mature state this embryo may fill the whole space within the skin, or testa, of the seed, as in the bean and acorn; or it may be a comparatively minute body, as in wheat, maize, and other cereals: the rest of the seed being filled with matter not incorporated in the embryo. The difference is one of degree in development. In the one case, the growing embryo has absorbed into its own system, as it were, before germination or the beginning of the growth of the embryo into a new plant, the whole of the nutrient material provided in the seed for reproduction; whereas, in the latter case, the process of absorption and utilisation of the "albumen," or nutrient matter, takes place after the seed is detached from the parent plant, and during the earliest stage of growth of the new plant; so that the plant is nourished until it has formed organs capable of assimilating the food obtainable from the atmosphere and earth. Between these two extremes of development of the embryo, or future plant, before organic connection with the parent ceases, there is every conceivable degree and variety; and, as will presently be explained with examples, some plants are viviparous, in the sense that the embryo commences active life before being severed from the parent, so that when this occurs the plant is in a position to draw its sustenance from unassimilated or inorganic materials. Now it is a curious and unexplainable fact that certain seeds exhibiting the extremes of embryonal development, instanced in the bean and wheat, are equally retentive of their germinative power. The longevity, if it may be so called, of seeds is exemplified in "exalbuminous" seeds as well as in "albuminous" seeds of every degree. It should be mentioned, however, that the difference is not so much one of assimilation or development as of the earlier or later transfer of the nutrient matter of the seed to the embryo or plantlet. Assuming the perfect maturation of a seed, certain conditions are necessary to quicken its dormant vitality: and the two principal factors are heat and moisture, varying enormously in amount for different plants, and acting much more rapidly on some seeds than on others, even when the amount required is much the same. Neither under natural nor under artificial conditions will some seeds retain their vitality more than one season; and all the resources of the accumulated experience of seed-importers from distant countries are insufficient in some cases to maintain their vitality. It is not altogether because the interval between the dispersal and the germination of the seed, under ordinary conditions, is necessarily longer; but rather because in the one case the conditions under which a seed will germinate are much more restricted than in the other. Let us now examine the natural conditions under which seeds are commonly produced and dispersed, in relation to the retention of their vitality; and we shall learn how much more it depends on their nature, or natural means of protection, than on the seasons. An oak tree sheds its acorns in autumn, and the leaves which fall afterwards afford them some protection from frost and excessive dryness. But the leaves might be blown away from one spot, and the acorns exposed to intense frost or drought, either of which will speedily kill them. In another spot the leaves may drift into thick layers, with an excessive accumulation of moisture, causing decay of the underlying acorns; and there are many other unfavourable conditions which may destroy the vitality of the acorn. It is apparently impossible, however, to preserve an acorn's vitality by any artificial means for more than one season.

The scarlet-runner bean loses its germinative power on exposure to comparatively slight frost, the degree depending upon the amount of moisture in it; yet it will retain its vitality for an almost indefinite period under favourable artificial conditions. In both of the examples given, germination would naturally follow as soon after maturation as the conditions allowed. The seeds of the hawthorn behave differently. Each haw contains normally three to five seeds, every one of which is encased in a hard, bony envelope, in addition to its proper coat or testa. Committed to the earth, and under the most favourable conditions, these seeds do not germinate till the second year, and often not so soon. In this instance prolongation of vitality is probably due in some measure to the protective nature of the shell enclosing the seed.

Returning to seeds in which the embryo or plantlet forms only a very small part of the whole body, wheat may be taken as a familiar and easily observed illustration of a seed, the vital energy of which requires very little to stimulate it into active growth; and yet this same seed, having no special protection in the way of coating, will retain its vitality as long, perhaps, as any kind of seed, if not under the influence of moisture. The primary condition to the preservation of vitality in a seed is perfect ripeness. Unripe seeds of many kinds will germinate and grow into independent plants if sown immediately after removal from the parent. The facility with which immature wheat will germinate is most disastrously exemplified in a wet harvest, when the seeds will sprout while the corn is standing or in sheaf; thus destroying more or less completely the value of the grain for flour, as the starch or flour is consumed in the development of the embryo, or what is left is so deteriorated by chemical change that it is not good for food. There is perhaps no other seed more susceptible to moisture, and none less affected by dryness, or by heat or cold in the absence of moisture.

The kind of vivipary exhibited by the wheat is occasionally observed in various other plants; and sometimes the seeds of pulpy fruits germinate in the fruit. There is also a class of plants in which vivipary is normal. Prominent in this class are the mangroves (*Rhizophoreæ*, &c.) of muddy sea-shores in the tropics. In these plants there is a remarkable adaptation to conditions, which ensures their reproduction. From the very inception of the embryo there is no apparent interruption of active vitality in its development and germination. In the earliest stage the cotyledons or seed-leaves are formed, and the radicle or future primary root is represented by a very small point. When the former have attained their full development, which is not great, the latter begins to grow and rapidly increases in size. Each fruit or seed-vessel, it should be mentioned, contains only one seed, the rootlet of which points to the apex of the fruit. Soon this rootlet pushes its way through the apex of the fruit, and grows into a spindle-shaped body of great density and length; the cotyledons or seed-leaves remaining partly inside the fruit, and acting as an organ of absorption from the parent plant to nourish the seedling. In *Rhizophora mucronata* this radicle attains a length of two to three feet, and the seedling eventually falls, and by its own weight penetrates and sticks in the mud, leaving the fruit, containing the exhausted cotyledons, attached to the tree, where it dries up. Another singular adaptation to conditions is the vital development of the seeds of aquatic plants which ripen their seeds on or under water. *Vallisneria* is a remarkable instance of this. The unisexual flowers are formed under water: the female on long coiled stalks, which at the right period uncoil, and the flower rises just above the surface of the water. Simultaneously the short-stalked male flowers are detached from the base of the leaf-stalks, and also rise to the surface. After impregnation has taken place, the stalk of the female flower coils up again, and draws the seed-vessel down under water, where the seeds ripen.

It has been explained that heat, moisture, and air are necessary to the germination of seeds, varying immensely for different seeds. We come now to the behaviour of certain seeds under the influence of an unusual or unnatural amount of moisture, heat or cold, especially in relation to the length of the duration of the exposure to any one of these factors. It has been proved beyond dispute, by actual experiment, that the vitality of certain seeds, notably various kinds of bean and convolvulus, is not impaired by immersion in sea-water (or rather floating and partially submerged) for a period of at least one year; and that after having been kept quite dry for two or three years. Plants are actually growing at Kew from seeds treated as described; and some years ago several seeds of *Entada*, cast ashore in the Azores, whither they had been transported by the Gulf Stream, were raised at Kew. So far as at present known, all the seeds that will bear very long immersion without injury have an intensely hard, bony, or crustaceous coat, that would withstand boiling for a minute or two without killing the embryo. Yet it is difficult to understand this power of resistance, especially after being kept dry for a long time. This imperviousness to water explains the wide distribution of many sea-side plants, the seeds of which are conveyed by oceanic currents. How long such seeds would retain their vitality in water is uncertain, because experiments have not reached the limit. Many readers will remember Darwin's experiments in this connection; but it should be borne in mind that they were chiefly with seeds of plants not at all likely to be dispersed by the sea.

It has already been stated that some seeds will bear immersion in boiling water for a short time, and gardeners occasionally practise this treatment to accelerate the germination of hard-coated seeds. But seeds of all kinds will bear for a considerably longer period a much higher dry temperature than soaking in water of the same temperature. It is recorded, by trustworthy authorities, that the seeds of many plants (poppy, parsley, sunflower, and various kinds of grain, for instance) if perfectly dry, do not lose their vitality when subjected to a temperature of 212° F. for forty-eight hours; and for shorter periods to a much greater heat. The result in most cases, though not all, is a considerable retardation of germination. Dry grain is equally impervious to cold. In 1877, seedling wheat was exhibited at the Linnean Society that had been raised at Kew from grain that had been exposed to the intense cold of the Arctic expedition of 1874 to 1876. The next question that arises is: how long do seeds retain their vitality when stored in the ordinary ways adopted by dealers? As a rule, seedsmen and gardeners prefer new seed, because a larger percentage germinates; and mixing old seeds with new, tells its own tale in irregular germination. Nevertheless, there are many seeds that retain their vitality from five to ten years sufficiently well to be depended upon to yield a good crop. Old balsam seed, other things being equal, has the reputation of yielding a larger proportion of double flowers than new; and some gardeners consider that cucumber seed of four or five years of age gives better results than the seed of the previous year. As already mentioned, perfectly ripened seed will retain its vitality longer than imperfectly ripened seed. In illustration of this, we note that carrot seed grown in France retains its germinative power, on the average, longer than English-grown seed, owing to climatal differences.

There is one other natural condition in relation to the vitality of seeds that should be mentioned; that is, the duration of the vitality of seeds on the mother plant. Some of the Australian *Proteaceæ*, and some of the fir trees, especially North American, bear the seed-vessels containing quick seeds of many successive seasons; and only under the influence of excessive drought or forest fires do they open and release the seed. Rapid forest fires are often not sufficient to consume the cones, but sufficient to cause them to open and free the seed for a succession of trees. The unopened cones of thirty years have been counted on some fir trees; and it is averred that the first seed-vessels of some proteaceous trees do not open to shed their seed, under ordinary conditions, until the death of the parent plant, so that a tree may bear the accumulated seed of half a century or more.

Finally, a few words respecting the extreme longevity attributed to certain seeds. The reputed germination of "mummy wheat," from two to three thousand years old, has been the theme of much writing; but the results of careful subsequent experiments with grain taken from various tombs do not support the doubtless equally conscientious, though less skilfully conducted, experiments, supposed by some persons to have established the fact of wheat of so great an age having germinated. Indeed it is now known that the experiments mainly relied upon to prove this long retention of vitality were falsified by the gardener who had charge of them. Nevertheless, there is no doubt that some seeds do retain their vitality for a very long period, as is proved by numerous well-authenticated instances. Almost every writer on physiological botany cites a number of instances. Kidney beans taken from the herbarium of Tournefort are said to have germinated after having been thus preserved for at least 100 years. Wheat and rye are also credited with having retained their vitality for as long a period. Seeds of the sensitive plant (*Mimosa pudica*) kept in an ordinary bag at the Jardin des Plantes, Paris, germinated freely when sixty years old. A long list might be made of seeds that have germinated after being stored for twenty-five to thirty years. If seeds retain their vitality for so long a period as this under such conditions, it is quite conceivable that seeds buried deep in the earth, beyond atmospheric influences, and

where there was not excessive moisture, might retain their germinative power for an almost indefinite period; and the fact that plants previously unknown in a locality often spring up where excavations have been made, bear out this assumption. The same thing happens in arable land, should the farmer plough deeper than usual; and deeper tillage, which would otherwise be beneficial, is often avoided on this account. A careful writer like Lindley states, though without qualification, that he had raspberry plants raised from seed taken from the stomach of a man, whose skeleton was found thirty feet below the surface of the ground. Judging from coins found at the same place, the seeds were probably 1600 or 1700 years old. One more example of seeds germinating that are supposed to have been buried some 1500 to 2000 years. About twenty years ago, on the removal of a quantity of slack of the ancient silver mines of Greece, several plants sprang up in abundance previously unknown in the locality. Among these was a species of *Glaucium*, which was even described as new; and it is suggested that the seed may have lain dormant for the long period indicated. But there is not the amount of certainty about any of these assumed very old seeds to convince the sceptical or to establish a fact. It remains yet for somebody to institute and carry out careful investigations where excavations are being made.

W. BOTTING HEMSLEY.

TERRESTRIAL HELIUM (?).

AT the meeting of the Royal Society on Thursday last (April 25), two papers dealing with the nature of the gas from uraninite were presented. We print both papers in full.

ON A GAS SHOWING THE SPECTRUM OF HELIUM, THE REPUTED CAUSE OF D_3, ONE OF THE LINES IN THE SPECTRUM OF THE SUN'S CHROMOSPHERE.[1]

In the course of investigations on argon, some clue was sought for, which would lead to the selection of one out of the almost innumerable compounds with which chemists are acquainted, with which to attempt to induce argon to combine. A paper by W. F. Hillebrand, "On the Occurrence of Nitrogen in Uraninite, &c." (*Bulletin of the U.S. Geological Survey*, No. 78, p. 43), to which Mr. Miers kindly directed my attention, gave the desired clue. In spite of Hillebrand's positive proof that the gas he obtained by boiling various samples of uraninite with weak sulphuric acid was nitrogen (p. 55)—such as formation of ammonia on sparking with hydrogen, analysis of the platinichloride, vacuum-tube spectrum, &c. I was sceptical enough to doubt that any compound of nitrogen, when boiled with acid, would yield free nitrogen. The result has justified the scepticism.

The mineral employed was cleveite, essentially a uranate of lead, containing rare earths. On boiling with weak sulphuric acid, a considerable quantity of gas was evolved. It was sparked with oxygen over soda, so as to free it from nitrogen and all known gaseous bodies except argon: there was but little contraction; the nitrogen removed may well have been introduced from air during this preliminary experiment. The gas was transferred over mercury, and the oxygen absorbed by potassium pyrogallate; the gas was removed, washed with a trace of boiled water, and dried by admitting a little sulphuric acid into the tube containing it, which stood over mercury. The total amount was some 20 c.c.

Several vacuum-tubes were filled with this gas, and the spectrum was examined, the spectrum of argon being thrown simultaneously into the spectroscope. It was at once evident that a new gas was present along with argon.

Fortunately, the argon-tube was one which had been made to try whether magnesium-poles would free the argon from all traces of nitrogen. This it did; but hydrogen was evolved from the magnesium, so that its spectrum was distinctly visible. Moreover, magnesium usually contains sodium, and the D line was also visible, though faintly, in the argon-tube. The gas from cleveite also showed hydrogen lines dimly, probably through not having been filled with completely dried gas.

On comparing the two spectra, I noticed at once that while the hydrogen and argon lines in both tubes accurately coincided, a brilliant line in the yellow, in the cleveite gas, was nearly *but* not *quite* coincident with the sodium line D of the argon-tube.

Mr. Crookes was so kind as to measure the wave-length of this remarkably brilliant yellow line. It is 587·49 millionths of a millimetre, and is exactly coincident with the line D_3 in the solar chromosphere, attributed to the solar element which has been named *helium*.

Mr. Crookes has kindly consented to make accurate measurements of the position of the lines in this spectrum, which he will publish, and I have placed at his disposal tubes containing the gas. I shall therefore here give only a general account of the appearance of the spectrum.

While the light emitted from a Plücker's tube charged with argon is bright crimson, when a strong current is passed through it, the light from the helium-tube is brilliant golden yellow. With a feeble current the argon-tube shows a blue-violet light, the helium-tube a steely blue, and the yellow line is barely visible in the spectroscope. It appears to require a high temperature therefore to cause it to appear with full brilliancy, and it may be supposed to be part of the high-temperature spectrum of helium.

The following table gives a qualitative comparison of the spectra in the argon[1] and in the helium-tubes.

	Argon-tube.	*Helium-tube.*	
Red ..	1st triplet.	1st triplet.	Equal in intensity.
	2nd pair.	2nd pair.	,, ,,
	Faint line.	Faint line.	,, ,,
	Stronger line.	Stronger line.	,, ,,
	Brilliant line.	Dull line.	Weak in helium.
	Strong line.	Very dim line.	
Red-orange	Moderate Line.	Moderate line.	Equal in intensity.
	,, ,,	,, ,,	,, ,,
	,, ,,	,, ,,	,, ,,
Orange	Faint line.	Faint line.	,, ,,
	Triplet.	Triplet.	,, ,,
Orange-yellow	Pair.	Pair.	,, ,,
			,, ,,
Yellow	Absent.	Brilliant.	W = 587·49 (the helium line, D_3).
Green	7 lines.	7 lines.	Equal in intensity.
Green-blue	5 lines.	5 lines.	,, ,,
	Absent.	Faint.	In helium only.
	Absent.	Brilliant.	,, ,,
Blue ..	Absent.	8 lines.	,, ,,
Blue-violet	3 lines, strong.	Barely visible, if indeed present at all.	
	2, fairly strong.	2, fairly strong.	Equal in intensity.
	Absent.	Bright line.	In helium only
	Absent.	4 bright lines.	
Violet	Violet pair.	Violet pair.	Equal in intensity.
	Single line.	Single line.	,, ,,
	Triplet.	Triplet.	,, ,,
	Triplet.	Triplet.	,, ,,
	Pair.	Pair.	,, ,,

It is to be noticed that argon is present in the helium-tube, and by the use of two coils the spectra could be made of equal intensity. But there are sixteen easily visible lines present in the helium-tube only, of which one is the magnificent yellow, *and there are two red lines strong in argon and three violet lines strong in argon, but barely visible and doubtful in the helium-tube.* This would imply that atmospheric argon contains a gas absent from the argon in the helium-tube. It may be that this gas is the cause of the high density of argon, which would place its atomic weight higher than that of potassium.

It is idle to speculate on the properties of helium at such an early stage in the investigation; but I am now preparing fairly large quantities of the mixture, and hope to be able before long to give data respecting the density of the mixture, and to attempt the separation of argon from helium.

[1] Preliminary Note, by Prof. William Ramsay, F.R.S.

[1] The tube then used was the one with which Mr. Crookes' measurements of the argon spectrum were made. It contains absolutely pure atmospheric argon.

ON THE NEW GAS OBTAINED FROM URANINITE.[1]

ON March 28, Prof. Ramsay was so good as to send me a tube containing a new gas obtained by him from uraninite (cleveite) showing a line in the yellow which was stated to be of the same wave-length as D_3 which I had discovered in 1868. This line Dr. Frankland and myself shortly afterwards suggested might be a line of hydrogen not visible under laboratory conditions, but solar work subsequently showed that this view was untenable, although the gas which produced it was certainly associated with hydrogen.

Subsequently other chromospheric lines were found to vary with the yellow line, and the hypothetical gas which gave rise to them was provisionally named helium, to differentiate it from hydrogen.

It was therefore of great interest to me to learn whether the new gas was veritably that which was responsible for the solar phenomena in question; and I am anxious to tender my best thanks to Prof. Ramsay for sending the tube to enable me to form an opinion on this matter. Unfortunately it had been used before I received it, and the glass was so blackened that the light was invisible in a spectroscope of sufficient dispersion to decide the question.

On March 29, therefore, as Prof. Ramsay was absent from England, in order not to lose time, I determined to see whether the gas which had been obtained by chemical processes would come over by heating in vacuo, after the manner described by me to the Society in 1879, and Mr. L. Fletcher was kind enough to give me some particles of uraninite (Broggerite) to enable me to make the experiment.

This I did on March 30, and it succeeded; the gas giving the yellow line came over associated with hydrogen in good quantity.

I have since obtained photographs of the gas, both in vacuum tubes while the Sprengel pump has been going; and at atmospheric pressure over mercury. To-day I limit myself to exhibiting two of these photographs.

One of the photographs exhibits a series of spectra taken during the action of the pump. The two lower spectra indicate the introduction of air by a leak, after the capillary had cracked near one of the platinums, giving us on the same plate the banded and line spectrum of air. These prove that there was no air present in the tube when the fourth spectrum was taken. This photograph has not yet been finally reduced, but a preliminary examination has indicated that most of the lines are due to the structure spectrum of hydrogen, but not all of them.

Among the lines which cannot be referred to this origin are two respectively near λ 4471, and λ 4302, which have been observed in the chromosphere, 4471 being as important as D_3 itself from the theoretical point of view to students of solar physics.

Whilst spectrum No. 4 was being photographed with the capillary tube end-on wise, eye observations were made in another spectroscope directed sideways at it. I give from the Laboratory Note Book the observations I made while photograph No. 4 was being taken, to show that the yellow line was visible during the whole exposure.

Thursday, April 4, 1895. *Plate F. Exposure 4.*

10 minutes' exposure.

4.42 Exposure started.
4.43 Yellow line brightening up considerably.
4.44 Suddenly as bright as hydrogen.
4.45 Yellow line double.
4.46 Comparison with D gives yellow line in position of D_3.
4.47 Pump much less full, 7 c.c. of gas collected. Yellow line much brighter.
4.48 Air break introduced. Line still visible, but very faint. Hydrogen lines getting brighter, and some fresh lines appearing in green.
4.48.5 Air break and jar removed. Yellow line the only one seen, being as bright as C. Line in green the only other line visible.
4.50 Replaced jar. Yellow brightening and the other lines more refrangible, brightening with it.
4.51 Very bright. Steeple nearly full of gas.
4.52

The lines which appear both in the photographs of the capillary tube and of the gas collected over mercury are as follows. The lines indicated by an asterisk are near lines recorded in the chromosphere by Young or myself, or photographed during the eclipse of 1893:—

Micrometer reading.	Wave-length (Rowland).
3·2495	4581*
·2917	4523*
·2981	4513*
·3234	4479
·3316	4469·5*
·4146	4368
·5740	4196*
·5884	4181
·5933	4177*
·6139	4156*
·6176	4152·5*
·6262	4144*
·6290	4141

With regard to the observations in the visual spectrum, I have not found the uraninite gas to contain the argon lines as given by Mr. Crookes, nor, with the exception of the yellow line, do I get the special lines noted by him in the gas. (Four of these, out of six, seem possibly to be due to nitrogen.)

But I do get lines nearly coinciding with chromospheric lines discovered by me in 1868.

On November 6 of that year I suspected a line less refrangible than C, and so near it that when both were showing brilliantly the pair appeared double, like D in a spectroscope of moderate dispersive power.

Later I discovered another line at 6678·3 (R), which was observed to vary with D_3. There is a line in this position, with the dispersion employed, in the spectrum of the new gas. This line has also been seen by Thalén, as stated by Prof. Cleve in a communication to the Paris Academy (*Comptes rendus*, April 16, p. 835); but the other lines given by him (with the possible exception of the one at 5016), have not been recorded by me.

Although I have at present been unable to make final comparisons with the chromospheric lines, the evidence so far obtained certainly lends great weight to the conclusion that the new gas is one effective in producing some of them, and it is suggested by the photographs that the structure lines of hydrogen may be responsible for others.

I may state, under reserve, that I have already obtained evidence that the method I have indicated may ultimately provide us with other new gases the lines of which are also associated with those of the chromosphere.

Messrs. Fowler, Baxandall, Shackleton and Butler assisted at various times in the investigation.

[1] Preliminary [illegible] Royal Society [illegible]

NOTES.

WE regret to report that Prof. Huxley is still in a critical state of health. The slight improvement noticed in his condition last week appears not to have been maintained. It is more than eight weeks since his illness began with an attack of influenza, from the effects of which he is now suffering.

M. NORDENSKIÖLD has recently discovered a uranium containing mineral which may prove of great interest at the present time. It forms carbonaceous beds of which the ashes contain two to three per cent. of uranium, and, in addition, traces of nickel and rare earths. This uraniferous material is said to yield a considerable quantity of *nitrogen*.

DR. RICHARD HANITSCH has been appointed Curator of the Raffles Museum at Singapore. Dr. Hanitsch has occupied for some years the post of Demonstrator of Zoology in University College, Liverpool, and is the author of a number of useful papers on the British Sponges.

THE third centenary of Christian Huygens will shortly be reached; for that celebrated Dutch physicist, astronomer, and mathematician died at the Hague on June 8, 1695. His investigations have been reviewed at length in these columns during recent years, and *Die Natur* for April 21 contains a notice concerning them.

The specimen of the Great Auk, to which we referred in these columns last week, has been sold to the Edinburgh Museum for £350.

Dr. Gordon E. Moore, well-known as a chemist, died at New York on April 16. Prof. Gustav Hirschfeld, a distinguished archæologist, has just died at Wiesbaden. We also notice the death of Prof. K. Thiersch, Professor of Surgery in Leipzig University.

Prof. Lloyd Morgan will lecture on "Habits of Birds," at the Royal Victoria Hall, Waterloo Bridge Road, on May 7. Other science lectures to be given during this month are: "Electric Tram Cars," by Dr. J. A. Fleming, F.R.S.; "The History of a Myth," by Prof. Sollas, F.R.S.; and "The Life of a Star," by Dr. A. Fison.

Gilbert White's original manuscript of the "Natural History of Selborne," in the form of letters to Thomas Pennant and Daines Barrington, first printed in 1789, was sold by auction last week by Messrs. Sotheby, Wilkinson, and Hodge. The manuscript contains many passages not printed in the several editions of the book, and has never been out of the possession of the lineal descendants of the author. It was bought by Mr. Pearson for £294.

The *Weekly Weather Report* of April 27 shows that some very heavy falls of rain occurred during the week; in nearly all districts amounts of an inch or upwards were measured, while over the greater part of England the fall was more than double the mean. But the amount of rainfall since the beginning of the year is still below the average, except in the north-east of England. The greatest deficiency is in the west of Scotland, where it amounts to about seven and a half inches.

The startling advance in market price of petroleum gives interest to the question of exhaustibility of the supply, following close upon the great decrease in supply of natural gas. In the height of the natural gas excitement, the warning of science was too little heeded, and lavish waste hastened the collapse. In 1887 the atmosphere of Pittsburgh was wonderfully clear, owing to the use of this new fuel; but Pittsburgh is again begrimed and sooty.

At the annual meeting of the National Academy of Sciences, recently held at Washington, Prof. Marsh, who has been president for several terms, was succeeded by Prof. Wolcott Gibbs, of Cambridge, who was elected for the ensuing term of six years, while Prof. Asaph Hall was re-elected home secretary. Prof. Alexander Agassiz is foreign secretary, and the members of the Council elected are Profs. George J. Brush, Othniel C. Marsh, Benjamin A. Gould, George H. Goodale, Simon Newcomb, and Ira Remsen.

A three days' conference on sanitary progress and reform was held at Manchester last week. A meeting introductory to the conference was held in the museum of Owens College, at which Prof. Boyd Dawkins delivered an address on prehistoric traces of sanitation. At the annual meeting of the Manchester and Salford Sanitary Association, in connection with which the conference was held, it was resolved that a Smoke Abatement League should be formed. Sir H. Roscoe, who afterwards took the chair at the conference session, pointed out that though attention was paid to the smoke from factory chimneys and from manufacturing operations, the larger question of the smoke from ordinary household fires was often neglected.

A very serious disaster is reported from France. A dam holding in check an immense reservoir of the Eastern Canal at Bousey, near Epinal, broke down on Saturday morning for a distance of some 300 feet. The torrent of water thus set free swept through Bousey, Avière, Uxegney, and Sanchey, carrying all before it, and washed away portions of the railway lines of Jussey and Nancy. Many bridges were carried away, and a great number of people were drowned. The Bousey reservoir (says the Paris correspondent of the *Times*) contained seven million cubic metres of water. The dam, which was constructed between 1879 and 1884, and was strengthened in 1888-89, was 60 feet thick at the base, and the foundation is laid in sandstone. According to a report sent out by the Minister of Public Works, there have never been any signs of weakness in the structure. Attempts are being made to throw the responsibility for the accident upon the engineers who superintended the strengthening of the dam six years ago.

A number of interesting objects obtained during the excavations of the Roman city at Silchester are on view at the Society of Antiquaries. During the past five years, the excavations have been carried on by Messrs. St. John Hope, Fox, Jones, and Stephenson, and some very valuable results were obtained last year. Twelve rectangular enclosures or buildings were found, all of the same type, and containing furnaces obviously of an industrial character and of various sizes, some of them being circular and others oblong. It is believed that these buildings and their adjuncts were devoted to the dyeing industry, and this conjecture is made probable by the large number of wells discovered, one of which was of peculiar and unusual construction. The circular furnaces correspond exactly with a dyeing furnace at Pompeii. They were, there is every reason to believe, used for dyeing. But there are a number of other furnaces with a straight flue, which are supposed to have been intended for drying. There are also traceable several rooms which, it is presumed, were intended for the storage of goods and materials, and open spaces with no remains of flues which may have been used for bleaching grounds. A number of querns for hand-grinding the madder-roots used for dyeing purposes have also been discovered.

The sixty-sixth anniversary meeting of the Zoological Society was held on Tuesday, with the President, Sir William H. Flower, K.C.B., F.R.S., in the chair. Dr. P. L. Sclater, F.R.S., read the report of the Council, in which it was announced that the silver medal of the Society had been awarded to Mr. Henry H. Johnston, C.B., H.M. Commissioner for British Central Africa, for his distinguished services to all branches of natural history by his collections made in Nyasaland, which had been described in the Society's *Proceedings*. The total receipts of the Society for 1894 amounted to £25,107 0*s.* 7*d.* The number of visitors to the Gardens during the year was 625,538, the corresponding number in 1893 having been 662,649; the decrease in the number of entrances (37,111) being due to the unfavourable weather of 1894. The number of animals in the Society's collection on December 31 last was 2563, of which 669 were mammals, 1427 birds, and 467 reptiles. Amongst the additions made during the past year, eleven were specially commented upon as of remarkable interest, and in most cases representing species new to the Society's collection. Among these were two remarkably fine specimens of the Hamadryad snake, a young white-tailed gnu (born in the Gardens), an eland of the striped form from the Transvaal (obtained by purchase), two giant tortoises, a young male Pleasant antelope, 2 Somali ostriches of the blue-skinned variety, 10 Surinam water-toads, a Pel's owl, and 2 tree kangaroos. About 30 species of mammals, 12 of birds, and 1 of reptiles had bred in the Society's Gardens during the summer of 1894. The Right Hon. George Denman, F. Du Cane Godman, F.R.S., Sir Hugh Low, G.C.M.G., Dr. St. George Mivart, F.R.S., and Osbert Salvin, F.R.S., were elected into the Council in the place of the retiring members, and Sir William H. Flower was re-elected President,

Charles Drummond, Treasurer, and Dr. Sclater, Secretary, to the Society for the ensuing year.

The first of the two conversaziones held at the Royal Society every year, takes place as we go to press. Annual receptions and exhibits, conducted upon much the same lines, are gradually being instituted by learned societies in various parts of the world. The New York Academy of Sciences recently held a similar exhibition, at which five hundred different objects of scientific interest were shown. From a report in the *Scientific American*, it appears that many of the exhibits were of a very striking character. A number of photographs of comets, of the Milky Way, and of star spectra, were shown by Profs. Barnard and Keeler, of the Lick and Allegheny Observatories. One of the most novel exhibits in physics, was a series of Chladni figures, shown by Prof. Alfred M. Mayer. The figures were formed in white sand upon vibrating metallic plates. Prof. Mayer's process consisted in fixing the sand upon a black background after the figures had been formed, by means of a fixative spray. These plates demonstrated the truth of Lord Rayleigh's theoretical deductions, and differed radically from all figures which are shown in text-books in the fact that none of the lines intersect. The physical exhibit was an extensive one, including a large number of instruments for spectroscopic, as well as for sound and light, measurements. The mineralogical exhibit included about one hundred objects. Biology was represented by preparations of nerve cells from the brain and spinal cord, by Prof. Golgi's method; and there were also shown several series of similar pictures bearing upon problems of inheritance, both in animals and plants. Bacteriology, mechanics, physiology, experimental psychology, anatomy, geology, and palæontology all took part in the exhibition. In vertebrate palæontology, the main exhibit was that showing the evolution of the horse. The series connecting the oldest known horse of the Lower Eocene period with the modern horse was probably the most complete which has ever been brought together. The little four-toed horse, recently acquired by the American Museum of Natural History from the collection of Prof. Cope, of Philadelphia, was exhibited. Although fully matured, it is only $3\frac{1}{2}$ hands high. The skull and limbs, nevertheless, display the characteristics of the horse. The teeth are short and simple; the limbs are scarcely larger in diameter than a good-sized pencil, and there are four toes, all resting upon the ground, in the fore-foot. A remarkable series of feet was also exhibited, giving all the stages between this four-toed horse and the modern one-toed animal. The reception at which the exhibits were shown was so successful that it has been decided to hold a similar one every year.

Dr. Bertram Windle contributes a paper to the *Journal of Anatomy and Physiology*, "On the effects of Electricity and Magnetism on development." The observations recorded were made on developing silkworms, trout, and chick embryos. In the case of the chick, the number of abnormally developed embryos was much greater in eggs incubated around the poles of a strong magnet than usual. With one exception all the malformations were associated with deficient development of the medullary area. Dr. Windle has not conclusively shown that this large proportion of abnormal embryos was actually due to the presence of the magnet, yet his results on the whole agree with those of Maggiorani, although certain points of difference were observed in the deformed embryos. The eggs of the silkworm moth were found to develop quite normally in a strong magnetic field. An electric current passing through a tank in which trout ova had been placed, seemed to produce an arrest of development. Dr. Windle concludes from his own observations and those of other authors, "that electricity produces an arresting effect upon development," while it is "very doubtful whether a magnetic field has any definite effect upon development or not."

A recent number of *Comptes rendus* contains an interesting paper by M. Branly, on the rate of loss of an electric charge due to the effect of light in the case of badly-conducting bodies. When the source of illumination is a body heated to a dull red, it is the condition of the illuminating surface which plays the chief part in the phenomenon. The nature of the charged body seems to have no effect. In the case where the illumination is rich in highly refrangible rays, however, the case is quite different, and the chief results obtained are as follows:—A disc of wood or marble, polished or unpolished, shows a marked loss of electricity when illuminated. If the disc is negatively electrified, the loss is more rapid than if it is positively electrified; but the difference is very much less marked than is the case with metal discs, particularly if they are polished. Similar results are obtained with cardboard, terra-cotta, and glass heated to 100°. The loss of a positive charge is rapid, while that of a negative one is slow in the case of varnished wood, or wood coated with a thin layer of oil, paraffin or tallow. With a metal disc coated with tallow, the loss when negatively electrified is slow, while the loss when positively electrified is very rapid. If a disc of polished wood, in which the loss of a negative charge is more rapid than that of a positive one, though the difference is not very marked, has the surface covered with a thin coating of plumbago, the loss with a negative charge becomes much more rapid than with a positive one. A metal plate covered with grease only loses a negative charge very slowly, the rate of loss of a positive charge being rapid. If, however, a thin coating of copper filings is spread over the tallow by means of a sieve, the loss with a positive charge becomes much more rapid than with a negative one. If powdered aluminium is used in the place of copper, the rates of loss in the case of positive and negative charges become nearly equal.

The United States Department of Agriculture publishes, in *Bulletin* No. 6 of the Department of Vegetable Pathology, a detailed paper, by Mr. D. G. Fairchild, on the use of "Bordeaux Mixture," a preparation of copper sulphate and lime, as a fungicide; and the mode of treatment of a number of diseases of fruit-trees, corn-crops, and garden plants caused by fungi.

The ninth edition has just been issued of part 1 of the *London Catalogue of British Plants*, comprising the Phanerogamia, Filices, Equisetaceæ, Selaginellaceæ, Marsileaceæ, and Characeæ. The changes introduced in this edition represent the results of the field-work, the critical study of British plants, and the researches on nomenclature, made during the last nine years. It is now edited by Mr. F. J. Hanbury.

The part of the *Agricultural Gazette of New South Wales* for January 1895 is chiefly occupied by papers on practical agriculture and breeding. Four species of so-called mahoganies of New South Wales are described by Mr. J. H. Maiden, all species of *Eucalyptus*. The life-history of the *Phylloxera vastatrix*, and the injuries inflicted by it on the vine, are described at length by Mr. J. A. Despeissis, and are illustrated by a coloured plate and numerous woodcuts.

With the title *Allgemeine botanische Zeitschrift für Systematik, Floristik, und Pflanzengeographie*, a new monthly botanical journal has been started at Carlsruhe, under the editorship of Herr A. Kneucker. Its aim is especially to deal with the study of difficult groups of plants, diagnoses of species, critical forms and hybrids, geographical botany, and the results of the travels of botanists.

In the *Bulletin* No. 9 of the *Minnesota Botanical Studies* is an interesting article by Mr. A. P. Anderson, on the Grand Period of Growth in the Fruit of *Cucurbita pepo*. From the time of fertilisation to that of ripening, the development may be divided into three periods—a period of active and continuous increase

from the time of pollination to the grand maximum; one of decline in the daily increase and rise in the daily decrease from the grand maximum to the beginning of ripening; and the ripening period. During this latter period an extended decrease, due to transpiration, lasting throughout the daily hours, was quickly followed by the maximum increase. At the time of the grand maximum the fruit gained 782 grammes in weight during twenty-four hours. The variations in length of the internodes occurred simultaneously with corresponding increase and decrease in the weight of the fruit.

THE Natural History Museum acquired last year some very remarkable corals, the largest weighing as much as fifteen hundred pounds. Two of these specimens have furnished Prof. Jeffrey Bell with subject for a note "On the variations observed in large Masses of *Turbinaria*," in the April *Journal* of the Royal Microscopical Society. The note is accompanied by two plates reproduced from photographs, and the point to which it directs attention is the considerable difference in size and form of the calicles in different portions of the same mass of coral. The plates show totally distinct forms comparatively close to one another, though the large mass, of which they represent parts, may be taken to be formed by a single species—*Turbinaria mesenterina*. The variability may, Prof. Bell points out, partly account for the difficulty which all students of corals have in determining specimens of the genus *Turbinaria*.

A YEAR ago the Board of Trinity College, Dublin, deposited in the Dublin Science and Art Museum the collection of weapons, &c., chiefly from the South Sea Islands, in their possession. A catalogue of the collection has now been prepared and published, with an introduction by Dr. V. Ball, the Director of the Museum. The collection has been known by common tradition as the "Cook Collection"; but a careful search has failed to bring to light direct evidence that the objects were really sent home by Captain Cook, though some of them are identical with objects figured in "Cook's Voyages." There is little doubt, however, about the reality of the association of the objects with the voyage, for the Minutes of the Board of Trinity College record that they were presented to the College in 1777 by Dr. Patten, who has been identified as the surgeon of the *Resolution* during Cook's second voyage. Part of the collection appears to have reached the College through the relatives of Captain King, who brought home the *Resolution* and *Discovery* after Captain Cook had been murdered. A brief statement as to other museums where collections of Cook's weapons are preserved, is given by Dr. Ball in the introduction to the catalogue. It is stated that in Great Britain the British Museum collection is the best in the world. Next to it in importance, in England, comes the collection in the Pitt-Rivers Museum. The Hunterian Museum in Glasgow University also contains some specimens, but how many is uncertain. So far as Dr. Ball has been able to ascertain, the museums on the continent which possess Cook collections are, arranged alphabetically, at Berne, Florence, Göttingen, Lausanne, Munich, Stockholm, and Vienna.

MESSRS. WILLIAM WESLEY AND SON have issued a very full catalogue of works on geology, offered for sale by them. The catalogue contains classified titles of more than two thousand different volumes, memoirs, and separate papers of interest to geologists. R. Friedländer and Söhn, Berlin, have sent us Nos. 1-5 of this year's *Naturae Novitates*. Bibliographers well know that the lists form a good index to current scientific literature. We have also received a catalogue, from Felix L. Dames, Berlin, containing titles of works on the invertebrates.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. Julius Scovell; a Pig-tailed Monkey (*Macacus nemestrinus*, ♀) from Sumatra, presented by Mr. D. Orville B. Dawson; three Maholi Galagos (*Galago maholi*) from South Africa, presented by Miss Van Beren; a Crowned Hawk Eagle (*Spizaetus coronatus*) from South Africa, presented by Dr. Schinland; an Antipodes Island Parrakeet (*Cyanorhamphus unicolor*) from Antipodes Island, New Zealand, presented by Sir Walter L. Buller; a Leopard Tortoise (*Testudo pardalis*), a Cape Viper (*Causus rhombeatus*) from South Africa, presented by Mr. J. E. Matcham; three Green Lizards (*Lacerta viridis*) from Jersey, presented by Masters J. S. and A. H. Hills; a Common Viper (*Vipera berus*) from Hampshire, presented by Mrs. P. C. Mitchell; two Angora Goats (*Capra hircus*, var.), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SATURN'S RINGS.—In a recent communication to the Royal Astronomical Society, Prof. Barnard states that his measurements of the rings of Saturn show that no changes have taken place since the first systematic measures were made, and that there is no ground for the supposition that the rings are closing in upon the planet.

SEARCH EPHEMERIS FOR COMET 1884 II.—Dr. Berberich gives the following search ephemeris for Barnard's periodic comet of 1884 (*Ast. Nach.* 3260):

	R. A. h.	m.	s.	Decl. °	′
May 2	22	5	36	−18	24
10		35	16	15	38
18	23	3	54	12	40
26		31	20	9	35
June 3		57	26	6	27
11	0	22	7	3	22
19		45	20	−0	24
27	1	6	59	+2	25

The positions are for Berlin midnight, and the probable error amounts to 20m. in R.A. and 3° in decl. The comet passes from Aquarius to Cetus early in June, and remains in that constellation throughout the month. It must be looked for before sunrise.

THE HAMBURG OBSERVATORY.—From the report of the Hamburg Municipal Observatory we learn that the chief astronomical researches during 1894 had to do with the movements of comets and minor planets, and with the changes in variable stars of long period. Two memoirs of some importance have also been published (*Mitt. der Hamburger Sternwarte*, Nos. 1 and 2, 1895). The first of these is a catalogue of the positions of 105 nebulæ and star-clusters, reduced from observations made in the period 1871–1880, by Dr. Pechüle and the present director, Prof. G. Rümker. The positions have been deduced from micrometric measures in relation to known comparison stars, and are tabulated for the epoch 1875. Comparisons are made with the results of other observers, and, considering the difficulties attending the observations, there is a good all-round accordance of results; but it seems yet too early to expect much information with regard to proper motions. The second memoir is an investigation by Dr. Carl Stechert of the orbit of the minor planet Tycho (258) and of the perturbations produced by Jupiter and Saturn. It is shown that the probable apparent semi-diameter of the planet at opposition is about 0″·05, the true semi-diameter being something between 50 and 80 kilometres. An ephemeris is given for observations during the opposition of June 20, 1895.

THE LATE M. TROUVELOT.—By the death of M. L. Trouvelot, on April 22, observational astronomy has lost one of its foremost workers. M. Trouvelot was born at Guyencourt, in 1827, and after the *coup d'état*, he went to Cambridge, U.S.A., where he lived until 1882. His first published works, which appeared in 1866, were on natural history subjects; later he became an astronomer at the Harvard College Observatory, and commenced the observations of the sun and planets which have made his name known to all students of celestial science. Shortly after the Meudon Observatory was founded, he returned to France, and has since then carried on his work in it. Trouvelot's important observations of the planet Venus, published in 1892, are still fresh in the minds of astronomers. He also paid attention to the planets Jupiter and Saturn. His beautiful drawings of celestial objects and phenomena observed by him are to be found in many works on astronomy.

THE SUN'S PLACE IN NATURE.[1]

V.

AT the end of the last lecture we arrived at that point of our inquiry which is connected with the possible first stage of all cosmical bodies, and we saw that there was a considerable amount of evidence in favour of the idea that in this first stage all cosmical bodies are not masses of hot gas, but that their temperature is low.

Continuing this inquiry in the light of the suggestion that the first stage might be connected with swarms of meteorites, we found the great probability that, in swarms or streams of meteorites, or meteoritic dust, we had to deal with the real basis of all cosmical bodies.

Now, if we take the heavens as we find them, whether we deal with stars, secondary bodies, or satellites, we find that they are all in movement, and it therefore follows that in these earliest stages with which we have now to deal, whether they were meteoritic swarms or streams, they were also in movement. I have already taken an opportunity of pointing out to you how very important these considerations are when we come to inquire into the constitution of each nebula. I showed you in the last lecture a beautiful photograph (Fig. 7, vol. li. p. 397), taken by Dr. Roberts, of the spiral nebula in one of our northern constellations, and I now propose to show you another very similar to it, in order, if I can, to bring more closely before you certain of the facts which were then indicated. In this wonderful photograph of the nebula in Andromeda we are undoubtedly dealing with streams, and the movements towards the centre are all along spirals. In the case of the other nebula we were in a better condition for observing the actual direction of motion because we were looking down on the system, we got a section in the plane of movement; but we are looking at this nebula in an inclined direction, though I think you will still have no difficulty in seeing that the various streams round the centre of condensation are all of them of a spiral form, with certain condensations interspersed here and there along them.

Fig. 2.—The Great Nebula in Andromeda, from a photograph by Dr. Roberts.

We have a condensation in the prolongation of one of the spirals, and there is considerable clustering of apparent stars along those lines, which I ventured in my last lecture to call stream lines, for the reason that I was anxious to indicate that we had in these appearances, not signs which told us of the existence of matter—so that when you have not the appearances you would be justified in supposing that there was no matter—but an indication of movement in matter, so that we may imagine that this nebula and others like it do probably consist of a swarm of meteorites, extending enormously in space beyond the indications which you see, for the reason that towards the centre the movements will be more violent than they are towards the outside. We are there face to face with the idea that we have to deal with orderly movements of meteoritic masses. Now, let me call your attention to this fact. If the movements are orderly, it means that the movements of the constituent particles of the swarm, all of them, or most of them, will be in the same direction: that in that case you have the condition of minimum disturbance, and therefore the condition of minimum temperature.

We can pass from such a nebula as this to the well-known planetary nebulæ. Almost all the knowledge which we have of these nebulæ we owe to the labours of Sir William and Sir John Herschel. You will see that so far as appearance goes, we have in these planetary nebulæ almost to deal with a planet like Jupiter, except that we do not see the belts. That is why these bodies are called planetary nebulæ; they give us the idea that we are dealing with discs. If we pass for a moment from a nebula which is simply discoidal to one such as is represented in another part of the diagram, you find there that we get a very faint disc, including much brighter condensation at the centre. If you look at the others, you will find that we get a very obvious condensation towards the centre; there is a very considerable difference in the intensity of the light given out as the centre is approached.

Of course we understand that if in these, also, the movements are quite orderly, we must not expect to get any very great disturbance, and therefore—if these disturbances produce high temperatures—we shall not expect to get indications of any particularly high temperatures from their external portions.

Dealing with nebulæ, then, as a whole, it does not seem too much to say that we are justified in supposing that they may advance towards condensation along two perfectly distinct roads. If we consider a regular spiral nebula, like the one in Andromeda, or a planetary nebula, we may imagine them living their life as nebulæ without very much disturbance; there is not much fighting to be done, they progress in orderly fashion towards the condition of complete condensation at the centre.

But there is another way.

In the nebula of Orion we get absolute absence of anything like regularity. In any part where the structure can be studied, we find it consists of whirls and streams crossing each other, some of them straight, some of them curved, the whole thing an irregular complicated mixture of divergent movements, so far as the photographs, which are absolutely untouched, can give us any idea of what is going on. Take, for instance, the magnificent streamer trending upwards. It gradually becomes brighter until it reaches one of the brightest parts of the nebula; and observe, also, the stars which seem dotted over it as on a shield. It is quite obvious that we cannot, in such a structure as that, expect to get the same conditions that we met with in the nebula of Andromeda, and in the planetary nebulæ. And, in fact, we do not. In this nebula, which speaks of disturbance in every inch of it, we have spectroscopic indications of very high temperature indeed. Carbon is replaced by hydrogen. In such a nebula as this, it is impossible for us to pick out the place of condensation; the condensation may be held to be anywhere, for disturbances are obviously everywhere. And you remember, I hope, that I pointed out to you that the part of

[1] Revised from the [illegible] of [illegible] Lectures to Working Men at the Museum of Practical Geology during November and December 1894. (Continued from [illegible].)

the nebula ordinarily seen is but the brightest part of a nebula extending over a space in the surrounding neighbourhood, which recent research shows is scarcely limited to the whole constellation.

Now, it so happens that the spectrum of the nebula of Orion has recently been very carefully studied from the point of view of the chemical substances which may be building up this special spectroscopic type. Here is a photograph of a part of the spectrum of the nebula of Orion; and I may tell you that it is a very difficult thing to obtain a photograph of such a very feeble light source. It is a copy of a photograph which was exposed for something like three hours at the focus of a 30-inch mirror of short focus; and in copying it we lose a great deal of the detail, a great many of the lines which are recorded by Dame Nature herself in what we call the negative. The negative

FIG. 23.—The Great Nebula in Orion, from a long exposure photograph by Dr. Roberts.

contains something like fifty lines, which have already been measured; but in the attempt to enlarge, a great many of these have been left behind.

You will see, however, without any difficulty, that the spectrum shows many bright lines; that being so, an attempt has been made to determine the positions of all of them. The result is really extremely interesting. We find, in fact, that there may in all probability be three perfectly different sources of the bright lines which, taken together, build up the so-called spectrum of the nebula. In the first place, I showed you that when we experiment with meteoritic dust in our laboratories, it has not been subjected to a low pressure very long before it begins to give out certain compounds of carbon, mingled with hydrogen gas, and we find that in the nebula of Orion we really do get indications of gaseous compounds of carbon, and also of the gas hydrogen. In order to make the distinction perfectly clear between the two other possible sources of nebula lines, let me ask you for one moment to conceive yourselves in the middle of the gigantic battle which is going on between meteoritic particles in such a nebula as that of Orion. You have particles rushing together in all possible directions, particles, no doubt, different in origin. You will expect, among those millions and billions and trillions of collisions, to get a very considerable number of grazes; and the whole point of collisions among physical particles is that, if two things go straight at each other, you get what you may call an end-on collision, which may be bad for one or both of the bodies concerned; physically we may say the temperature under these circumstances is at a maximum. But you will understand that the number of grazes, or near misses, must be very much greater than the number of end-on collisions; in such a case as we are imagining, there will be an immense number of grazes. What will a graze do? It is simply a slight collision; the amount of temperature developed by it will be small; we shall therefore get the production of vapours at a low temperature, and if we get any luminous effect at all, it will be one proper to the vapours at low temperature. So that on first principles we should expect in such a nebula as the one we are discussing to get a very large number of grazes, giving us low temperature effects, and a very much smaller number of end-on collisions, giving us very high temperature effects. Now, what are the facts? We say the most numerous collisions are partial ones, grazes. Well, there is the fluting, most probably due to magnesium at λ500, and that fluting of magnesium is the lowest temperature indication of the existence of magnesium; if magnesium becomes luminous at all by virtue of its temperature, one of the first things revealed to us spectroscopically is the particular fluting in question. We may also note the longest lines seen in the oxy-hydrogen flame of iron, calcium and magnesium as well. Those lines we are justified in considering as indications of an enormous number of grazes among these meteoritic particles. But that is not all. Going further, we find that there will be a small number of end-on collisions giving us the highest possible temperature. Being students of science, we are of course anxious to know what conditions are present in a case of that kind; that is, we want to know what the possible results of the highest temperature will be. The natural thing, I think, is to go to the sun, which is pretty hot, and then find out the very hottest place, which we can do by means of our spectroscopes, and then study very carefully, for years even, the spectroscopic indications in that particularly hottest place of the nearest star which we can get at. I hope you will acknowledge that that is a philosophic way of going to work. Thus we are landed in what is called the chromosphere of the sun. The upper atmosphere of the sun must be rapidly cooling, but the chromosphere is a thin envelope some 5000 or 10,000 miles thick, just outside the photosphere, agreed to be the hottest part of the sun within our ken, and therefore any lines which we see special to that region are called chromospheric lines, and they should be proper to high temperatures.

The chromospheric line D_3 represents a line near the sodium line D in the solar spectrum, which with a few others has the proud pre-eminence of nearly always being bright: hence we suppose that we have something hotter than anything else which exists at the exterior level of the solar photosphere. Running in couples with this line D_3 there is another in the blue part of the spectrum, represented by a certain wave-length (4471) which behaves always in the same way, *i.e.* it is almost always seen very bright, and it is never seen dark among the Fraunhofer lines in the solar spectrum. From the solar point of view then, as the sun is a thing that we can get at better than any of the other stars, because it is so near to us, a mere trifle of 90 millions of miles or so, we are justified in saying that these two lines represent, in fact, the spectrum of the hottest part of space about which we can be absolutely certain. Hence it is very interesting to inquire whether or not these two lines exist in the nebulæ as representing the high temperature results of end-on collisions.

They do exist in nebulæ, and in some of them they are among the most striking indications in the spectrum.

So that we find in the spectrum of the nebula of Orion, when it is carefully studied, indications of the gases which are known to be occluded in meteorites, and which are perfectly prepared to come out of them the moment you give them the least

chance. Then, also, there is the indication of the results of an infinitely great number of grazes in the shape of lines of metals which we see at the temperature of the oxy-hydrogen flame, but which we do not see so well at the temperature of the arc and the spark; and, on the other hand, there are indications of the results of high temperature which we can study in the sun, and such obvious indications of high temperature that we get the two lines which I have referred to, neither of which has ever been seen so far in any terrestrial laboratory, although they are very familiar indeed to students of solar physics.

The total result of all this inquiry has been that the mean temperature of the meteoritic phenomena brought before us by the nebula of Orion is distinctly low. That is a result of extreme interest and importance, because, remembering what was said about the objection to Laplace's view of high temperature gas because it violated the laws of thermodynamics, we have now, after minute study, come to a conclusion regarding the structure of these nebulæ, which is quite in harmony with the laws of thermodynamics.

When the series of lines associated with high temperatures was first recorded in the spectrum of the nebulæ, I stated that possibly this might be due to the fact that in regions of space where the pressure always operative is extremely low, we might be in the presence of chemical forms which are unfamiliar to us here, because all that we know of here chemically is the result probably of considerable temperature, and not very low pressure. It was therefore supposed that these lines might represent to us the action of unfamiliar conditions in space. Thus, if we have a compound chemical substance, and increase its temperature sufficiently, the thing goes to pieces—is dissociated; but imagine a condition of things in which we have that same chemical substance for a long time exposed to the lowest possible pressure. Is it possible that that substance will ever get pulled to bits? If so, we may imagine parts of space which will contain these substances pulled to bits which really constitute finer forms of matter than our chemical substances. So that we may logically expect to get the finest possible molecules as distinct entities in the regions where the pressure is the lowest possible. These forms are, of course, those we should expect to be produced by a very high temperature brought on by end-on collisions; hence the line of thought is not greatly changed in both these explanations, and I think that probably future research may show that we are justified in looking to both of these possible causes as those which produce for us those so-called "chromospheric lines" which we find in the spectrum of the nebulæ.

However that may be, we have arrived finally at the conclusion that the temperature of these nebulæ is low on the meteoritic hypothesis.

I have already referred in my previous lectures to Dr. Huggins's views connected with the nebulæ and stars, and you will therefore quite understand that I am delighted to find that Dr. Huggins has now come to the conclusion that in nebulæ we have distinctly a relatively low temperature. In 1889 Mr. Huggins wrote:[1] "They [the nebulæ] consist probably of gas at a high temperature," but in the address of 1891, to which I have already had occasion to refer, he gives this view up, and refers to "the much lower mean temperature of the gaseous mass *which we should expect at so early a stage of condensation*!"[2]

I am also glad to say that Dr. Keeler is also perfectly prepared to accept the view I have been insisting on. So that, if the opinion of astronomers of repute is worth anything, we do seem to have arrived at very solid ground indeed on this point, so far as consensus of opinion can make any ground solid.

J. Norman Lockyer.

(*To be continued.*)

THE RARER METALS AND THEIR ALLOYS.[3]

THE study of metals possesses an irresistible charm for us, quite apart from its vast national importance. How many of us made our first scientific experiment by watching the melting of lead, little thinking that we should hardly have done a bad life's work if the experiment had been our last, provided we had only understood its full significance. How few of us forget that we wistfully observed at an early age the melting in an ordinary fire of some metallic toy of our childhood; and the experiment has, like the "Flat iron for a farthing," in Mrs. Ewing's charming story, taken a prominent place in literature which claims to be written for children. Hans Andersen's fairy tale, for instance, the "History of a Tin Soldier," has been read by children of all ages and of most nations. The romantic incidents of the soldier's eventful career need not be dwelt upon; but I may remind you that at its end he perished in the flames of an ordinary fire, and all that could subsequently be found of him was a small heart-shaped mass. There is no reason to doubt the perfect accuracy of the story recorded by Andersen, who at least knew the facts, though his statement is made in popular language. No analysis is given of the tin soldier; in a fairy tale it would have been out of place, but the latest stage of his evolution is described, and the record is sufficient to enable us to form the opinion that he was composed of both tin and lead, certain alloys of which metals will burn to ashes like tinder. His uniform was doubtless richly ornamented with gold lace. Some small amount of one of the rarer metals had probably—for on this point the history is silent—found its way into his constitution, and by uniting with the gold, formed the heart-shaped mass which the fire would not melt, as its temperature could not have exceeded 1000°; for we are told that the golden rose, worn by the *artiste* who shared the soldier's fate, was also found unmelted. The main point is, however, that the presence of one of the rarer metals must have endued the soldier with his singular endurance, and in the end left an incorruptible record of him.

This has been taken as the starting-point of the lecture, because we shall see that the ordinary metals so often owe remarkable qualities to the presence of a rarer metal which fits them for special work.

This early love of metals is implanted in us as part of our "unsquandered heritage of sentiments and ideals which has come down to us from other ages," and future generations of children will know far more than we did; for the attempt will be made to teach them that even psychology is a branch of molecular physics, and they will therefore see far more in the melted toy than a shapeless mass of tin and lead. It is really not an inert thing; for some time after it was newly cast, it was the scene of intense molecular activity. It probably is never molecularly quiescent, and a slight elevation of temperature will excite in it rapid atomic movement anew. The nature of such movement I have indicated on previous occasions when, as now, I have tried to interest you in certain properties of metals and alloys.

This evening I appeal incidentally to higher feelings than interest, by bringing before you certain phases in the life-history of metals which may lead you to a generous appreciation of the many excellent qualities they possess.

Metals have been sadly misunderstood. In the belief that animate beings are more interesting, experimenters have neglected metals, while no form of matter in which life can be recognised is too humble to receive encouragement. Thus it happens that bacteria, with repulsive attributes and criminal instincts, are petted and watched with solicitude, and comprehensive schemes are submitted to the Royal Society for their development, culture, and even for their "education,"[1] which may, it is true, ultimately make them useful metallurgical agents, as certain micro-organisms have already proved their ability to produce arseniuretted hydrogen from oxide of arsenic.[2]

It will not be difficult to show that methods which have proved so fruitful in results when applied to the study of living things, are singularly applicable to metals and alloys, which really present close analogies to living organisms. This must be a new view to many, and it may be said, "it is well-known that uneducated races tend to personify or animate external nature," and you may think it strange that the attempt should be made to trace analogies which must appear to be remote, between moving organisms and inert alloys, but "the greater the number of attributes that attach to anything, the more real that thing is."[3] Many of the less known metals are very real to me, and I want them to be so to you; listen to me, then, as speaking for my silent metallic friends, while I try to secure for them your sympathy and esteem.

First, as regards their origin and early history. I fully

1 [illegible]

2 In this portion of the passage, the italics and notes of exclamation are mine.—J. N. L.

3 A Friday evening discourse, delivered at the Royal Institution on March [illegible], by Prof. Roberts-Austen, C.B., F.R.S.

1 Dr. Percy Frankland specially refers to the "education" of bacilli for adapting them to altered conditions. *Roy. Soc. Proc.*, vol. lvi., 1894, p. 539.

2 Dr. Brauner. *Chem. News*, Feb. 15, 1895. P. 79.

3 Lotze, "Metaphysic," § 1, quoted by Illingworth, "Personality, Human and Divine." Bampton Lectures, 1894, p. 43.

share Mr. Lockyer's belief as to their origin, and think that a future generation will speak of the evolution of metals as we now do of that of animals, and that observers will naturally turn to the sun as the field in which this evolution can best be studied.

To the alchemists metals were very living indeed; they treated them as if they were, and had an elaborate pharmacopœia of "medicines" which they freely administered to metals in the hope of perfecting their constitution. If the alchemists constantly draw parallels between living things and metals, it is not because they were ignorant, but because they recognised in metals the possession of attributes which closely resemble those of organisms. "The first alchemists were gnostics, and the old beliefs of Egypt blended with those of Chaldea in the second and third centuries. The old metals of the Egyptians represented men, and this is probably the origin of the *homunculu* of the middle ages, the notion of the creative power of metals and that of life being confounded in the same symbol."[1]

Thus Albertus Magnus traces the influence of congenital defects in the generation of metals and of animals, and Basil Valentine symbolises the loss of metalline character, which we now know is due to oxidation, to the escape from the metal of an indestructible spirit which flies away and becomes a soul. On the other hand, the "reduction" of metals from their oxides was supposed to give the metals a new existence. A poem[2] of the thirteenth century well embodies this belief in the analogies between men and metals, in the quaint lines:—

> "Homs ont l'estre comme metaulx,
> Vie et augment des vegetaulx,
> Instinct et sens comme les bruts,
> Esprit comme ange en attributes."

"Men have being" constitution like metals: you see how closely metals and life were connected in the minds of the alchemists.

"Who said these old renowns, dead long ago, could make me forget the living world?" are words which Browning places in the lips of Paracelsus, and we metallurgists are not likely to forget the living world; we borrow its definitions, and apply them to our metals. Thus nobility in metals as in men, means freedom from liability to tarnish, and we know that the rarer metals, like the rarer virtues, have singular power in enduing their more ordinary associates with firmness, elasticity, strength, and endurance. On the other hand, some of the less known metals appear to be mere "things" which do not exist for themselves, but only for the sake of other metals to which they can be united. This may, however, only seem to be the case because we as yet know so little about them. The question naturally arises, how can the analogies between organic and inorganic bodies be traced? I agree with my colleague at the École des Mines of Paris, Prof. Urbain le Verrier, in thinking that it is possible[3] to study the biology, the anatomy, and even the pathology of metals.

The anatomy of metals—that is, their structure and framework—is best examined by the aid of the microscope, but the method of autographic pyrometry, which I brought before you in a Friday evening lecture delivered in 1891, is rendering admirable service in enabling both the biology and pathology of metals to be studied, for, just as in biological and pathological phenomena vital functions and changes of tissue are accompanied by a rise or fall in temperature, so molecular changes in metals are attended with an evolution or absorption of heat. With the aid of the recording pyrometer we now "take the temperature" of a mass of metal or alloy in which molecular disturbance is suspected to lurk, as surely as a doctor does that of a patient in whom febrile symptoms are manifest.

It has, moreover, long been known that we can submit a metal or an alloy in its normal state to severe stress, record its power of endurance, and then, by allowing it to recover from fatigue, enable it to regain some, at least, of its original strength. The human analogies of metals are really very close indeed, for, as is the case with our own mental efforts, the internal molecular work which is done in metals often strengthens and invigorates them. Certain metals have a double existence, and, according to circumstances, their behaviour may be absolutely harmful or entirely beneficial.

[1] Berthelot, *Les origines des alchimie*, 1885, p. 6.
[2] *Les Remonstrances ou la complaint de nature a l'alchimist errant.* Attributed to Jehan de Meung, who with Guillaume de Lorris wrote the *Roman de la Rose.* M. Méon, the editor of the edition of 1814 of this celebrated work, doubts, however, whether the attribution of the *complaint de nature* to Meung is correct.
[3] "La Metallurgie in France," 1894, p.

The dualism we so often recognise in human life becomes allotropism in metals, and they, strangely enough, seem to be restricted to a single form of existence if they are absolutely free from contamination, for probably an absolutely pure metal cannot pass from a normal to an allotropic state. Last, it may be claimed that some metals possess attributes which are closely allied to moral qualities, for, in their relations with other elements, they often display an amount of discrimination and restraint that would do credit to sentient beings.

Close as this resemblance is, I am far from attributing consciousness to metals, as their atomic changes result from the action of external agents, while the conduct of conscious beings is not determined from without, but from within. I have, however, ventured to offer the introduction of this lecture in its present form, because any facts which lead us to reflect on the unity of plan in nature, will aid the recognition of the complexity of atomic motion in metals upon which it is needful to insist.

The foregoing remarks have special significance in relation to the influence exerted by the rarer metals on the ordinary ones. With exception of the action of carbon upon iron, probably nothing is more remarkable than the action of the rare metals on those which are more common; but their peculiar influence often involves, as we shall see, the presence of carbon in the alloy.

Which, then, are the rarer metals, and how may they be isolated? The chemist differs somewhat from the metallurgist as to the application of the word "rare." The chemist thinks of the "rarity" of a compound of a metal; the metallurgist, rather of the difficulty of isolating the metal from the state of combination in which it occurs in nature.

The chemist in speaking of the reactions of salts of the rarer metals, in view of the wide distribution of limestone and pyrolusite, would hardly think of either calcium or manganese as being among the rarer metals. The metallurgist would consider pure calcium or pure manganese to be very rare, I have only recently seen comparatively pure specimens of the latter.

The metals which, for the purposes of this lecture, may be included among the rarer metals are: (1) those of the platinum group, which occur in nature in the metallic state; and (2) certain metals which in nature are usually found as oxides or in an oxidised form of some kind, and these are chromium, manganese, vanadium, tungsten, titanium, zirconium, uranium, molybdenum (which occurs, however, as sulphide). Incidental reference will be made to nickel and cobalt.

Of the rare metals of the platinum group I propose to say but little; we are indebted for a magnificent display of them in the library to my friends Messrs. George and Edward Matthey and to Mr. Sellon, all members of a great firm of metallurgists. You should specially look at the splendid mass of palladium, extracted from native gold of the value of £2,500,000, at the melted and rolled iridium, and at the masses of osmium and rhodium. No other nation in the world could show such specimens as these, and we are justly proud of them.

These metals are so interesting and precious in themselves, that I hope you will not think I am taking a sordid view of them by saying that the contents of the case exhibited in the library are certainly not worth less than ten thousand pounds.

As regards the rarer metals which are associated with oxygen, the problem is to remove the oxygen, and this is usually effected either by affording the oxygen an opportunity for uniting with another metal, or by reducing the oxide of the rare metal by carbon, aided by the tearing effect of an electric current. In this crucible there is an intimate mixture, in atomic proportions, of oxide of chromium and finely divided metallic aluminium. The thermo-junction (A, Fig. 1) of the pyrometer which formed the subject of my last Friday evening lecture here, is placed within the crucible, B, and the spot of light, C, from the galvanometer, D, with which it is connected, indicates on the screen that the temperature is gradually rising. You will observe that as soon as the point marked 1010° is reached, energetic action takes place: the temperature suddenly rising above the melting-point of platinum, melts the thermo-junction, and the spot of light swings violently; but if the crucible be broken open, you will see that a mass of metallic chromium has been liberated.

The use of alkaline metals in separating oxygen from other metals is well known. I cannot enter into its history here, beyond saying that if I were to do so, frequent references to

the honoured names of Berzelius, Wohler, and Winkler would be demanded.[1]

Mr. Vautin has recently shown that granulated aluminium may readily be prepared, and that it renders great service when employed as a reducing agent. He has lent me many specimens of rarer metals which have been reduced to the metallic state by the aid of this finely-granulated aluminium; and I am indebted to his assistant, Mr. Picard, who was lately one of my own students at the Royal School of Mines, for aid in the preparation of certain other specimens which have been isolated in my laboratory at the Mint.

The experiment you have just seen enables me to justify a statement I made respecting the discriminating action which certain metals appear to exert. The relation of aluminium to other metals is very singular. When, for instance, a small quantity of aluminium is present in cast-iron, it protects the silicon, manganese, and carbon from oxidation.[2] The presence of silicon in aluminium greatly adds to the brilliancy with which aluminium itself oxidises and burns.[3] It is also asserted that aluminium, even in small quantity, exerts a powerful protective action against the oxidation of the silver-zinc alloy which is the result of the desilverisation of lead by zinc.

Moreover, heat aluminium in mass to redness in air, where oxygen may be had freely, and a film of oxide which is formed will protect the mass from further oxidation. On the other hand, if finely divided aluminium finds itself in the presence of an oxide of a rare metal, at an elevated temperature, it at once acts with energy and promptitude, and releases the rare metal from the bondage of oxidation. I trust, therefore, you will consider my claim that a metal may possess moral attributes has

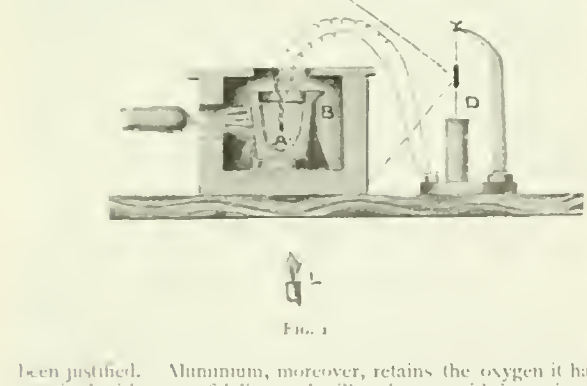


FIG. 1

been justified. Aluminium, moreover, retains the oxygen it has acquired with great fidelity, and will only part with it again at very high temperatures, under the influence of the electric arc in the presence of carbon.

[A suitable mixture of red lead and aluminium was placed in a small crucible heated in a wind furnace, and in two minutes an explosion announced the termination of the experiment. The crucible was shattered to fragments.]

The aluminium loudly protests, as it were, against being entrusted with such an easy task, as the heat engendered by its oxidation had not to be used in melting a difficultly fusible metal like chromium, the melting point of which is higher than that of platinum.

It is admitted that a metal will abstract oxygen from another metal if the reaction is more exothermic than that by which the oxide to be decomposed, was originally formed. The heat of formation of alumina is 391 calories, that of oxide of lead is 51 calories; so that it might be expected that metallic aluminium, at an elevated temperature, would readily reduce oxide of lead to the metallic state.

The last experiment, however, proved that the reduction of oxide of lead by aluminium is effected with explosive violence, the temperature engendered by the reduction being sufficiently high to volatilise the lead. Experiments of my own show that the explosion takes place with much disruptive power when aluminium reacts on oxide of lead *in vacuo*, and that if coarsely ground, fused litharge be substituted for red lead, the action is only accompanied by a rushing sound. The result is, therefore, much influenced by the rapidity with which the reaction can be transmitted throughout the mass. It is this kind of experiment which makes us turn with such vivid interest to the teaching of the school of St. Claire Deville, the members of which have rendered such splendid services to physics and metallurgy. They do not advocate the employment of the mechanism of molecules and atoms in dealing with chemical problems, but would simply accumulate evidence as to the physical circumstances under which chemical combination and dissociation take place, viewing these as belonging to the same class of phenomena as solidification, fusion, condensation, and evaporation. They do not even insist upon the view that matter is minutely granular, but in all cases of change of state, make calculations on the basis of work done, viewing changed "internal energy" as a quantity which should reappear when the system returns to the initial state.

A verse, of some historical interest, may appeal to them. It occurs in an old poem to which I have already referred as being connected with the *Roman de la Rose*, and it expresses nature's protest against those who attempt to imitate her works by the use of mechanical methods. The "argument" runs thus:—

> "Comme nature se complaint,
> Et dit sa douleur et son plaint,
> A ung sot souffleur sophistique,
> Qui n'use que d'art méchanique."

If the "use of mechanical art" includes the study of chemistry on the basis of the mechanics of the atoms, I may be permitted to offer the modern school the following rendering of nature's plaint:—

> "How nature sighs without restraint,
> And grieving makes her sad complaint
> Against the subtle sophistry
> Which trusts atomic theory."

An explosion such as is produced when aluminium and oxide of lead are heated in presence of each other, which suggested the reference to the old French verse, does not often occur, as in most cases the reduction of the rarer metals by aluminium is effected quietly.

Zirconium is a metal which may be so reduced. I have in this way prepared small quantities of zirconium from its oxide, and have formed a greenish alloy of extraordinary strength by the addition of $\frac{1}{10}$ per cent. of it to gold, and there are many circumstances which lead to the belief that the future of zirconium will be brilliant and useful. I have reduced vanadium and uranium from its oxide by means of aluminium as well as manganese, which is easy, and titanium, which is more difficult. Tungsten, in fine specimens, is also before you, and allusion will be made subsequently to the uses of these metals. At present I would draw your attention to some properties of titanium which are of special interest. It burns with brilliant sparks in air; and as few of us have seen titanium burn, it may be well to burn a little in this flame. [Experiment performed.] Titanium appears to be, from the recent experiments of M. Moissan, the most difficultly fusible metal known; but it has the singular property of burning in nitrogen—it presents, in fact, the only known instance of vivid combustion in nitrogen.[1]

Titanium may be readily reduced from its oxide by the aid of aluminium. Here are considerable masses, sufficiently pure for many purposes, which I have recently prepared in view of this lecture.

The other method by which the rarer metals may be isolated is that which involves the use of the electrical furnace. In this connection the name of Sir W. Siemens should not be forgotten. He described the use of the electric arc-furnace in which the carbons were arranged vertically, the lower carbon being replaced by a carbon crucible, and in 1882 he melted in such a furnace no less than ten pounds of platinum during an experiment at which I had the good fortune to assist. It may fairly be claimed that the large furnaces with a vertical carbon in which aluminium and other metals are now reduced by the combined electrolytic action and tearing temperature of the arc, are the direct outcome of the work of Siemens.

In the development of the use of the electric arc for the isolation of the rare, difficultly fusible, metals Moissan stands

[1] An interesting paper by H. F. Keller, on the reduction of oxides of metals by other metals, will be found in the *Journal* of the American Chemical Society, December 1894, p. [illegible].
[2] *Bull. Soc. Chim. Paris*, vol. xi. 1894, p. 377.
[3] "Dinglers [illegible] Metallurgie," part [illegible] p. [illegible].

[1] Lord Rayleigh has since stated that titanium does not combine with argon; and M. Guntz points out that lithium in combining with nitrogen produces incandescence.

in the front rank. He points out[1] that Deprez[2] used in 1849, the heat produced by the arc of a powerful pile; but Moissan was the first to employ the arc in such a way as to separate its heating effect from the electrolytic action it exerts. This he does by placing the poles in a horizontal position, and by reflecting their heat into a receptacle below them. He has shown, in a series of classical researches, that employing 800 amperes and 110 volts a temperature of at least 3500° may be attained, and that many metallic oxides which until recently were supposed to be irreducible may be readily made to yield the metal they contain.[3]

A support or base for the metal to be reduced is needed, and this is afforded by magnesia, which appears to be absolutely stable at the utmost temperatures of the arc. An atmosphere of hydrogen may be employed to avoid oxidation of the reduced metal, which, if it is not a volatile one, remains at the bottom of the crucible almost always associated with carbon—forming, in fact, a carbide of the metal. I want to show you the way in which the electric furnace is used, but unfortunately the reductions are usually very tedious, and it would be impossible to actually show you much if I were to attempt to reduce before you any of the rarer metals; but as the main object is to show you how the furnace is used, it may be well to *boil* some silver at a temperature of some 2500°, and subsequently to melt chromium in the furnace (Fig. 2). This furnace consists of a clay receptacle, A, lined with magnesia, B. A current of 60 amperes and 100 volts is introduced by the carbon poles, C, C′; an electro-magnet, M, is provided to deflect the arc on to the metal to be melted. [By

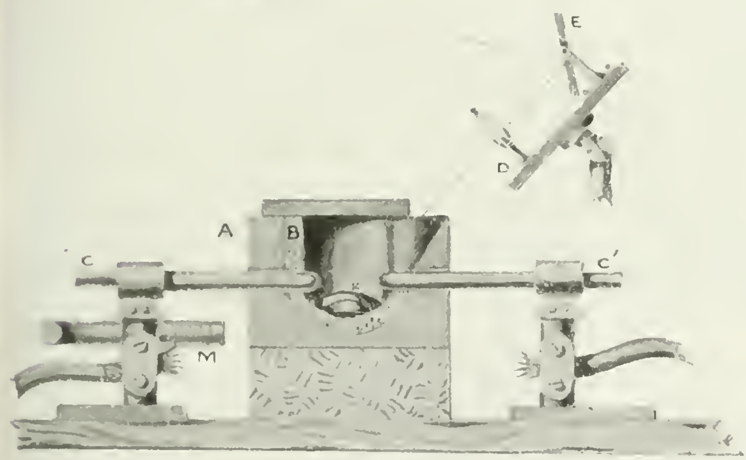


FIG. 2.

means of a lens and mirror, D, E, the image of the arc and of the molten metal was projected on to a screen. For this purpose it was found convenient to make the furnace much deeper than would ordinarily be the case.]

It must not be forgotten that the use of the electric arc between carbon poles renders it practically impossible to prepare the rare metals without associating them with carbon, often forming true carbides; but it is possible in many cases to separate the carbon by subsequent treatment. Moissan has, however, opened up a vast field of industrial work by placing at our disposal practically all the rarer infusible metals which may be reduced from oxides, and it is necessary for us now to consider how we may best enter upon our inheritance. Those members of the group which we have known long enough to appreciate are chromium and manganese, and these we have only known free from carbon for a few months. In their carburised state they have done excellent service in connection with the metallurgy of steel; and may we not hope that vanadium, molybdenum, titanium, and uranium will render still greater services? My object in this lecture is mainly to introduce you to these metals, which hitherto few of us have ever seen except as minute cabinet specimens, and we are greatly indebted to M. Moissan for sending us beautiful specimens of chromium, vanadium, uranium, zirconium, tungsten, molybdenum, and titanium. [These were exhibited.]

The question naturally arises: Why is the future of their usefulness so promising? Why are they likely to render better service than the common metals with which we have long been familiar? It must be confessed that as yet we know but little what services these metals will render when they stand alone; we have yet to obtain them in a state of purity, and have yet to study their properties, but when small quantities of any of them are associated or alloyed with other metals, there is good reason to believe that they will exert a very powerful influence. In order to explain this, I must appeal to the physical method of inquiry to which I have already referred.

It is easy to test the strength of a metal or of an alloy; it is also easy to determine its electrical resistance. If the mass stands these tests well, its suitability for certain purposes is assured: but a subtle method of investigation has been afforded by the results of a research entrusted to me by a committee of

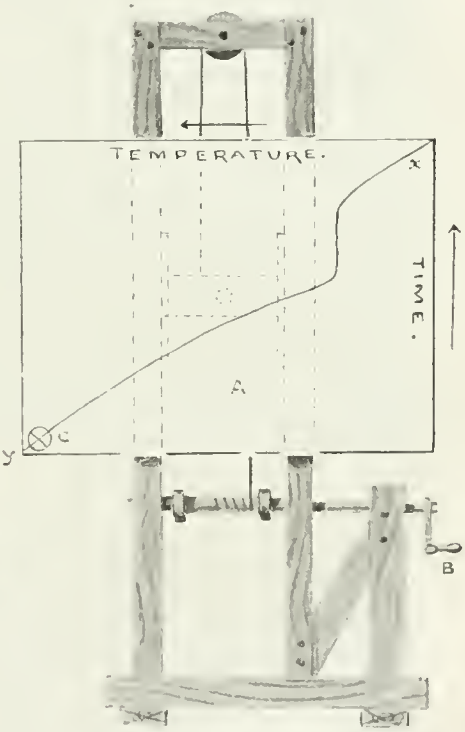


FIG. 3.

the Institution of Mechanical Engineers, over which Dr. Anderson, of Woolwich, presides. We can now gather much information as to the way in which a mass of metal has arranged itself during the cooling from a molten condition, which is the necessary step in fashioning it into a useful form; it is possible to gain insight into the way in which a molten mass of a metal or an alloy, molecularly settles itself down to its work, so to speak, and we can form conclusions as to its probable sphere of usefulness.

The method is a graphic one, such as this audience is familiar with, for Prof. Victor Horsley has shown in a masterly way that traces on smoked paper may form the record of the heart's action under the disturbing influence caused by the intrusion of a bullet into the human body. I hope to show you by similar records the effect, which though disturbing is often far from prejudicial, of the introduction of a small quantity of a foreign element into the "system" of a metal, and to justify a statement which I made earlier, as to the applicability of physiological methods of investigation to the study of metals. In order that the nature of this method may be clear, it

[1] *Ann. de Chim. et de Phys.* vol. iv. 1895, p. 305.
[2] *Comptes rendus*, vol. xxix. 1849, p. 48, 545, 712.
[3] The principal memoirs of M. Moissan will be found in the *Comptes rendus*, vol. cxv. 1892, p. 1031; *ibid.* vol. cxvi. 1893, pp. 347, 349, 549, 1222, 1225, 1429; *ibid.* vol. cxix. 1894, pp. 15, 20, 935; *ibid.* vol. cxx. 1895, p. 290. The more important of the metals he has isolated are uranium, chromium, manganese, zirconium, molybdenum, tungsten, vanadium, and titanium. There is an important paper by him on the various forms of the electric furnace in the *Ann. de Chim. et de Phys.* vol. iv. 1895, p. 365

it is considered that if a thermometer or a pyrometer, as the case may be, is plunged into a mass of water or of molten metal, the temperature will fall continuously until the water or the metal begins to become solid: the temperature will then remain constant until the whole mass is solid, when the downward course of the temperature is resumed. This little thermo-junction is plunged into a mass of gold; an electric current is, in popular language, generated, and the strength of the current is proportional to the temperature to which the thermo-junction is raised; so that the spot of light from a galvanometer to which the thermo-junction is attached enables us to measure the temperature, or, by the aid of photography, to record any thermal changes that may occur in a heated mass of metal or alloy.

It is only necessary for our purpose to use a portion of the long scale, and to make that portion of the scale movable. Let me try to trace before you the curve of the freezing of pure gold. It will be necessary to mark the position occupied by the movable spot of light at regular intervals of time during which the gold is near 1045°, that is, while the metal is becoming solid. Every time a metronome beats a second, the white screen A (Fig. 3), a sheet of paper will be raised a definite number of inches by the gearing and handle, B, and the position successively occupied by the spot of light, C, will be marked by hand.

You see that the time-temperature curve, x, x', so traced is not continuous. The freezing point of the metal is very clearly marked by the horizontal portion. If the gold is very pure the angles are sharp, if it is impure they are rounded. If the metal had fallen below its freezing point without actually becoming solid, that is, if superfusion or surfusion had occurred, then there would be, as is often the case, a dip where the freezing begins, and then the temperature curve rises suddenly.

If the metal is alloyed with large quantities of other metals, then there may be several of these freezing points, as successive groups of alloys fall out of solution. The rough diagrammatic method is not sufficiently delicate to enable me to trace the subordinate points, but they are of vital importance to the strength of the metal or alloy, and photography enables us to detect them readily.

Take the case of the tin-copper series: you will see that as a mass of tin-copper alloy cools, there are at least two distinct freezing points. At the upper one the main mass of the fluid alloy became solid; at the lower, some definite group of tin and copper atoms fall out, the position of the lower point depending upon the composition of the mass.

(To be continued.)

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE ordinary spring meeting of the Institution of Mechanical Engineers was held on Wednesday and Friday evening of last week, April 24 and 26, the President, Prof. Alexander B. W. Kennedy, F.R.S., occupying the chair both evenings. The following was down on the agenda of the meeting: Adjourned discussion on Captain H. Riall Sankey's paper on "Governing of Steam Engines by Throttling and by Variable Expansion"; the "Third Report to the Alloys Research Committee," by Prof. W. C. Roberts-Austen, C.B., F.R.S.; "Appendix on the Elimination of Impurities during the Process of making 'Best Selected' Copper," by Mr. Allan Gibb; "Appendix on the Pyrometric Examination of the Alloys of Copper and Tin," by Mr. Alfred Stansfield.

In the discussion on Captain Sankey's paper a number of members spoke. As a general result it may be stated that the position taken by the author in his paper was supported, viz.: that for certain purposes, governing by means of the throttle valve was to be preferred; whilst under other conditions variable expansion governors would have advantages over the other method. Captain Sankey in his contribution impartially discussed both systems, and his paper may be taken as a good model of what a memoir of the kind should be, no undue bias being shown on either side.

The report of Prof. Roberts-Austen was perhaps of even greater interest than those which have preceded it; whilst the two appendices of Messrs. Gibb and Stansfield discussed important practical details. A request had been made that the investigations of Warburg and Tegetmeier on molecular porosity, and their observations on the "Electrolysis of Glass"[1] should be repeated. It will be remembered that atoms of sodium were made to pass through glass at a temperature of 200° C. under the influence of the electric current. Lithium atoms were then made to follow along the tracks or molecular galleries left by the sodium, the lithium having a lower atomic volume and weight than the sodium. When potassium, having a higher atomic weight and volume, was substituted, it was not found possible to trace out the sodium. We are thus, the author said, confronted with a molecular porosity which can in a sense be gauged, and the mechanical influence of the volume of the atom is thus made evident. It will also be evident that there is a direct connection between the properties of a mass and the volume of its atoms. The results previously obtained were entirely confirmed and somewhat extended in the experiments the author had undertaken. The septa, or dividing partitions, in these fresh experiments, were made mostly of soda glass, of which thick bulbs were blown from barometer tube. In most of the experiments the glass was electrolysed, using mercury and an amalgam of some metal as cathode and anode respectively. The temperature was from 250 to 350° C. The electromotive force employed was 100 volts, and the current in the case of the sodium experiments averaged about one-thousandth of an ampere, and was sometimes as high as one-fiftieth of an ampere. When the glass bulbs were employed they soon became cracked, and the free passage of the current fused the glass, forming a well-rounded hole. In each experiment a safety fuse was placed in series, to stop the current in case of breakage. In experiments in which sodium amalgam had been placed in the bulb and pure mercury outside, sodium passed into the mercury to the extent of 0·03 gramme or 0·46 grain. In one experiment, which lasted eighteen hours, the amount of sodium found in the mercury was 0·0131 gramme, or 0·2022 grain. The quantity of electricity which passed through the glass was measured by the aid of an electrolytic cell placed in series, in which copper was deposited to the amount of 0·0206 gramme, or 0·3179 grain. Calculating the number of coulombs of electricity passed by means of the electrolysis of glass, the number 55 is found, and by the electrolysis of copper sulphate, 62; thus showing, as well as a rough approximate experiment could, that the passage of sodium into the mercury follows the ordinary law of electrolysis. It is doubtful whether the sodium from the amalgam actually penetrated right through the glass: but there can be no question that it replaced a considerable proportion of the sodium which the glass contained. An attempt to pass potassium through the same glass failed. Gold was then used, both in the form of amalgam and dissolved in metallic lead, but in the latter case the temperature employed was, of course, higher. No gold was found to have been transmitted through the glass; but the glass employed became coloured by gold, and minute spangles of the metal were found embedded in it. The same result was obtained when copper was used as an amalgam; and in this case minute nodules of copper were deposited below the surface of the glass, an effect which is highly suggestive in connection with the formation of mineral veins by earth currents. Sodium amalgam placed in a bulb and surrounded with mercury, but with no current, gave negative results, showing that simple diffusion did not play any important part in the results obtained. The fact that a current passes at all through glass is a proof that electrolytic action has taken place; so that, even if a metal be not actually transmitted through glass, the passage of a current indicates that sodium, potassium, or other metallic constituent of the glass, must be leaving it, and is probably replaced by one or more of the metals in the metallic bath which constitutes the anode.

The author next referred to an addition made to the recording pyrometer by means of which increased sensitiveness was obtained. The galvanometer, which affords the means of measuring the temperatures of the masses of metal or alloy under examination, may occupy one of two positions; it may either be nearer to the slit through which the ray of light falls upon the photographic plate, or it may be further away from it. It will be evident that two galvanometers may be used simultaneously, with the light from their respective mirrors playing

[1] E. Warburg, "Ueber die Elektrolyse des festen Glases," *Wiedemann's Annalen*, vol. xxi. 1884, p. 622. E. Warburg and F. Tegetmeier, "Ueber die elektrolytische Leitung des Bergkrystalls," *Wiedemann's Annalen*, vol. xxxv., 1888, page 455. F. Warburg, "Ueber eine Methode Natrium Metall in geschlossene Röhren einzuführen," *Wiedemann's Annalen*, vol. xl. 1890, page 1.

through the same slit upon the photographic plate. The further galvanometer can have a much lower resistance, and consequently greater delicacy, than the nearer one, so that, while the line photographed on the moving sensitised plate from the nearer galvanometer might represent a range of temperature of, say, 1500 degrees, the line traced by the mirror of the further galvanometer should represent only one-tenth of this. The angular deflection of the nearer mirror would not exceed the limits of the sensitised plate, while the mirror of the delicate galvanometer might traverse a far larger range. Both galvanometers would be connected "in parallel" with the same thermo-junction; and obviously any portion of the extended range which it was desirable to reflect on the sensitised plate could easily be caught by a suitable adjustment of the mirror on the further galvanometer. If, therefore, the thermo-junction is plunged into a mass of metal cooling from say an initial temperature of 1500 degrees, the whole of the cooling curve could be traced by the mirror of the less delicate galvanometer, while only the portion greatly magnified would be recorded by the mirror of the more delicate galvanometer. The first curve derived from the less delicate galvanometer would serve as a "calibration curve" for that afforded by the other galvanometer.

By means of diagrams exhibited on the walls of the theatre, a large number of cooling curves for electro-iron were shown, care being taken that the iron was exceedingly pure. The points of recalescence were well shown on these curves, which may be studied with interest in the *Transactions* of the Institution, as bearing on the question of allotropy of iron, which has already been fully discussed in a former report. The cooling-curve of an aluminium-copper alloy was also given. This was the alloy containing 6 per cent. of copper, used by Mr. Yarrow in the construction of torpedo boats for the French Government. Two freezing points were shown, one due to the main mass, and the other at a lower point due to the copper associated with the aluminium. The pyrometric examination of iron-aluminium alloys was also treated at some length, but it would be difficult to give results without reproducing the curves and the diagram shown.

One feature that may be noticed, however, was that the freezing point of iron alloyed with, say, one per cent. of aluminium, is but little lower than that of iron itself; that is to say, the melting point of nearly pure iron is only slightly lowered by a small addition of aluminium. Osmond had already shown that aluminium does not produce any considerable lowering of the freezing point of cast-iron; and the usually accepted idea that cast-iron or steel containing aluminium is very fusible, must be due to the fluidity of the metal when it is melted.

Another interesting point was that the samples of alloys used in these experiments were kept for some months before being analysed, and it was found that during this time those which contained from 40 to 60 per cent. of aluminium had spontaneously disintegrated, and had fallen to powder. The powder was not oxidised, but consisted of clean metallic grains, probably resulting from chemical changes which had gradually taken place in the solid alloy. Whether the iron and aluminium were in a state of solution or were chemically combined when molten, there can be little doubt that they are so combined in the metallic powder, as attempts to re-melt this powder have proved unsuccessful, which points to the formation of an infusible compound.

Some experiments made by Mr. Thomas Wrightson to ascertain whether the welding of iron is attended with a fall of temperature, as is the case in the regelation of ice, were next described. The welding was done by means of electricity and observations were taken by means of the pyrometer formerly described. The results have been communicated to the Royal Society, and tend to show that the welding of iron and the regelation of ice are analogous phenomena, a point of no small theoretical importance.

In his last report the author had called attention to the fact that M. André Le Chatélier had suggested that the prejudicial action of an element is due to its forming a fusible compound with the metallic mass in which it is hidden; while, on the other hand, the presence of an element which forms an infusible compound with the mass, promotes the formation of a fine grain and imparts strength. The author did not wish it to be supposed, however, that the action of the added element is due solely to its infusibility, or to its power of forming a fusible compound with a portion of the mass which contains it; for cases are numerous in which such an explanation does not apply. In this connection a suggestion made long ago by Raoult Pictet (*Comptes rendus*, vol. lxxxviii. 1879, pp. 855 and 1315), well deserved consideration. He urged that there must be a connection between the melting-points of metals and the periodic law of Mendeléeff; for he showed that for all metals there is a simple relation between their atomic weight, the amplitude of the movement of their molecules under the influence of heat, and their melting-point. Pure metals with high melting-points—such as platinum, iron, copper, and gold—are comparatively strong; and, conversely, metals with low melting-points—zinc, lead, cadmium, bismuth, and tin—are relatively weak. Metals with high melting-points must necessarily be coherent and tenacious, because much heat is required to drive their molecules apart in reducing them to the liquid mobile state in which the molecules have very small coherence; and therefore at ordinary temperatures much force must be applied to overcome the cohesion of the molecules and break the mass. Conversely, in metals with low melting-points a small elevation of temperature will overcome the molecular cohesion, and render them liquid—that is, will melt them. Such metals will be weak, the author continued, because if little heat is required to melt the metal, less force will be needed to tear it apart. Hence melting-point and tenacity are clearly connected. The absolute temperature of the melting-point of a metal must be closely connected with its atomic volume, because the former is inversely proportional to the rate at which the amplitude of the oscillations of the molecules increases with temperature; and the rate of increase of amplitude at any given temperature is obtained by multiplying the ordinary thermal coefficient of linear expansion by the cube root of the atomic volume.

Prof. Roberts-Austen here pointed out that the recent work of Dewar and Fleming (*Philosophical Magazine*, vol. xxxiv. 1892, p. 326) bears directly on this question. They employed very low temperatures, and show that at the absolute zero of temperature pure metals would probably offer no resistance to the passage of an electric current, but that the electrical resistance of alloys does not diminish so rapidly with the lowering of temperature as in the case of pure metals. Prof. Dewar (*Proceedings* of the Royal Institution, vol. xiv. part 2, 1895, p. 1) has shown, moreover, that the tenacity of pure metals and alloys is greatly increased by extreme cold—that is, by the closer approximation of their molecules; and this affords additional evidence that metals become stronger at temperatures which are further and further removed from their melting-points.

The discussion on this paper was of a somewhat brief nature, the reading of the report and the appendices, together with the carrying out of certain experiments and illustrations, taking a considerable time. Mr. Wrightson also explained at some length his welding experiments, which, as stated, have been placed before the Royal Society.

Prof. Goodman, of Leeds, gave some interesting particulars of the work upon which he has been engaged during the last two years in connection with anti-friction alloys. He had discovered that these substances must always contain a metal of high atomic volume, and there seemed to be a direct connection between the efficiency of the anti-friction of alloy and the atomic volume of one of its constituents. If the atomic volume of the alloy were small, then the friction was enormously increased, but with high atomic volume it was reduced. He had produced an anti-friction metal which would withstand a pressure of two tons to the square inch when running at 550 revolutions per minute, the temperature being 140°; that was a very remarkable result for a white metal. The alloy used had a higher atomic volume than bismuth, but he was not at liberty then to state the nature of the substance. He wished, however, to impress the necessity of absolute purity, or that if there were any impurities, they should be of high atomic volume.

Mr. Blount, in referring to the author's remarks on the electrolysis of glass, and the fact that potassium would not follow sodium and lithium, said he would be glad of an explanation why gold, which had a lower atomic volume than sodium, should not have traversed the "galleries" left in the glass by the sodium.

The summer meeting of the Institution will be held in Glasgow, commencing Tuesday, July 30.

THE ROYAL COMMISSION ON TUBERCULOSIS.

IN July 1890, a Royal Commission was appointed to inquire and report "what is the effect, if any, of food derived from tuberculous animals on human health, and if prejudicial, what are the circumstances and conditions with regard to the tuber-

culosis in the animal which produces that effect upon man. Lord Basing was the chairman, and the other commissioners were: Prof. G. T. Brown, Sir George Buchanan, Dr. G. F. Payne, and Prof. Burdon Sanderson. After the death of Lord Basing, in October last, the commission was reorganised with Sir George Buchanan as chairman. The report of this commission, upon the evidence and experimental inquiries received since the appointment of the original commission five years ago, was presented to Parliament last week. The general results of the inquiries instituted by the commissions in connection with the matter referred to them, will be found in the subjoined summary appended to the report:

"We have obtained ample evidence that food derived from tuberculous animals can produce tuberculosis in healthy animals. The proportion of animals contracting tuberculosis after experimental use of such food is different in one and another class of animals: both carnivora and herbivora are susceptible, and the proportion is high in pigs. In the absence of direct experiments on human subjects we infer that man also can acquire tuberculosis by feeding upon materials derived from tuberculous food animals. The actual amount of tuberculous disease among certain classes of food animals is so large as to afford to man frequent occasions for contracting tuberculous disease through his food. As to the proportion of tuberculosis acquired by man through his food or through other means we can form no definite opinion, but we think it probable that an appreciable part of the tuberculosis that affects man is obtained through his food. The circumstances and conditions with regard to the tuberculosis in the food animal which lead to the production of tuberculosis in man are, ultimately, the presence of active tuberculous matter in the food taken from the animal and consumed by the man in a raw or insufficiently cooked state. Tuberculous disease is observed most frequently in cattle and in swine. It is found far more frequently in cattle (full grown) than in calves, and with much greater frequency in cows kept in town cowhouses than in cattle bred for the express purpose of slaughter. Tuberculous matter is but seldom found in the meat substance of the carcase; it is principally found in the organs, membranes, and glands. There is reason to believe that tuberculous matter, when present in meat sold to the public, is more commonly due to the contamination of the surface of the meat with material derived from other diseased parts than to disease of the meat itself. The same matter is found in the milk of cows when the udder has become invaded by tuberculous disease, and seldom or never when the udder is not diseased. Tuberculous matter in milk is exceptionally active in its operation upon animals fed either with the milk or with dairy produce derived from it. No doubt the largest part of the tuberculosis which man obtains through his food is by means of milk containing tuberculous matter. The recognition of tuberculous disease during the life of an animal is not wholly unattended with difficulty. Happily, however, it can in most cases be detected with certainty in the udders of milch cows. Provided every part that is the seat of tuberculous matter be avoided and destroyed, and provided care be taken to save from contamination by such matter the actual meat substance of a tuberculous animal, a great deal of meat from animals affected by tuberculosis may be eaten without risk to the consumer. Ordinary processes of cooking applied to meat which has got contaminated on its surface are probably sufficient to destroy the harmful quality. They would not avail to render wholesome any piece of meat that contained tuberculous matter in its deeper parts. In regard to milk, we are aware of the preference by English people for drinking cows' milk raw—a practice attended by danger on account of possible contamination by pathogenic organisms. The boiling of milk, even for a moment, would probably be sufficient to remove the very dangerous quality of tuberculous milk. We note that your Majesty's gracious commands do not extend to inquiry or report on administrative procedures available for reducing the amount of tuberculous material in the food supplied by animals to man, and we have regarded such questions as being beyond our province."

THE GEOLOGICAL DEVELOPMENT OF AUSTRALIA.

BY the kindness of the Secretary of the Australasian Association for the Advancement of Science, we have been favoured with a complete account of the proceedings of the late meeting at Brisbane. The Hon. A. C. Gregory, C.M.G., the president of the meeting, took as the subject of his address "The Geographical History of the Australian Continent during its successive Phases of Geological Development." The subject afforded Mr. Gregory an opportunity for putting on record the knowledge he has gained from personal inspection of a larger proportion of Australian territory than has been explored by any other investigator. We are glad to be able to give the text of his address.

Primary Condition and Form of Land.

In dealing with the geological history of Australia, it is convenient to refer to the groups of formation, as the scope of this address is insufficient for the separate consideration of the component members of each group which has taken prominent part in the geographical establishment of sea and land. Like all histories of remote events, the evidence of what was the primary condition and form of the land is necessarily of very limited character, but some evidence does remain for our guidance. The earliest indications of the existence of land within the limits of the present Australian continent consists in the fact that many of the more elevated summits are composed of "granite," which is certainly the oldest rock formation with which we are acquainted.

It is here necessary to state that the term granite is used to indicate ancient or continental granite, and that the granitoid rocks, which are so closely allied in lithological aspect as to pass under the same designation, but are really intrusive masses of more recent date, even as late as the Permo-carboniferous period will be termed intrusive granite. Now the higher portions of the granite ranges show no superincumbent strata, while sedimentary beds fold round their flanks in a manner which indicates that the edges of these strata were formed near the margin of an ancient sea, above which the more elevated masses of granite rose as islands. As an instance of this early existence of land, we find on the present east coast that the granite tract of New England is flanked by Devonian slates and marine beds of spirifer limestones in positions which indicate that their deposition was in an ocean of at least 2000 feet in depth, above which the granite mountains rose to an elevation of 2000 feet. Adopting similar evidence as a basis for the estimation of the area of land at this earlier date, it appears that there existed a chain of islands extending from Tasmania northerly along the line of the present great dividing range, between the eastern and western streams nearly to Cape York, a distance of about 2000 miles, and with a breadth seldom exceeding 100 miles. In Western Australia a much broader area of dry land existed in the form of a granite tableland, the western limit of which, commencing at Cape Leeuwin, extended north for 600 miles, with a straight coast-line rising 500 feet to 1000 feet above the ocean. This land had a breadth east and west of about 200 miles, but its eastern shores were comparatively low and irregular, with probably detached insular portions, more especially on the northern side, as the stratified rocks in which the West Australian gold mines are worked have an exceedingly irregular outline where they overlay the granite. Between these eastern islands and the western land, there probably existed some granite peaks which rose above the ocean, but the evidence is that they were not of important area, and principally located in the northern parts. The remainder of the present continent was covered by an ocean gradually increasing in depth from the western land to the central part, and great depth continued to the shores of the eastern islands.

Sedimentary Deposits.

The next step in our history is that the natural decomposition of the granite, both terrestrial and marine, supplied material for sedimentary deposits; and we find a series of imperfectly stratified grit rocks, together with schists and slates, the former the results of the deposition of the coarser drifts, and the latter the more gradual deposit of the finer particles. These rocks, which are classed as Laurentian, Cambrian, and Silurian, did not extend far from the eastern islands, and are principally developed in Queensland to the north and in Victoria to the south, but, being of marine formation, they did not then materially affect the geographical configuration, though they are important features of the present time, and are the chief sources of our tin mines; and silver, lead, and copper also exist in sufficient quantity to afford prospect of future industrial success. There is also a marked characteristic in the abundant occurrence of fluor spar, which is an exceedingly rare mineral in the later formations.

while gold does not occur in important quantity except in its upper or Silurian strata in Victoria. Near Zilmantown (lat. 17° 20′ S., long. 144° 30′ E.) there are interesting developments of these rocks, which now form steep ranges with flat-bottomed valleys, in which coralline limestone of the Devonian period rests unconformably, and in places rises abruptly several hundred feet, presenting the form of ancient coral reefs, such as now exist on the great Barrier Reefs. In fact, they indicate that at some remote time a passage existed from the east coast to the southern part of the Gulf of Carpentaria, under similar conditions to those of the present Torres Straits, and that the subsequent elevation of the land has now placed it more than 500 feet above sea-level. This description of the present state of these rocks is, however, a digression in regard to geological sequences of the early period.

More Favourable Conditions.

The Cambrian and Silurian period was succeeded by the Devonian, during which there is little evidence of any great variation in the limits of the sea and land, but organic remains show that the conditions were becoming more favourable for the development of marine life. The rocks consist principally of fine-grained slates, which must have been deposited in a deep sea, and in some places the now visible sections indicate a thickness of 10,000 feet.

The upper strata connected with the Devonian series have been classed by geologists as belonging to the Permo-carboniferous, on account of the marine fossils which have been found in the Gympie series of rocks. Some difficulties, however, arise in regard to the identification of Australian rocks with those of Europe on the sole basis of the occurrence of nearly the same species of mollusca, and it may be remarked that in Central North America the appearance of fossil mollusca and plants, which would in Europe indicate a definite horizon, often occurs in rocks which lithologically and stratigraphically are of an earlier date; and the same conditions of the earlier appearance of species and genera seem to obtain in Australia, and if ultimately established would clear away many of the existing difficulties in the comparison of Australian and American fossils with those of Europe. Accepting the classification of the Gympie rocks as Permo-carboniferous, there was no important alteration in the geographical limits during the Devonian period, or in the earlier Permo-carboniferous Gympie beds, but shortly after this there were very decided variations in both the area and altitude of the land. The whole of the present continental area was raised sufficiently to lift large portions of the previous sea-bottom above its surface. The principal elevation was on the eastern coast, where the rise must have been several thousand feet; while on the west it was less pronounced, though the area added to the land appears to have included nearly the whole of what is now Western Australia. And in regard to the intervening space between it and the eastern ranges there is only the negative evidence, of no later marine deposits to indicate that it also was above the ocean. Although the general elevation of the continent appears to have been quiescent in the western and central parts, there were violent disruptions on the eastern coast, and the strata were apparently crushed by a force from the east which lifted them into a series of waves showing the faces of dislocation to the east and strata sloping to the west, the most easterly wave being near the present coast-line, and the succeeding waves more gradual as they recede to the west, both in angle and height, until they merge into the level of Central Australia. It is also probable that the South Australian range was also the result of this compression, causing the strata to rise in abrupt masses on an axis nearly north and south. It was at this stage of disruption and elevation of strata that the more important auriferous deposits of both the eastern and western parts of the continent were formed, and these may be divided into two classes—true fissure veins, or lodes, in which the deposits of ore are found filling fissures in the slate strata, and generally nearly vertical; and floors of ore which occur in sheets dipping at a less angle from the horizontal than the vertical, the including rock being of crystalline character, being, in fact, intrusive granites. The dip of these sheets of ore is in the direction of the huge dykes of intrusive rock in which they occur.

Auriferous Deposits in Lodes.

There was not only great disruption of the strata, but igneous rocks forced themselves into the fissures in the sedimentary beds, and the resulting metamorphism of the adjacent rocks increased the confusion, as beds of slate may be traced through the transformation of their sedimentary character, by the recrystallisation of their component elements into diorites having that peculiar structure of radiating crystals which usually characterise rocks of volcanic origin. As regards the auriferous deposits in these lodes, it appears that first simple fissures were filled with water from the ocean or deep-seated sources; but in either case the powerful electric currents which continually traverse the earth's surface east and west met resistance at the lines of disruption, and electric action being developed, the mineral and metallic salts in the water in the fissure and the adjacent rocks would be decomposed, and the constituents deposited as elements, such as gold and silver, or as compounds, such as quartz, calcspar, and sulphide of iron, all which were in course of deposit at the same time as the angles of the crystals cut into each other. There have been many speculations as to the source from which the gold was derived, but that which best accords with the actual conditions is that the metal exists in very minute quantities in the mass of the adjacent rocks, from which it has been transferred through the agency of electric currents and the solvent action of alkaline chlorides, which dissolve small quantities of the precious metals, and would be subject to decomposition at the places where fissures caused greater resistance to the electric current. One remarkable circumstance is that the character of the rocks forming the sides of the fissures has an evident influence on the richness of the ores in metals where lime, magnesia, or other alkaline compounds, or graphite, enter into their composition; the gold especially is more abundant than where the rocks contain silica and alumina only.

Queensland's Testimony.

In Queensland, Gympie affords some instructive examples of fissure lodes. In some, large masses of rock have fallen into the fissure before the ore was deposited, and have formed what miners term "horses," where the lode splits into two thin sheets to again unite below the fallen mass. The Mount Morgan mine may also be cited as a case where several fissure lodes rise to the surface in close proximity. The ore was originally an auriferous pyrites, but the sulphide of iron was largely decomposed, leaving the gold disseminated through the oxide of iron. In other cases the sulphur and iron have both been dissolved out, and left cellular quartz, with gold in the cavities or as fragments of gold, mixed with minute crystals of quartz, presenting the aspect of kaolin, for which it has been mistaken. The auriferous deposits, which occur in the intrusive granites, appear under conditions differing from the true lodes in sedimentary rocks, as the intrusive granitoid rock forms dykes which fill fissures in the older true granites, and also cut through the sedimentary slates. It bears evidence of intrusion in a state of fusion, or, at least, in plastic condition and subsequently crystallised, after which there has been shrinkage, causing cavities as the sides of the dyke were held in position by the enclosing rock. The vertical shrinkage being greater than the horizontal, the cavities were nearer the horizontal than the vertical, and being afterwards filled with ore, formed what are called "floors," one characteristic of which is the tendency to lenticular form, or a central maximum thickness with thinner edges. The Charters Towers goldfield exhibits a good illustration of this class of auriferous intrusive granite. Here the intrusive granite appears as a dyke of great thickness, exceeding a mile, with a length of twenty miles; the rock is well-crystallised quartz and felspar, with very little mica or hornblende. One shaft has been sunk 2000 feet to a floor showing gold, and similar to the floors that outcrop on the surface. The dip of these floors is north, about 30 degrees from the horizontal, and the strike across the direction of the dyke. There are, however, no good natural cross-sections, as the watercourses are small, so that the length and breadth have to be estimated to some extent by the character of the soil derived from the decomposed rock, it being more fertile than that of the other rocks in the locality. The exploratory shafts which have been sunk are in positions selected for the purpose of reaching known sheets of ore at greater depth, or under the impression that the ore deposits were true fissure lodes, and would have extension in the direction of the discovered outcrops, and therefore not calculated to extend our knowledge of the auriferous deposits. The most instructive instance of the occurrence of auriferous intrusive granite exists in the valley of the Brisbane River, near Eskdale, where a granitoid dyke, fifty yards wide, cuts through a slate hill for a distance of three miles, and in places shows thin sheets of quartz containing gold; the strike is at right angles to the length of the dyke, and the dip is 30 degrees. Some of the

quartz sheets have been traced across the dyke to within an inch of the slate which encloses it, but there is no trace of any variation in the sedimentary slate opposite the end of the quartz. A small watercourse cuts through the dyke and exposes arsenical pyrites and iron oxide, with small particles of gold. A more accessible instance of intrusive granite is exposed in the cutting for the bywash of the Brisbane Waterworks, at Enoggera, where the igneous rock has intruded between the strata of the slate.

Permo-carboniferous Rocks.

From the middle to the close of the Permo-carboniferous period the dry land teemed with vegetation, of which the Lepidodendron was a conspicuous type, along the eastern division, for though this plant was most abundant in Queensland, it is also found in Victoria, and on the Philips River, in West Australia, where the later Permo-carboniferous rocks are found on the south coast, extending from Albany eastward to Israelite Bay, forming the Stirling Range, with an elevation of 3000 feet, the Mounts Barren, and Russell Range. The age of these rocks is determined by the occurrence of large fragments of carbonised vegetation, the aspect of which closely resemble Lepidodendron stems. This formation is limited to the coast district, as, at a distance of fifty miles inland, the granitic plateau is reached with its partial colouring of Devonian slates. On the northern coast the Permo-carboniferous rocks are developed in the valley of the Victoria River for a hundred miles from the sea. Also on the Kimberley goldfield, to the south-west of Victoria.

Geographical Features.

The geographical features of this period appear to have been a continent somewhat similar in form to that of the present Australia. There was an elevated range along the east coast which attracted moisture, and a climate favourable to vegetation, and also by rapid degradation of its rocks supplied suitable soil for tropical growth. The central interior was not favoured by such a climate, and there are few traces of either deposit or denudation. The western interior enjoyed a moderate rainfall, and the detritus was carried down towards the north and south coasts, where it was deposited in regions where the carboniferous flora flourished, though not to the same degree as in East Australia, where it laid the foundation of the great coalfields of New South Wales and Queensland.

Further Elevation of Continent.

About the end of the Palæozoic or the commencement of the Mesozoic periods there appears to have been a further elevation of the continent, especially in the eastern part, for though in many places the deposits of the strata show little interruption, in others there has been considerable disturbance and unconformity of succession, with indications of an increase in the elevation of the land, which, with a contingent increase of rainfall, accounts for the luxuriant growth of the carbonaceous flora and its extension much further to the west. The artesian bores which have been made show that the cretaceous beds rest on the carbonaceous at a depth of 2000 feet below the present ocean level, and the fresh-water beds of the coal series are not less than 3000 feet in thickness, showing that the terrestrial level of the mountains has been decreased 5000 feet, or, in other words, they were 5000 feet higher during the Mesozoic period. On the western coast the elevation is not so well defined, but the land was at a greater height above the ocean than at present, as fragments of coal and its accompanying minerals have been washed up from the deep sea, and may be found embedded in the Tertiary limestones of the coast. There is thus proof that on the west coast the land extended further, and was covered with Australian fresh-water flora of the coal period; but this area is now submerged, and, taking into consideration the great depth of the ocean on this coast, the height of the land must have exceeded its present level by a thousand feet. Examining the ocean depths around the present Australian coast, even 5000 feet would make little difference in the limits of the west, south, and south-west coasts; but on the north and east the land would extend to the Great Barrier Reef. Papua would have been connected, and even the Arafura Sea and Island of Timor might have been brought within the limits of *Terra Australis*.

Elevation of Australia.

The mountain ranges of the east coast would be connected with those of Papua and form a magnificent series of summits of 13,000 feet elevation, a configuration that must have arrested the moisture from the Pacific Ocean, and resulted in a moist tropical climate, well calculated to support the luxuriant growth of the vegetation of the coal period so far as East Australia was affected, though it might also have had the effect of rendering the climate of Central and West Australia so dry as to render the land a desert during the continuance of this carbonaceous period. East Australia has thus, on its lower levels, accumulated stores of fuel for use in ages long subsequent. The luxuriant vegetation necessary to the production of coal was limited to the area east of the 140th meridian, except in a portion of South Australia, which seems to have been favoured by the overflow of some large rivers draining the western slopes of the Great Range, and had their outlet through Spencer's Gulf. The vegetation of Australia at this period, however well adapted for the formation of coal deposits, was not such as in the present would be suitable for the maintenance of mammalian life, as it consisted of ferns, cycadea, palms, and pine-trees, of which only the *Araucaria Bidwillii* has left a living representative, and its silicified wood from the coal formation presents exactly the same structure as the tree now growing on the ranges. Australian geography underwent little change during the Mesozoic period, but at the commencement of the Cretaceous a general subsidence of the whole continent began. The coal deposits ceased, and a fresh-water deposit known as the Rolling Downs formation accumulated, the constituents being soft shales, which in the earlier period supported a growth of ferns and pine timber. The land continued to subside until the ocean invaded a large portion of the lower lands, but only as a shallow sea, or possibly in the form of estuaries, as the fresh-water vegetation appears intercalated with marine limestones containing Ammonites and other mollusca of the Cretaceous epoch.

The Cretaceous Period.

The depression during the Cretaceous period must have been gradual and of long continuance. The ocean apparently first covered the land near the Great Australian Bight on the south, and Arnheim's Land on the north, as in each of these localities there are extensive deposits of thick bedded limestones, which may have continuity across the continent under cover of the ferruginous sandstones of the latter part of the epoch. On the east coast the ocean rose from 100 feet to 200 feet above its present level in Queensland, as the margin of the Cretaceous rocks is visible close to South Brisbane, and there is a belt along the coast from Point Danger to Gladstone. Further north there are extensive patches of Desert Sandstone belonging to this period, though the designation seems to have been applied to two distinct beds of sandstone, one belonging to the close of the Mesozoic, and the other to the last part of the Cretaceous.

Great Depression and Eruptions.

Ultimately the dry land was reduced to the eastern ranges, from Cape Howe northerly to lat. 15°; the eastern side nearly the same as the present coast line, and extending from 100 to 300 miles westerly, while the Mount Lofty Range in South Australia existed as an island. This great depression was accompanied by dislocations of strata and also the eruption of porphyritic masses, the age of these eruptions being easily determined as they rest on the Ipswich coal strata. At Mount Flinders the base of the mountain consists of coal shales with abundant impressions of *Pecopteris*, while there is a more instructive instance near Teviot Brook, where in a deep ravine there is a dyke of porphyry cutting through a bed of carbonaceous shale with *Pecopteris* and the silicified stems of pine-trees embedded. The dyke itself is dark-coloured and highly crystalline, but where it spreads out into a flat sheet on the top of the hill it assumes the same appearance as the light-coloured porphyry of Brisbane. This porphyry forms the Glass-house Mountains, which are so conspicuous from the entrance of Moreton Bay, and also Mounts Warning, Leslie, Maroon, and Barney.

The central and western parts of the continent were almost entirely submerged in the ocean, but not to any great depth, as the higher granite peaks of the north-west do not show traces of submergence, though the sedimentary deposits approach closely to their bases. The Stirling and Mount Barren Ranges on the south coast were only partially covered, as there is an ancient sea beach on the south side of middle Mount Barren, about 300 feet above the present sea-level. The interior tableland, though now of greater altitude than Mount Barren, was submerged, as evidenced by the extension over the whole of the rest of West Australia of soft sandstones and claystones in which salt and

gypsum are of common occurrence. On the northern coast the submergence was greater, as the sandstones and shales have a thickness of more than a thousand feet.

THE CRETACEOUS DEPOSITS.

One characteristic of the later part of the Cretaceous deposits is that in the lower part they consist chiefly of white, blue, and pale red shales, which readily disintegrate, while the upper portion consists of variegated sandstones of a harder character, with a comparatively thin covering of ferruginous concretionary pebbles or nodules, often with a nucleus of organic origin. On the west coast (latitude 29°), on Moresby's Flat-topped Range, these features are well developed, and in the upper part a bed of limestone, containing Ammonites and other mollusca of the Cretaceous series. And it was from this locality that the first proofs of the existence of the Cretaceous formation in Australia were furnished to Prof. M'Coy. Closely associated with these limestones are ferruginous sandstones, containing casts of large accumulations of fragments of wood and vegetable débris, such as may be found after floods on the margins of rivers, indicating an estuarine system, where fresh and salt water alternated.

AUSTRALIA AN ISLAND.

The Mesozoic period closed with Australia reduced to the area of a large island on the east coast and some small islands on the south-west and north-west of the present continent, and then the connection with Papua was severed

A NEW ELEVATION.

Early in the Tertiary period a new elevation of the land commenced, but the rise was not attended by any great disturbance of the strata, as in almost every instance where the Upper Cretaceous rocks remain they are remarkable for their horizontal position. The elevation of the continent on this occasion was nearly equal in all parts; the ultimate altitude was at least 500 feet greater than at present, and the geographical effect was that Australia assumed nearly its present limits.

FEATURES OF THE CONTINENT.

The features of the continent at this time appear as high ranges on the east coast and a nearly level tableland extending to the west coast, but the whole of the interior with a general incline towards Spencer's Gulf. Short watercourses flowed direct to the sea, but far the greater area was drained by much longer streams towards Spencer's Gulf, while a secondary series occupied the basin of the Murray and Darling Rivers. The climate evidently differed greatly from that now existent, as the denudations of the tableland removed tracts of country many hundreds of square miles, each forming immense valleys bounded by flat-topped hills and ranges representing the marginal remnants of the original surface. Enormous quantities of the finer-grained portions of the degraded shales must have been swept into the ocean by the rivers, but the coarser sands have been left in what is now the desert interior, where the wind drifts it into long steep ridges of bright red sand, having a northerly direction near the south coast, but spreading out like a fan to the east and west in the northern interior.

VALLEYS AND RIVER SYSTEMS.

The interior rivers formed a grand feature of the country so long as the rainfall continued sufficiently copious to maintain their flow, but in the arid climate which now obtains it does not even compensate for the evaporation. The river channels have been nearly obliterated, and some parts of the wider valleys changed to salt marshes or lakes, such as Lakes Amadeus and Torrens, while the entrance to Spencer's Gulf is choked with sand. It was during this period when the great valleys of the river systems were being excavated that a great proportion of the outbursts of volcanic rock in the form of basalt occurred. The age of these basalts is established by their superposition on cretaceous rocks. Thus, at Roma, the Grafton Range is a mass of basalt, resting on the cretaceous sandstones and shales. Mount Bindango is a similar instance. On the Upper Warrego there is a deep ravine through cretaceous rocks partly undermining a basaltic cone. On the Victoria River a large basin has been eroded in the cretaceous rocks and then several hundred square miles flooded by an eruption of basalt, through which watercourses have cut instructive sections, showing the subordinate sandstones baked and fused by contact and the cracks filled by the covering basalt.

It does not appear that the eruption of basalt has materially affected the geographical outline of the coast, but there were considerable variations of level and important tracts of fertile country formed by the basaltic detritus, such as Peak Downs and Darling Downs in Queensland, and to the west of Melbourne in the south.

LARGE ANIMAL PERIOD.

It was not till after the convulsions which attended this outflow of basalt, and lakes, marshes, and rivers had been formed, and produced a luxuriant growth of vegetation, that the gigantic marsupials gave any decisive evidence of their advent, as their fossil remains are found in the drifts of watercourses mixed with basaltic pebbles and detritus. The physical conditions of the country during the period of the Diprotodon, Nototherium, and associated fauna, differed materially from that which now subsists, for the structure of the larger quadrupeds would render them incapable of obtaining a subsistence from the short herbage now existing in the same localities, and it is evident that their food was of a large succulent growth, such as is found only in moist climates and marshy land or lake margins. This view is also supported by the fact that on the Darling Downs and Peak Downs the associated fossils include crocodile and turtle, so that what are now open grassy plains must have been lakes or swamps, into which the streams from the adjacent basaltic hills flowed, and, gradually filling the hollows with detritus, formed level plains.

ENORMOUS RAINFALLS.

That this gradual filling up of lakes actually occurred is shown by the beds of drift which are found in sinking wells and in sections exposed by erosion of watercourses; but in all these instances there is evidence that the ancient rainfall was excessive, as even our present wettest seasons are inadequate to the removal of the quantities of drift which have been the result of a single flood in the ancient period. On the ridges around the lakes there existed a forest growth, as many species of opossum have left their bones as evidence; but the timber evidently differed from the present scanty growth of eucalypti. Whether the same abundant rainfall extended far into the western interior is uncertain, but the rivers evidently maintained a luxuriant vegetation adapted to the sustenance of these gigantic animals, as the discovery of a nearly complete skeleton of Diprotodon on the shore of Lake Mulligan, in South Australia, shows that these animals lived in this locality, as it is not probable that their bodies could have floated down the Great River which drained the interior of the continent through Lake Eyre.

ANOTHER CHANGE.

It is evident that the climate gradually became drier, that the rivers nearly ceased their flow, and the lakes and marshes became dry land, while the vegetation was reduced to short grasses that no longer sufficed for the subsistence of the huge Diprotodon and gigantic kangaroo, though some of the smaller may still survive to keep company with the dingo, who, while he left the impressions of his teeth in the bones of the Diprotodon, has shown a greater facility for adapting himself to altered conditions. Is this the survival of the fittest? It was in these days that some of the rivers flowing direct to the coast cut through the sandstones into the softer shales beneath, and by their erosion formed considerable valleys bounded by rocky cliffs, and when the land was subsequently depressed the sea flowed in and formed inlets, of which Sydney Harbour and the entrance to the Hawkesbury River on the east coast, Port Darwin and Cambridge Gulf on the north-west, and the Pallinup River on the south-west of the continent may be cited as examples.

CONCLUSION.

Thus Australia, after its first appearance in the form of a group of small lands on the east, and a larger island on the west, was raised at the close of the Palæozoic period into a continent of at least double its present area, including Papua, and with a mountain range of great altitude. In the Mesozoic times, after a grand growth of vegetation which formed its coal beds, it was destined to be almost entirely submerged in the Cretaceous sea, but was again resuscitated in the Tertiary period with the geographical form it now presents. Thus its climate at the time of this last elevation maintained a magnificent system of rivers, which drained the interior into Spencer's Gulf, but the gradual decrease in rainfall has dried up these watercourses, and their channels have been nearly obliterated, and the country changed from one of great fertility to a comparatively desert interior which can only be partially reclaimed by the deep boring of artesian walls.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The preliminary resolutions in reference to the admission of graduates of other Universities to courses of advanced study and research were passed *nem. con.* by the Senate on April 25. The Syndicate for the purpose will now proceed to frame the detailed regulations for carrying the scheme into effect.

An Exhibition of fifty guineas a year for three years is offered by the Clothworkers' Company for the encouragement of Physical Science. Candidates must be non-collegiate students of one term's standing, or persons not yet in residence who propose to become non-collegiate students next October. The examination will be held next July. Information as to conditions, &c., may be obtained from the Censor, Fitzwilliam Hall, Cambridge.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, April 22.—M. Marey in the chair.—On the effects of the air carried below, without gyration, in the interior of tempests, water-spouts, and tornados, by M. H. Faye. The author shows that water-spouts are of the same type as, though on a smaller scale than, cyclones and typhoons. He illustrates by an experiment the character of the air-movements in the case of a water-spout. A gyratory movement at the base of a cloud causes the formation of a descending cone which has no effect below until the apex reaches the ground or water, when the air from above carried down in the centre of the cone escapes with violence in every direction. The phenomenon consists then of an interior comparatively calm core, down which proceeds air from the upper regions, and this is surrounded by a shell of cloud having a rapid rotatory motion. The analogy of the air-movements in cyclones and typhoons is brought out by a detailed consideration of (1) a storm encountered by the corvette *l'Eglé*. (2) a typhoon which passed centrally over Manilla Observatory on October 20, 1882. The calm column in the latter case was much hotter (11°) and drier than the surrounding shell of storm; the direction and force o the wind, temperature, and humidity were continuously registered, and completely bear out the explanation advanced.—On a new type of wells in the granitic rocks of Sweden, by M. Nordenskiold. These are artesian wells bored to a depth of from 30 to 50 metres in solid crystalline rocks in the hope of meeting with water coming through horizontal cracks expected to occur in the mass owing to the variations of temperature suffered by the surface portions. Such cracks supplying sweet water have invariably been encountered at a depth of 33 to 35 metres.—On a new deposit containing uranium, by M. Nordenskiold. A uraniferous substance giving nitrogen (see "Notes," p. 8).—Crystals forming at the bottom of a solution of greater specific gravity than themselves, by M. Lecoq de Boisbaudran. The inverse effect to that previously described by the author, where substances were shown to crystallise under some circumstances at the top of solutions of less specific gravity than the crystals. Crystals of sodium sulphate, floating on a solution of sodium iodide saturated with the sulphate, gradually disappeared, re-crystallising around a sulphate crystal previously fixed at the bottom of the solution. The same phenomenon occurs with ice in a dilute ammoniacal solution. This action depends on small temperature variations, as previously explained.—Every algebraical surface may be described by means of an articulated system, by M. G. Kœnigs.—On curves of the fourth class, by M. Georges Humbert.—On the dilatation of water, by M. Stephane de Lannoy. The author discusses the dilatometer method of taking the expansion of water, and tabulates his results with three instruments. A table is then given comparing the mean results with Rosetti's values, and with the corresponding quantities calculated from these values for the same temperatures by the air thermometer.—Specific heat and boiling point of carbon, by M. J. Violle. Above 1000° C. the mean specific heat of graphite increases linearly with the temperature, thus $C_0^t = 0.355 + 0.00006t$. 2050 calories are given up by 1 gram of graphite on cooling from the volatilisation temperature to 0°. The temperature of ebullition must therefore be 3600° C.—Electric resistance at the contact of two metals, by M. Edouard Branly. It is shown that certain pairs of metals, such as copper-zinc, have no contact resistance, whereas other pairs, lead-aluminium, lead-iron, tin-aluminium, tin-iron, bismuth-iron, bismuth-aluminium for instance, have an electric contact resistance.—On an optical method of analysing alternating currents, by M. J. Pionchon.—On photography in natural colours, by the indirect method, by MM. Auguste and Louis Lumière. Several negatives are prepared with differently coloured screens, and each is used to print off in a layer of the appropriately tinted bichromated-gelatine.—Molecular rotation and molecular deviation, by M. Ph. A. Guye.—On some derivatives of quinone-diorthoamido-benzoic acid, $C_6H_2O_2(NH.C_6H_4.CO_2H)_2$, by MM. J. Ville and Ch. Astre.—Remarks on the *pars intermedia* of Weisberg, by M. A. Cannieu.—On the absorbent power of the bladder in man, by MM. A. Pousson and C. Sigalas. Healthy vesical epithelium is impermeable in general, but absorption may take place (1) when the subject with a healthy bladder requires to void its contents, the urine then bathing the prostatic portion of the urethra; (2) when the vesical epithelium is altered.—On the seat of the colouration of brown oysters, by M. Joannes Chatin.—On the presence of a diastase in *vins cassés*, by M. G. Gouirand.

Erratum.—In the last report, p. 622, line 19 from bottom of second column, "left-handed" curves should read "skew" curves.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Anales del Museo de la Plata. Paleontologia Argentina, ii. and iii. (Contributions to a Knowledge of the Fossil Vertebrates of Argentina): R. Lydekker (La Plata).—A Manual of Forestry: Prof. W. Schlich. Vol. 3: Forest Management (Bradbury).—Organic Chemistry: Prof. I. S. Scarf (Collins).

PAMPHLETS.—Catalogue of the Michigan Mining School, 1892-4 (Houghton).—City and Guilds of London Institute Report, March (London).—An Historical and Descriptive Account of the Field Columbian Museum (Chicago).—Sixty-third Annual Report of the Royal Zoological Society of Ireland (Dublin).—Science and Art Museum, Dublin, Art and Industrial Department. Collection of Weapons, &c., chiefly from the South Sea Islands, deposited in the Museum by the Board of Trinity College, Dublin, July 1894 (Dublin).—On the Relation of Diseases of the Spinal Cord to the Distribution and Lesions of the Spinal Blood-Vessels: Dr. R. T. Williamson (Lewis).—The Federated Institution of Mining Engineers. Report of the Proceedings of the Conference on Inland Navigation, Birmingham, February 12, 1895 (Newcastle-upon-Tyne).—Temperaturmaalinger, i. Lofoten, 1891-1892 (Christiania, Werner).

SERIALS.—Mittheilungen der Hamburger Sternwarte, Nos. 1 and 2 (Hamburg).—English Illustrated Magazine, May (Strand).—Longman's Magazine, May (Longmans).—Good Words, May (Isbister).—Sunday Magazine, May (Isbister).—Quarterly Review, April (Murray).—American Journal of Mathematics, April (Baltimore).—London Catalogue of British Plants, Part 1, 9th edition (Bell).—Journal of the Institution of Electrical Engineers, April (Spon).—Journal of the Royal Microscopical Society, April (Williams).—Bulletin of the American Museum of Natural History, Vol. 6, 1894 (New York).—Natural Science, May (Rait).—Century Magazine, May (Unwin).—Contemporary Review, May (Isbister).—National Review, May (Arnold).

CONTENTS.

THURSDAY, MAY 9, 1895.

THE PYGMIES.

The Pygmies. By A. de Quatrefages. Translated by Frederick Starr. (London and New York: Macmillan and Co., 1895.)

SOME surprise was expressed when Prof. de Quatrefages was appointed, in 1855, to the chair of Anthropology in the Museum of Natural History at Paris. He was then forty-five years of age, and had acquired a considerable reputation as a zoologist, but his published original researches related only to the lower marine forms of animal life. Thenceforward, however, he devoted himself with great energy and success to the cultivation of the subject under his special charge, and the great development of the collections in the Museum and the numerous contributions to the literature of the natural history of man, which he continued to make almost up to the time of his death, three years ago, at the age of eighty-two, abundantly justified his selection for the post. It is true, that during the greater part of this time he had the advantage of the assistance and harmonious co-operation in much of his work of M. E. T. Hamy, who has naturally succeeded to the chair.

The work now under notice, which has just appeared in an English form, was originally published in 1887, as one of the "Bibliothèque scientifique contemporaine," and is essentially popular in its character. It commences by giving an account of the wide-spread belief among the more cultivated nations of antiquity in the existence of a race or races of human beings of exceedingly diminutive stature, who dwelt in some of the more remote and unexplored regions of the earth. The scattered notices of these people, called *Pygmies* by the Greeks, found in the writings of Homer, Aristotle, Herodotus, Ctesias, Pliny, Pomponius Melo, and others, are cited and commented upon. Aristotle places his pygmies in Africa, near the sources of the Nile, and Herodotus gives a circumstantial account of their existence near a river now generally identified with the Niger, while Ctesias describes a race of dwarfs in the interior of India. Whether these legends were merely the offspring of a fertile imagination, or whether they had a solid foundation in fact, may be still an open question. Our author is convinced that the latter view is correct, and devotes the greater part of the work to the task of collecting all the reliable information upon the existing races of people of diminutive stature who inhabit the regions of the earth in which the pygmies of the ancients were supposed to dwell, and to the endeavour to harmonise the scanty notices of those old writers with the facts as now shown by scientific investigation.

A considerable portion of the book is given to an account of the characteristics and culture of that singularly interesting race, the natives of the Andaman Islands, which is naturally taken mainly from the observations of Mr. E. H. Man. These people Quatrefages persists in calling "Mincopies," although it has long been shown that the name is quite unknown in their own language. A chapter is then devoted to showing that people having the general physical characters (small stature, black colour, frizzly hair, and roundish heads) and many of the habits and customs (especially the dexterous use of the bow) of the Andamanese, form a groundwork of the native population of many of the islands of the Malay Archipelago, living mostly in the mountainous regions of the interior. To this race, Quatrefages has given the name of "Negrito." But it is not only in the islands that the Negrito race dwell. Traces of them are found also on the mainland of Asia, but everywhere under the same conditions; in scattered tribes, occupying the more inaccessible mountainous regions of countries otherwise mainly inhabited by other races, and generally in a condition more or less of degradation and barbarism, resulting from the oppressive treatment they have received from their invading conquerors; often, moreover, so much mixed that their original characters are scarcely recognisable. The Semangs of the interior of the Malay Peninsula, the Sakays from Perak, the Moys from Annam all show traces of Negrito blood. In India proper, especially among the lowest and least civilised tribes, not only of the central and southern districts, but almost to the foot of the Himalayas, in the Punjab, and even to the west side of the Indus, according to Quatrefages, frizzly hair, negro features, and small stature, are so common that a strong argument can be based on them for the belief in a Negrito race forming the foundation of the whole pre-Aryan or Dravidian, as it is generally called, population of the peninsula. The crossing which has taken place with other races has, doubtless, greatly altered the physical characters of this people, and the evidences of this alteration manifest themselves in many ways; sometimes the curliness of the hair is lost by the admixture with straight-haired races, while the black complexion and small stature remain; sometimes the stature is increased, but the colour, which seems to be one of the most persistent of characteristics, remains. The localities in which the Negrito people are found in their greatest purity, either in almost inaccessible islands, as were the Andamans till in comparatively recent times, or elsewhere in the mountainous ranges of the interior only, and their social conditions and traditions wherever they exist—all point to the fact that they were the earliest inhabitants; and that the Mongolian and the Malay races on the east, and the Aryans on the west, which are now so rapidly exterminating and replacing them, are later comers into the land. We now see what constitutes the great interest of the Andamanese natives to the student of the ethnological history of the Eastern world. Their long isolation has made them a remarkably homogeneous race, stamping them all with a common resemblance not seen in the mixed races generally met with in continental areas. They are the least modified representatives of the people who were, so far as we know, the primitive inhabitants of a large portion of the earth's surface, but who are now verging on extinction.

The next portion of the book is devoted to an examination of the so-called "pygmy" races of the African continent. These are the well-known Bushmen or "Sân" of South Africa, to whose religious beliefs a whole chapter, derived mainly from the observations of Hahn, is devoted, and another race to which Hamy has given the name of "Negrillos," about which far less is known at present, who seem to hold the same relation to the larger

long-headed African negroes, among whom they dwell, that the small round-headed Negritos of the Indian Ocean do to their larger long-headed Melanesian neighbours. Scattered communities of these small negroes, all much resembling one another in size, appearance and habits, scarcely over four feet in height, and all great hunters, expert with the bow, and living on the produce of the chase, occur at various isolated spots across the great African continent, within a few degrees north and south of the equator, extending from the Atlantic coast almost to the Indian Ocean. In many parts, especially at the west, they are obviously holding their own with difficulty, if not actually disappearing, and there is much about their condition of civilisation and the situations in which they are found, to induce us to look upon them, as in the case of the Bushmen to the south and the Negritos in the east, as the remains of a population which occupied the land before the incoming of the main body of the present natives. If the account of the Nasamonians, related by Herodotus, be accepted as historical, the river they came to, "flowing from west to east," must have been the Niger, and the northward range of the dwarfish people far more extensive twenty-three centuries ago than it is at the present time.

The translator has given, in an appendix, a list of the principal contributions to the literature of the little races of man which have appeared since the publication of the French edition of M. de Quatrefages' book. It would have been still better if he had given some epitome of the considerable advances that have been made in our knowledge of the subject, especially of the recent researches of R. G. Haliburton and Kollmann, which tend to show the former extension of dwarf races over a considerably larger area of the earth's surface than was suspected by our author, such as the whole of North Africa, the Pyrenees, Switzerland, and even Central America.

W. H. Flower.

AN ATTEMPT TO POPULARISE EVOLUTION

A Primer of Evolution. By Edward Clodd. (Longmans, Green, and Co., 1895.)

THE title of this little book is hardly justified by its contents, since it nowhere defines or explains evolution, or deals with it in a systematic manner. As the author tells us in a prefatory note, the book is an abridgment of his former work, "The Story of Creation"; and he does not appear to have made any attempt to rearrange his materials, or to introduce such new matter as was required to constitute it a real introduction to the theory of evolution for those who know little or nothing about it. Such a book should give, at starting, a full statement of what is meant by evolution in modern science and philosophy; should explain how it differs from previous theories of the universe; and should clearly mark out its range of action and its limitations, showing in what way it is supposed to have "evolved" the material universe, and how much must be postulated as the materials and the forces with which it works.

But instead of any explanation of this nature, the first half of the book is devoted to a general descriptive sketch of the universe, inorganic and organic, so brief and elementary as to be quite unnecessary, since any one prepared to enter on the study of evolution would be already acquainted with so much of the facts to be explained. In all this portion, occupying more than half the book, evolution is not once referred to. Then, in the second part, which is headed "Explanatory," all the ground is gone over again, with explanations which assume evolution, but do not often refer to it. Some of this is interesting and well written, the chapter on "Proofs of Derivation of Species" being one of the best; and if this part had been more fully developed, and had been preceded by such an account of the principle of evolution as has been suggested, the work might have been useful to beginners.

But, besides these deficiencies of arrangement and of subject matter, there are more serious defects in numerous obscurities and misstatements, and in the adoption of very doubtful theories as if they were universally accepted. As examples of these faults, the very first sentence states that—"The universe is made up of matter and motion," as if they were things of the same nature. And on turning to the "explanatory" part, we are informed that the "materials which make up the universe" are "matter and motion." On page 3, we are told that "matter is made up of chemical units or elements," about seventy in number, and that "These elements are named atoms." On page 91, we have force and energy defined as being respectively "motion which draws the atoms together," and "motion which drives the atoms apart." This appears to have been adopted from a well-known popular writer, but as it is quite different from what is to be found in the usual text-books it should not have been adopted in a "primer." At page 95, the friction of the ethereal medium in retarding the orbital motion of the planets, is stated as if it were a demonstrated fact. The abundance of the compounds of carbon are said to be partly due to its having "an affinity for itself" (p. 102); and among the erroneous statements of fact we are told that, among the lower races the great toe survives "as a grasping organ" (p. 127), and that there are in America certain wandering tribes who use gestures as "the sole mode of communication" (p. 157). Again, without a word of doubt or reservation, we have the statements that "The origin of life is not a more stupendous problem to solve than the origin of water" (p. 103); and that "mind is the highest product of the action of motion upon matter" (p. 174). These few samples are sufficient to show that this little work requires very careful revision to render it a safe guide for the elementary student.

STEEL AND THE NEW IRON-ALLOYS.

Steel Works Analysis. By J. O. Arnold. (London: Whittaker and Co., 1895.)

CHEMISTS engaged in steel works have long been wanting a trustworthy manual adapted to their special requirements, and this work is the latest attempt to meet the want. The work is undoubtedly an advance on its predecessors, for, while it retains the best of the well-known processes, many newer operations are now, for the first time, published in a comparatively handy form. Everything that a steel works analyst may fairly be called upon to examine, finds a place in this volume.

This applies more especially, perhaps, to the sections treating on the examination of chrome-iron, silicon-iron, nickel alloys, &c.

The volume is particularly valuable as embodying the results of an extensive experience in the examination of certain iron alloys which are bound to become of special importance in the near future; most steel works analysts will cordially appreciate this portion.

As the results of my own practice, I can confirm the accuracy and efficiency of most of the selected methods; more especially as applied to the assay of ferro-chrome, ferro-aluminium, silicon, nickel, &c.

In regard to the assay of ferro-chrome or steels, Galbraith's method is to be preferred, if the precautions given are adopted. The original process did not always give concordant results. The gravimetric methods are, however, on the whole most trustworthy. Results are apt to be low unless great care is taken; no doubt for the reasons shown at page 207. The estimation of small quantities of aluminium presents difficulties not easily overcome; indeed, simpler and less complicated methods are required: a remark which applies to most of the methods now practised.

The assay processes for sulphur and phosphorus are clearly set forth, leaving practically nothing to be desired. For the former element, certainly, gravimetric estimations are best; but it is nearly impossible to obtain the necessary acids quite free from sulphur compounds: this constitutes a serious drawback, and entails the necessity of a blank experiment, which should be avoided when possible. The evolution methods give only relative results, agreeing pretty closely amongst themselves, but somewhat under those obtained gravimetrically. The author's colour test is a good one, but somewhat complicated. A more simple modification of the colour test consists in passing the evolved H_2S through 50 c.c. of a very dilute lead acetate solution ($\frac{1}{10}$ grm. in litre H_2O) contained in a long test-tube. This is compared with a standard steel, treated in the same manner, containing a known percentage of sulphur. No precipitate is formed, and a clear brown tint is obtained, which lasts for some time, and is easily compared with the standard.

The processes advocated for phosphorus (pp. 110-115) are complete, but the necessary manipulative skill required to carry them out can only be acquired by constant practice. I find, however, that the addition of a little HCl to the nitric acid solution assists the precipitation of phosphorus when precipitating with ammonium molybdate. Further, I agree with the author that in ordinary steels the presence of silicic acid may be ignored: with regard to time, fifteen or twenty minutes is ample; if longer, molybdic acid is precipitated. In addition, even if this does not occur, the precipitate may redissolve to a notable extent. The dried phospho-molybdate precipitate is distinctly soluble in dilute nitric acid.

The author's method of precipitating arsenic with H_2S is good, but no others are given. The process with modifications gives good results, but the ordinary method is preferable when it is desired to estimate this element. For the mere elimination of arsenic from the phosphoric acid, in order to determine the latter, the boiling or distillation process is useful.

It is to be regretted that no trustworthy process has been given for the determination of oxygen in steel. A thorough examination of the whole work, however, reveals the pains taken by the author, not only as regards the portions mentioned in the foregoing, but also in the somewhat less important sections dealing with fuel and other materials. There can be little question that Prof. Arnold has rendered steel-works analysts a decided service by the publication of his work. JOHN PARRY.

OUR BOOK SHELF.

Wayside and Woodland Blossoms. A Pocket Guide to British Wild Flowers for the Country Rambler. By Edward Step. With coloured figures of 156 species, black and white plates of 22 species, and clear descriptions of 400 species. (London: Frederick Warne and Co., 1895.)

MANY persons who admire the beautiful flowers that adorn our woods and pastures would fain know their names, with a view to further knowledge of them; but for various reasons they are unable to use the ordinary "Flora," however simply compiled. Here is a little book that will meet the wants of such persons, and do more, we believe, to lay the foundation of a sound knowledge of plants than the form in which "life-histories" are taught in ordinary schools and classes for the purpose of passing an examination. In spite of all that is said to the contrary, to know a large number of plants, animals, or minerals by sight, is of more value, to begin with, than a more detailed knowledge of a single, or few, organisms or objects; especially when this detailed knowledge is gained by rote, and not by observation. We therefore commend this little book to the notice of those interested in, and believing in, small beginnings, though the kind of information it contains is not exactly what the examiner demands. The coloured figures are well drawn, and the colouring, although a little crude, is good enough to enable one to recognise the plants the figures are intended to represent. The majority of the common and prominent plants of our native flora are figured. Many of them are drawn of the natural size, whilst others are reduced and a few enlarged, without indications of the reduction or enlargement. These things should be explained for a beginner. The descriptive and explanatory letterpress is instructive, and free from pedantry, by which we mean the display of technical terms only used by "teachers" of botany; not by botanists. There are some inconsistencies in the choice of subjects for illustration. For example, the exceedingly rare *Holosteum umbellatum* is represented, whereas the allied genus *Cerastium*, found in every county, and perhaps in every parish and field in the kingdom, is left out. There is also an unexplainable absence of characteristic sea-side plants. The black and white figures mentioned in the title represent native trees and some of the commonly-planted exotic species. An omission here is the common yew, which might well have taken the place of the very poor figure of *Ailantus*. In spite of the shortcomings indicated, we strongly recommend this little pocket-book to those in search of some practical knowledge of common wild plants. W. B. H.

The Lepidoptera of the British Islands; a Descriptive Account of the Families, Genera, and Species indigenous to Great Britain and Ireland, their Preparatory States Habits, and Localities. By Charles G. Barrett, F.E.S. Vol. ii. Heterocera, Sphinges, Bombyces. (London: L. Reeve and Co., 1895.)

MR. BARRETT'S great work on British *Lepidoptera* is making steady progress, and we are glad to find that the second volume which includes the Sphinges and the first

nine families of Bombyces, ending with the *Psychidæ*, is written in the same careful and painstaking manner as its predecessor. The first volume has been well received abroad, but the foreign critics regret the absence of references, a deficiency more felt by them than by British lepidopterists. The foreign critics speak of the plates as a veritable storehouse of remarkable varieties; but we must again comment very severely on the action of the publishers in issuing two editions of the work, one with, and the other without illustrations, without any reference to the illustrated edition in the letterpress of the other, so far as we have noticed; and in the case of the second volume, without even as much as an advertisement to call attention to its existence.

There are several points of general scientific interest suggested by an examination of Mr. Barrett's book. A great number of species recorded as British by the older entomologists, but rejected by Doubleday and Stainton, have latterly been rediscovered and reinstated. This has happened so often, that it seems likely that when we eliminate accidentally introduced species (chiefly North American), and European species wrongly determined, it will be found that the information given by the older writers was far more accurate than the writers of the middle of the century were at all disposed to admit. Nor did the latter allow for the difficulty of communication with the continent at the beginning of the century, which added much to the improbability of specimens asserted to have been taken in England, having been simply brought over from the continent.

In estimating the probability of a reputed species being truly British, the chief factor to be taken into account is its continental range. It is evident that the British fauna is slowly changing, some specimens becoming rarer or even disappearing, and others becoming commoner, or establishing themselves in England for the first time. There is also some tendency in Mediterranean species to extend their range further north in Western Europe. As the late Mr. Stainton once remarked, the comparison of our present lists with those of the future, will be likely to yield highly unexpected and interesting results.

W. F. K.

Quellenkunde. Lehre von der Bildung und vom Vorkommen der Quellen und des Grundwassers. Von Hyppolyt J. Haas. 8vo. pp. 220. Illustrations in the text. (Leipzig: J. J. Weber, 1895.)

PROF. HAAS, of Kiel, when asked to edit and bring up to date the "Quellenkunde" of Abbé Paramelle, came to the conclusion that in order to state the present position of the science of springs and underground water in a satisfactory form, an entirely new work was necessary. Hence the book under notice. In such small compass, nothing approaching a complete treatise could possibly be attempted. The chief features of springs, their classification and relation to geological conditions, are discussed according to a clearly arranged plan under five principal heads. First comes a discussion of springs in general, including an historical introduction, in illustration of which several of Athanasius Kircher's quaint pictures are reproduced. The following sections deal with thermal and mineral springs, underground water, and the art of finding springs. In the last division we find some remarks on the divining rod. The book should prove useful to students of physical geography and to those concerned with the practical utilisation of a water-supply derived from wells.

A number of diagrams are reproduced from the works of Daubrée and other authorities. Although several English authors are cited, we fear that Prof. Haas has not made himself familiar at first hand with the literature of the subject in English, which is by no means meagre in records of original observations on the movements of underground water, and deserves more recognition than it receives.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Uniformitarianism in Geology.

DR. ALFRED WALLACE, in his letter to NATURE of May 2, calls attention to the significant fact that catastrophes caused by volcanoes "may be of greater magnitude now than in geologic times," owing to the crust of the earth being thicker now than it was then. He, however, is mistaken in supposing that this consideration has been overlooked by geologists. If he will kindly refer to "Geology," vol. i. p. 440, he will find it there stated, speaking of the older fissure and explosive eruptions, that "there is nothing to show that this [the explosive] action was on the same scale of magnitude and permanence as those of late Tertiary and recent date. With the greater thickness of the earth's crust and the greater resistance presented by its rigidity, volcanic eruptions must with time, as suggested long ago by Elie de Beaumont, have altered with the alterations of those conditions, and may now be exhibited under a phase very different from those of the earlier periods."

Or again, he will find in "The Position of Geology" ("Collected Papers," p. i.) it stated that, though one form of volcanic action (the fissure) was more active in the past than at present, that "explosive eruptions are more violent now than in former times." And again, at p. 145 of the same work, I remark that "while with the thinner crust of former times, there would be a more frequent extrusion of the molten rock, there are probably with the thicker crust now formed and consequently its greater resistance, greater forces stored in the explosive eruptions of the present day."

The instance relied upon by Dr. Wallace is, however, another striking example, if others were needed—though in this case it is on the inverse side as against meteorological agencies—of the non-uniformity *in degree* between the action of the forces of past and present times. The increased thickness of the crust is not, however, the sole cause of the violence of recent eruptions, nor are they, I imagine, due to the presence of occluded water in the volcanic foci. The terrific eruptions of Krakatão and other volcanoes are, I conceive, due simply to the access of vast volumes of surface waters and their sudden flashing into steam.

Volcanic action, therefore, does not seem to me to be in any way in contradiction to the conception of uniformity of *kind* or law, and to non-uniformity on the question of *degree*.

Sevenoaks, May 4. JOSEPH PRESTWICH.

Green Oysters.

I HAVE just received a "Note," extracted from the *Monitore Zoologico Italiano*, of Florence, by Dr. Carazzi, in which a number of unsupported statements are made as to "phagocytosis in Mollusca."

Amongst other statements, I find "Non solo sono osservazioni erronee quelle del Lankester, malamente ripetute dello Chatin, ma lo sono egualmente quelle del Pelseneer e del Bruyne." I am surprised that my zoological friends in Florence should publish a bare statement of this nature without a shred of evidence to support it. I desire to draw attention to the simple assertion made by Dr. Carazzi, and to let those who are responsible know that I and others expect him to show in detail what is the error in the observations published by me on the green oysters of Marennes.

It is certainly not a usual thing for a Society to allow an author to print vague accusations of inaccuracy in reference to other writers, without the smallest attempt to justify such accusations.

Dr. Carazzi's assertion is all the more remarkable, since it appears that he has not examined the true *huîtres de Marennes* at all, and is singularly ill-informed as to the histology and physiology of Mollusca.

I shall be very much surprised if Dr. Carazzi can show that the observations published by me on green oysters in 1886 (*Quart. Journ. Micr. Sci.* vol. xxvi.) are erroneous, and shall at once re-examine the matter if he succeeds in throwing doubt on the facts as stated by me.

Inferences from observed facts stand in a different position from the observations themselves.

I was the first to describe the cells laden with green granules

which occur in the epithelium of the gills and labial tentacles of the Marennes oyster.

I also showed that such cells are present in the common oysters, but that the granules they contain are not green. I further showed that these cells occur abundantly on the *surface* of the gills, crawling about and exhibiting amœboid movement. I also showed that the Marennes oysters are specially fed upon *Navicula ostrearia* which contains a highly refractory blue pigment "Marennin," and I *inferred* that the granular cells of the gills derive their colour from the blue pigment of the naviculæ—since it was shown long ago by Gaillon (in 1824) that the *huitres de Marennes* are purposely placed by the oyster-culturist into tanks containing the *Navicula ostrearia*; that when placed there they have gills of the usual yellow-brown colour, but rapidly acquire the green colour; that they actually feed on the *Navicula ostrearia*, and that when removed from this article of diet, they lose the green colour of gills.

The inference that the "granular cells" are to be regarded as wandering phagocytes, was not first published by me; and, though I have no doubt of its justification, I may point out that it is an interpretation, and not an observation of fact.

Lastly, let me say that I showed by chemical analysis that the green colour of the oyster's gill is *not* due to any metallic base—either copper, iron, or chromium. The statement made by Carazzi that there is "abbondanza di sesqui-ossido di ferro" in the mud of the tanks where the oysters are fed, is therefore doubly futile. Every one knows that such mud contains abundance of iron; but as there is no iron in the green pigment of the oyster, it is useless to draw attention to the iron in the mud. E. RAY LANKESTER.

Oxford, May 4.

The Origin of the Cultivated Cineraria.

I MADE two objections to Mr. Dyer's account of the history of the Cineraria; the careful reader will observe that his letter meets neither. Mr. Dyer informed us that the cultivated Cinerarias were produced "*by the gradual accumulation of small variations*," *i.e.* without the selection of definite sports. My object in adducing historical evidence of Cineraria sports was to prevent Mr. Dyer's pronouncement from being repeated without further evidence. That purpose I think has been attained; for I notice that in now restating his account Mr. Dyer does not refer to the point, though it was the object of his original exhibition of the Cineraria to the Royal Society. That the Cineraria was an excellent "illustration of the amount of variation which could be brought about under artificial conditions in a limited time" I should be the last to dispute. As I showed in my first letter, there is evidence that the time was very short indeed.

Compared with this point, the second question—that of the hybrid origin of cultivated Cinerarias—is of subordinate interest. For the view that they were originally hybrids, resulting from crosses between *C. cruenta*, *C. lanata*, and other species, I have given the evidence, quoting the explicit statement of contemporaries and the almost universal opinion of practical gardeners, with references to the sources of information. Mr. Dyer, however (with him Mr. Rolfe) declares that they are descended from *C. cruenta* alone. Is this statement a mere inference from the want of likeness between particular cultivated Cinerarias and the wild species, or have Mr. Dyer and Mr. Rolfe evidence of a more substantial character? Of course these authorities may be right, and the rest who have written on the matter may be wrong; but I ask for proof of this, and the request can hardly be thought unreasonable.

Mr. Dyer has referred to a remark I made at the meeting respecting the Camellia. At the risk of diverting attention from the real issues, I feel bound to speak of this, for I was then in the wrong. In justice the circumstances must be stated. Speaking of the Cineraria, Mr. Dyer declared that though the flowers have changed so much, the foliage, which had not been an object of Selection, still resembled that of his wild plant. I replied that though this might be true of the Cineraria, it led to no universal induction, for it is well known that the foliage of many plants selected solely for their flowers or for their fruits had varied greatly. As an illustration taken on the spur of the moment, I said that though the matter had not come within my own observation, there was, I believed, a passage in one of Darwin's books to the effect that the foliage of the several kinds of Camellia differed so much that they could be recognised by it alone. Upon Mr. Dyer interjecting that this was not true, I immediately gave up the illustration as not coming within my own knowledge, and substituted that of the Apple, of which I myself know several kinds to have distinct and characteristic foliage. Such examples may be multiplied indefinitely. Now the passage in Darwin is as follows:—"Verlot mentions a gardener who could distinguish 150 kinds of Camellia when not in flower" ("Animals and Plants," ed. 1885, II. chap. xxii. p. 238); but Darwin takes the case as an illustration of the fact that structures "though appearing to an unpractised eye absolutely undistinguishable, yet really differ." My use of this case was therefore a wrong one, and as Mr. Dyer has thought fit again to refer to the matter, I take the opportunity of withdrawing it once more. W. BATESON.

St. John's College, Cambridge, May 5.

The Assumptions in Boltzmann's Minimum Theorem.

MR. CULVERWELL's letter in your issue of April 18 leaves many important points in connection with the reversibility of Boltzmann's Minimum Theorem untouched. On the question as to what different people mean (or think they mean) when they assert that the theorem is true, enough has already been said. What we want to know is what assumptions are involved in the mathematical proofs of the theorem, why they have to be made, and for what systems they are likely to hold. This question has been ably treated by Mr. Burbury, but in view of Prof. Boltzmann's assertion that the theorem is one of probability, it is desirable to examine more fully where probability considerations enter into proofs such as Dr. Watson's, which contain no explicit reference to them.

Dr. Watson starts by assuming two sets of molecules so distributed that the *numbers* having coordinates and momenta within the limits of the corresponding differentials are

$$F(P_1 \ldots Q_m)dP_1 \ldots dQ_m \text{ and } f(p_1 \ldots q_n)dp_1 \ldots dq_n.$$

If, however, the differential elements are taken very small (as when we consider a volume-element comparable with molecular dimensions), these expressions no longer represent numbers of molecules, and it is *assumed* that in this case they represent the *probabilities* of a molecule having coordinates and momenta within the given limits.

It is then necessary to *assume* that *the probabilities for the two kinds of molecules are independent of each other*. This assumption was pointed out to me by Mr. Burbury, and is what I intended to imply in my previous letter when I said that Dr. Watson's assumption was more *natural* than any other. Under these circumstances alone can we assert that the probability of a given combination of coordinates and momenta of *two* molecules is proportional to

$$FdP_1 \ldots dQ_n \times fdp_1 \ldots dq_n$$

To make the proof independent of the choice of coordinates, let $y_1 \ldots y_{m+n}$ be *any other* system of coordinates specifying the *pair* of molecules, so chosen that $y_1 = 0$ at the beginning of an encounter. Then if $x_1 \ldots x_{m+n}$ denote the corresponding momenta, we may employ the theorem proved in my last British Association Report, § 14, to write the above expression in the form

$$FfJdy_1dy_2 \ldots dy_ndx_1 \ldots dx_{m+n}.$$

and if we write $(dy_1/dt)dt$ for dy_1, the probability of a configuration in which an encounter will take place in the time-element dt becomes

$$FfJdy_2 \ldots dx_{m+n}(dy_1/dt)dt$$

corresponding to Watson's expression with (dy_1/dt) in place of (dq_n/dt). This step involves the assumption (made above) that dy_1 is small in comparison with the dimensions of a molecule.

From this point on Dr. Watson's proof is easy. But it will be seen that the probabilities for two molecules are not independent of each other *after* a collision between them. The method would fail if the same pair of molecules were likely to collide repeatedly. Thus the Minimum Theorem depends on the free motions of the molecules quite as much as on the collisions themselves, and it only applies to gases whose molecules mix freely among each other between collisions, not to media where they are densely crowded. In such cases, however, we have Mr. Burbury's investigation (*Phil. Mag.* January 1894).

If we were to reverse the motion exactly, we should have one in which the probabilities for two molecules *before* an

encounter were not independent, and our assumptions (*however improbable*) would be therefore entirely based on our previous experience with the direct motion. Without such assumptions we should have inferred, by the ordinary laws of probability, that H would be likely to decrease. This is what I intended to imply in my previous letter; but as I had used accented and unaccented letters in my statement, I failed to make my meaning clear to Mr. Culverwell, who evidently found it difficult to understand a proof involving their use. G. H. BRYAN.

The Unit of Heat.

I WAS glad to read Prof. Joly's communication in your issue of May 2, for I have made many efforts to call attention to the unsatisfactory nature of our present system of calorimetric measurements, and now that a more powerful voice than mine has been raised in favour of a change, I have some hopes of progress.

The indifference with which, as it appears to me, our physicists regard this matter is probably due to several causes. They ignore the fact that the science of calorimetry has recently made great strides, and that an ambiguity as to the unit, which formerly was of little consequence, has now become almost the only bar to further progress; also, as Prof. Joly has pointed out, our system of calorimetric measurements has been so wedded to the method of mixtures, that the union has (wrongly) come to be regarded as essential.

As to Prof. Joly's proposal, there is much to be said in its favour. It is practical and definite. At the same time the change would be so radical, that I should not feel justified in counting myself as his disciple in this matter without serious consideration.

My own inclination is rather in the direction of a C.G.S., or absolute unit, and the course adopted by Prof. Schuster and Mr. Gannon, in entitling their recent important communication to the Royal Society "The Specific Heat of Water," rather than the "Mechanical Equivalent of Heat," shows that a step has already been taken in this direction.

When we reflect on the attention and the labour which have been devoted to the establishment of our present system of electrical units, it is a cause for wonder that so important a unit as that of heat should have been left ill-defined and unregarded.

I would propose that at the forthcoming meeting of the British Association, the attention of Section A should be particularly directed to this matter; and it would prepare the way for such action if those who have definite proposals to make would, in the meantime, communicate them to your columns.

Cambridge. E. H. GRIFFITHS.

REFERRING to Dr. Joly's letter last week, would it not be well definitely to adopt the "Joule" as the only fundamental unit of heat, and to realise distinctly that researches such as those of Mr. Griffiths, Prof. Rowland, and Dr. Joly are determinations of the specific heat of water and of the latent heat of steam in terms of it? OLIVER J. LODGE.

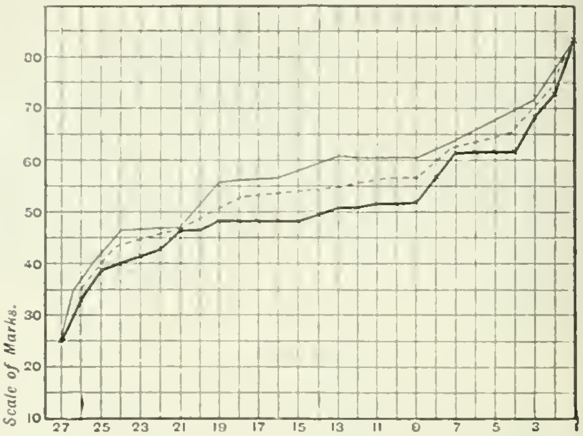
The Examination Curve.

THE extremely interesting article, by Prof. Lloyd Morgan (vol. li. pp. 617–619), on the graphic representation of the marks given in an examination, and of their great use to an examiner, leads me to ask whether even this method may not be developed further with advantage to all concerned, for, as Lloyd Morgan says: "If, after an extensive set of papers has been looked over and carefully marked, an interval of time be allowed to elapse, and then the papers are gone over again, the result of this re-examination is that the head and tail remain practically unchanged, but that there is not a little redistribution among the mediocrities." In other words, the personal equation of the examiner varies, showing itself mostly in the middle of the curve.

The first thing to strike me on looking at Fig 2 (vol. li. p. 618), was the great similarity of the two halves of the curves, and on tracing it, and then turning the tracing half round so that the upper end of the traced curve became superimposed upon the lower end of the original, and *vice versa*, the similarity was so marked as to make one think, that had a larger number of papers been examined and as carefully marked as the first set, the traced curve would have covered the other.

If such be the case, why should not the examiner, after plotting the marks he thinks best, make a tracing of the curve, then reverse it, superimposing the two ends as before, and sketch it in alongside his first curve (easily done by means of oil-paper), then, if they differed, draw a fresh curve midway between the two: subsequently re-marking his examination papers from this smoothed mean curve? An illustration may be of use: let it be founded on Fig. 1, as it contains the less smooth curve. The dark line is that of the marks first adjudged; the light line, the same curve reversed; and the dotted line, the smoothed mean curve of the two from which his papers are finally marked.

Granting that the plus variations and the minus variations on the two sides of the mean nearly balance, the question would appear to be—Would one be justified in smoothing them in accordance with the generalised results of many such series? It involves some forcing of the examiner's marking into the general mould, but would this be more than sufficient to correct



Examinees. FIG. 1.

his personal equation? On the other hand, the two halves—say from paucity of examiners—might be so dissimilar, that the mean curve would differ very much from the original form. In this case, would it be possible to give any general rule whereby one could be guided whether to adopt the mean curve, or to remain satisfied with the original marks given?

In Herbert Spencer's "Principles of Sociology," (vol. i. p. 88) are many references to the fact that "the children of Australians, of Negroes in the United States, of Negroes on the Nile, of Andamanese, of New Zealanders, of Sandwich Islanders [and others], are quicker than European children in acquiring simple ideas, but presently stop short from inability to grasp the complex ideas readily grasped by European children, when they arrive at them." F. HOWARD COLLINS.

April 29.

Teaching Young Pheasants to Peck.

IT may interest Prof. Lloyd Morgan and others to know that when Assamese find newly hatched chicks in the jungles, they have a system of teaching the little ones to peck and pick up food, without which, I am told, many of them would die.

Walking down a road one morning with a neighbour, we suddenly noticed a little ball of fluff between my feet, and I could hardly avoid stepping on it, as it stuck close to me; almost immediately another appeared at my friend's feet, and we saw they were newly-hatched pheasants, the mother probably carried off by some wild cat.

As it was difficult to walk with these little things running so close and in the way, we lifted them into the short grass alongside, and hurried on some fifty yards.

On returning we had forgotten them, but one ran out, and so pertinaciously stuck to my boots, that to save it I put it into my pocket, and on our arrival at the bungalow tried to feed it with small fragments of hard-boiled egg, rice, and white ants. Of all these it took no notice.

Next morning the other chick was found at the foot of the bungalow steps, having probably followed us unnoticed the day before. I then called my "Babu," as I could not get them to eat, and he said "they must be *taught*."

He put the gauze wire cover they were under, and the crushed

rice, egg, &c., on a hard wood table, and taking a pencil from his pocket and collecting the eatables together, close to the edge of the gauze cover, he lifted its edge, and with the pencil point inserted, began sharply tapping among the rice débris. The two chicks at once ran over to that place and bent over, watching the tapping, and to our astonishment they began tapping with their little beaks the same way, and before long had begun to feed on their own account, just as the "Babu" had predicted; and after that lesson we had no trouble.

As I happen to be writing, I may mention that our land lizard (3 feet 6 inches to 4 feet 6 inches total length, name unknown to me) has begun calling in the early dawn and dusk at evening. It is silent during the day and night.

From the bearings taken, it can be heard plainly at a mile in forest, and often five or six calling at once in different directions. The native Asamese name is "Gui," which is precisely the sound it makes; by the old spelling it is "Gooee." S. E. PEAL.

Sibsagar, Asam, April 4.

The Bagdad Date-mark.

THERE will be found in Grattan Geary's "Through Asiatic Turkey" all about the date-mark—a mysterious and troublesome excoriation, coming only once, but which lasts a year, leaving an ugly scar the size and outline of the fruit—visitors for any length of time at Bagdad seldom, and residents never, escape. It is also known at Aleppo and other places, but is worst in Bagdad, almost every native being marked. Even nitric acid has been found to have little effect upon it. I lately spent forty-four days, off and on, at Bagdad, and imagined I had escaped: not so, however, as it proved six weeks after my return to India. But the mark yielded forthwith, and before any damage was done, to hyposulphite of soda, which does so much "fixing" for every amateur photographer, and seemed worth trying. The fact may be usefully mentioned in the interest of Mesopotamian explorers who do not want to be date-marked as a memento; but it is to physiologists they must look for an explanation.

Bombay, April 12. A. T. FRASER.

THE ROYAL SOCIETY SELECTED CANDIDATES.

THE following are the names and qualifications of the fifteen candidates recommended by the Council of the Royal Society, on Thursday last, for election into the Society.

J. WOLFE BARRY,

C.B., Civil Engineer. Vice-President of the Institution of Civil Engineers. Is eminently distinguished in his profession, and has designed and executed many works of national importance, which include the Tower Bridge, opened by H.R.H. the Prince of Wales, 1894; the City Terminus extension of the Charing Cross Railway, the Inner Circle Railway, and the Barry Dock. Has served as a member of the following Royal and Departmental Commissions:—Royal Commission on Irish Public Works, 1887; Highlands and Islands of Scotland Commission, 1890; Commission on the River Ribble, 1891; Thames Navigation Commission, 1894. Member de la Commission Consultative des Travaux de la Campagnie Universelle du Canal Maritime de Suez. Is the author of many papers, mainly in reference to engineering works, which have been published in the *Transactions of the Institution of Civil Engineers* and elsewhere. Is the author of several professional treatises, among which the following are the more important: "The Barry Dock" (British Association Report, 1888): "Railway Appliances," "Railways and Locomotives," published in conjunction with Sir F. Bramwell, Bart.

ALFRED GIBBS BOURNE,

D.Sc. (Lond.), Professor of Biology in the Presidency College, Madras. Fellow of University College, London. For many years engaged in teaching and in researches upon Comparative Anatomy and Embryology, especially of Invertebrata. Especially known to comparative anatomists for his discoveries in the structure of leeches, and as discoverer of the hydroid phase of Limnocodium, also of two remarkable new genera of Chætopod worms, described by him as Haplobranchus and Chætobranchus. Author of the following, as well as several other memoirs: "On the Structure of the Nephridia of the Medicinal Leech" *Quart. Journ. Micros. Sci.*, 1880); "Contributions to the Anatomy of the Hirudinea" (*ibid.*, 1884); "On the Hydroid Form of Limnocodium" (*Proc. Roy. Soc.*, 1884): "On the Supposed Communication of the Vascular System with the Exterior in Pleurobranchus" (*Quart. Journ. Micros. Sci.*, 1885). Since he has been in India, Prof. Bourne has sent home important researches on Indian Earthworms, on Chætobranchus (a new naidiform worm), on a new Protozoon of the genus Pelomyxa, with observations on the structure of protoplasm, and some valuable experimental researches on the suicide of Scorpions (*Proc. Roy. Soc.*, 1889).

GEORGE HARTLEY BRYAN,

M.A., Fellow of Peterhouse, Cambridge. Lecturer (on Thermodynamics, &c.) on the University list. Fifth Wrangler, 1886; Class I, Division 1, 1887; bracketed with Senior Wrangler, Smith's Prize, 1888, for the Essay "On the Curves on a Rotating Spheroid of Finite Ellipticity" (*Phil. Trans.*, 1889 A). Author of the following papers:—"On the Stability of a Rotating Spheroid of Perfect Fluid" (*Proc. Roy. Soc.*, vol. xlvii.); "On the Stability of Elastic Systems"; "Waves on a Viscous Rotating Cylinder" (*Proc. Camb. Phil. Soc.*, vol. vi.); and several others in *Phil. Mag.*, *Proc. Lond. Math. Soc.*, and *Proc. Camb. Phil. Soc.*, &c. Also joint author, with Mr. Larmor, of the Report on Thermodynamics, published in the British Association Reports, 1891.

JOHN ELIOT,

M.A. (Cantab.), Meteorological Reporter to the Government of India. Late Meteorological Reporter to the Government of Bengal. Was Second Wrangler and Smith's Prizeman, 1869. Mr. Eliot, as Meteorological Reporter to the Government of Bengal, and subsequently as Head of the Meteorological Department of India, has made many important additions to the physical data of Indian meteorology, and has done much in their utilisation, and in the improvement of the administration of the department of which he is now the head. Under him have been carried out the publication of Daily Weather Charts for the Bay of Bengal and Calcutta, for Bombay and the Western Coasts of India, and general charts for the whole peninsula. He has also organised the systematic collection of marine observations from ships arriving at the chief Indian ports. His special work, contained in a long series of memoirs, published either in separate form by the Meteorological Department, or in the *Journal of the Asiatic Society of Bengal*, chiefly relates to storms in India and Indian seas, and comprises complete histories and discussions of fifteen cyclones and upwards of one hundred storms that have occurred between 1877 and 1886. The Annual Reports of the Meteorological Department, prepared by him, also contain many valuable and original discussions. He has contributed very largely to establish the Indian Meteorological Department on a thoroughly scientific basis, and to maintaining its high character and recognised practical importance to our great Indian dependency.

JOSEPH REYNOLDS GREEN,

D.Sc. (Cantab.), M.A., B.Sc. (Lond.), F.L.S. Professor of Botany, Pharmaceutical Society of Great Britain. Distinguished for his acquaintance with botany. Attached to science, and has contributed to its progress by discoveries in the region of physiological chemistry, with reference chiefly to plants. His more important contributions are contained in the following papers:—"On the Organs of Secretion in the Hypericaceæ" (*Journ. Linn. Soc.* (*Bot.*), vol. xx., 1883); (with Dr. Sheridan Lea) "Some Notes on the Fibrin-ferment" (*Journ. of Physiol.*, vol. iv., 1883); "On the Edible Bird's Nest of the Java Swift" (*ibid.*, vol. vi., 1885); "On Proteids occurring in Latex" (*Proc. Roy. Soc.*, 1886); "On the Action of Sodium Chloride in dissolving Fibrin" (*Journ. of Physiol.*, vol. viii., 1887); On Certain Points connected with the Coagulation of the Blood" (*ibid.*); "On the Changes in the Proteids of the Seed which accompany Germination" (*Phil. Trans.*, 1887); "On the Germination of the Tuber of the Jerusalem Antichoke" (*Annals of Botany*, vol. i., 1888): "On the Germination of the Seed of the Castor-oil Plant" (*Proc. Roy. Soc.*, 1888); "On the Occurrence of Diastase in Pollen," (Brit. Assoc. Report, 1891); "On the Occurrence of Vegetable Trypsin in the Fruit of *Cucumis utilissimus*" (*Annals of Botany*, vol. vi., 1892); (with Prof. Vines) "On the Reserve Proteid of the Asparagus Root" (*Proc. Roy. Soc.*, 1892); "On the Ger-

mination of the Pollen-grain and the Nutrition of the Pollen-tube" (*Phil. Trans.*, 1894): "On Vegetable Ferments" (*Annals of Botany*, vol. vii., 1893); "On the Influence of Light on Diastase" (*ibid.*, vol. viii., 1894).

ERNEST HOWARD GRIFFITHS,

M.A. Private Tutor. Author of the following papers:—"On the Comparison of Platinum Temperatures with the Kew Standard" (Rept. of Committee on Electrical Measurements, Brit. Assoc., 1890); "On the Determination of certain Boiling and Freezing Points" (*Phil. Trans.*, 1891 A); "The Electrical Resistance of Platinum Wire at Absolute Zero" (*Phil. Mag.*, Dec., 1892); "On the Determination of Low Temperatures by Platinum Thermometers" (*Proc. Camb. Phil. Soc.*, vol. viii., Part I.); "On the Increase in Resistance of a Conductor when Transmitting a Current" (*ibid.*, vol. viii., Part I.); "The Mechanical Equivalent of Heat, together with an Investigation into the Changes in the Capacity for Heat of Water" (*Phil. Trans.*, 1893 A); "The Boiling Point of Sulphur, together with a Method of Standardising Platinum Thermometers," jointly with Mr. Callendar (*Phil. Trans.*, 1891 A).

Supplementary Certificate. Appendix to the communication entitled "The Mechanical Equivalent of Heat" (*Proc. Roy. Soc.*, vol. lv., 1893); "A Method of Joining Glass and Metal Tubes" (*Proc. Phil. Soc. Camb.*, 1893); "The Measurement of Temperature" (*Science Progress*, 1894); "The Influence of Temperature on the Specific Heat of Aniline" (*Phil. Mag.*, 1895); "The Latent Heat of Evaporation of Water" (read Royal Society, January 1895).

CHARLES THOMAS HEYCOCK,

M.A., Lecturer on Natural Science, King's College, Cambridge. Author of "Revision of the Atomic Weight of Rubidium" (Brit. Assoc. Rept., 1882); joint author of: "Spectrum of Indium" (*Phil. Mag.* [5] I., 1876); "On a Simplified Form of Apparatus for Determining the Density of Ozone" (*Proc. Camb. Phil. Soc.*, v.); "Lowering of the Freezing Point of Tin by the Addition of other Metals" (*Proc. Chem. Soc.*, No. 65, 1889); "Lowering of the Freezing Point of Sodium by the Addition of other Metals" (*Trans. Chem. Soc.*, lv., 1889); "Molecular Weights of Metals when in Solution" *ibid.* (lvii.); "Freezing Point of Triple Alloys of Gold, Cadmium, and Tin" (*ibid.*, lix.); "Lowering of the Freezing Points of Cadmium, Bismuth, and Lead, when alloyed with other Metals" (*ibid.*, lxi.); "Isolation of a Compound of Gold and Cadmium" (*ibid.*); "Freezing Point of Alloys in which Thallium is the Solvent" (*ibid.*, 1894); "Freezing Point of Triple Alloys" (*ibid.*); "Change in the Zero of Mercury Thermometers" (*Proc. Camb. Phil. Soc.*, vii.).

SYDNEY JOHN HICKSON,

D.Sc. (Lond.), M.A. (Cantab.), Hon. M.A. (Oxon.), F.Z.S. Fellow of Downing College, Cambridge. Author of papers published in the *Philosophical Transactions*, "On the Ciliated Groove (Siphonoglyphe) in the Stomodæum of the Alcyonarians" (1883); "On the Sexual Cells and Early Stages in the Development of *Millepora plicata*" (1888). In the *Quart. Journ. Micros. Sci.*, "The Eye of Pecten" (1880); "The Eye of Spondylus" (1882); "The Structure and Relations of Tubipora" (1883); "The Eye and Optic Tract of Insects" (1895). In the *Tijdschr. van het Nederl. Aardrijkskund. Genootsch.*, "Omzwervingen in Noord-Celebes" (1887). In the *Journ. Anthrop. Inst.*, "Notes on the Sengirese" (1886). Author of the work, "A Naturalist in North Celebes."

HENRY CAPEL LOFFT HOLDEN,

Major, Royal Artillery. In India from 1877–84, he carried out a number of experiments in telephony and telegraphy for the Indian Government. Since 1885 he has been in charge of the Department for the proofs of Naval and Land Service Ordnance, and Gunpowders, and for experiment work connected therewith, and has invented and constructed many pieces of apparatus connected with the science of artillery, as well as with electrical and scientific research. Amongst those which have been publicly exhibited are his devices in connection with the chronograph, for measuring the velocity of projectiles; an extremely accurate and sensitive hydrometer for measuring the variations of the density of the acids in the electrolyte accumulator cells (exhibited Royal Society, 1887; see also paper before Iron and Steel Inst., 1891); a high-speed chronographic pen for recording minute intervals of time by electromagnetic means; various instruments for making accurate and rapid tests of the pressure and current in direct current circuits, and in alternating current circuits of both high and low frequency (some exhibited Royal Society, 1892); an instrument for rapidly ascertaining the E.M.F. and resistance of a galvanic cell (exhibited Royal Society, 1893); a compact moving coil galvanometer adapted to universal purposes, which was employed by Profs. Dewar and Fleming in their researches on the resistance of metals, and is used in the recording pyrometer of Prof. Roberts-Austen. He was deputed by the Commander-in-Chief to write the electrical sections of the Paris Exhibition of 1889, the Frankfort Exhibition of 1891, and the Chicago Exhibition of 1893, and furnished the Government with most valuable reports.

FRANK McCLEAN,

M.A., LL.D. (Glasg.), F.R.A.S., M.I.C.E. Author of "Photographs of the Red End of the Solar Spectrum from D to A" (*Monthly Notices*, vol. xlix.); "Parallel Photographs of the Sun, Iron, and Iridium, from H to near D" (*ibid.*); "Comparative Photographs of High and Low Sun H to A, with Notes on the Method of Photographing the Red End of the Spectrum" (*ibid.*, vol. li.); "Comparative Photographs of Sun and Metal Spectra" (Series 1 and 2, *ibid.*, vol. lii.). Inventor of McClean's Star Spectroscope, an invaluable aid in the study of stellar spectra. Attached to science, and anxious to promote its progress. Founder of the Isaac Newton Scholarship at Cambridge. Donor of a large telescope to the nation, to be used in physical inquiries at the Royal Observatory, Cape of Good Hope.

WILLIAM MACEWEN,

M.D. (Glasg.), Hon. LL.D. (Glasgow). Professor of Surgery, University of Glasgow. A distinguished Surgeon. Author of: "Observations concerning Transplantations of Bone, &c." (*Proc. Roy. Soc.*, May 1881, and *Comptes rendus* Acad. Sci., Paris, June 1881); "Treatise on Osteotomy" (London, 1880; translated into French, German, Italian, Roumanian, Swedish and Russian); "Osteogenic Factors in the Development and Repair of Bone" (*Annals of Surgery*, 1887); Address on the Surgery of the Brain and Spinal Cord (*Lancet*, and *Brit. Med. Journ.*, 1888); "The Pupil in its Semiological Aspects" (*Internat. Journ. of Med. Sciences*, 1887); "Radical Cure of Hernia" (*Annals of Surgery*, 1886); also numerous articles on special points in Surgery.

Supplementary Certificate. Author of a treatise on Pyogenic Infective Diseases of the Brain and Spinal Cord (1893); an Atlas of Head Sections, with fifty-three copper plates, fifty-three key plates and descriptive text (1893). Especially distinguished for his work on the Surgery of the Bones and in the Development and Practice of the Surgery of the Brain and Spinal Cord.

SIDNEY MARTIN,

M.D., B.S., B.Sc., F.R.C.P. Assistant Physician, University College Hospital, and Hospital for Consumption, Brompton. Distinguished for researches in chemical physiology and pathology; has carried out researches on chemical bacteriology for the Local Government Board, and for the Royal Commission on Tuberculosis. The following are his principal published papers:—"Papain Digestion" (*Journ. of Physiol.*, v.); "Nature of Papain and its action on Vegetable Proteids" (*ibid.*, vi.); "The Proteids of the Seeds of *Abrus precatorius*" (*Proc. Roy. Soc.*, xlii.); "Physiological Action of the Active Principle of *Abrus precatorius*" (*ibid.*, xlvi.); "The Toxic Action of the Albumose from the Seeds of *Abrus precatorius*" (*ibid.*); "Gluten and the Proteids of Flour" (*Brit. Med. Journ.*, 1886); "The Influence of Bile on Digestion" (with Dr. D. Williams—*Proc. Roy. Soc.*, xlv. and xlviii.); "The Chemical Products of the Growth of *Bacillus anthracis* and their Physiological Action" (*ibid.*, xlviii.); "Preliminary Report on the Chemical Products of the Life of *Bacillus anthracis*" (Rept. of the Med. Officer, Local Govt. Board, 1889–90); "Chemical Pathology of Anthrax" (*ibid.*, 1891); "Diphtheritic Paralysis" (*Proc. Roy. Soc.*, 1892); "Gulstonian Lectures on the Chemical Pathology of Diphtheria compared with that of Anthrax, Infective Endocarditis and Tetanus," 1892; "Two Classes of Vegetable Globulins" (*Proc. Physiol. Soc.*); "Pathology of the Proteids of the Body" (*Brit. Med. Journ.*, 1890).

GEORGE M. MINCHIN,

M.A. (Dubl.), Professor of Mathematics in the Royal Indian Engineering College, Cooper's Hill. Author of the following treatises: "Statics," "Uniplanar Kinematics," and "Hydro-

statics." Also of the following papers:—"Astatic Equilibrium of any System of Forces, treated by Quaternions" (*Proc. Lond. Math. Soc.*); "The Absolute Sine Electrometer" (*Nature*, *Electrical Review*, &c.); "Researches in Photo-electricity" (*Proc. Phys. Soc.* and *Phil. Mag.*): "Impulsion Cells" (*Electrician*, *Proc. Phys. Soc.*); "Seleno-Aluminium Cells and the Electromotive Forces of Starlight" (*Astronomy and Astro-Physics*); "The Magnetic Field of a Circular Current"; "The Magnetic Field close to the Surface of a Wire carrying a Current" (*Phil. Mag.*, *Proc. Phys. Soc.*).

WILLIAM HENRY POWER,

Assistant Medical Officer, H.M. Local Government Board. Author of Reports to the Local Government Board relating to the natural history of epidemic diseases and materially extending the knowledge thereof, more especially (*a*) Demonstration in 1882 of the existence of Scarlatinal Disease in Cows, explaining the previously obscure spread of Scarlatina in human communities by means of Cow's Milk; (*b*) Record of Cases (afterwards followed by Dr. Klein) where Diphtheria had been spread by the consumption of Cow's Milk; (*c*) Discovery, in 1881, of the ability of Smallpox to extend atmospherically (without other personal relation) from a hospital to houses in its neighbourhood. The subject was investigated by a Royal Commission which recognised the facts; they have been subjected to further demonstration by Mr. Power during subsequent years.

THOMAS PURDIE,

B.Sc., Ph.D., A.R.S.M., Professor of Chemistry in the University of St. Andrews. Author of the following:—"On the Synthesis of α Isoheptane"; and "On the Action of Sodium Alcoholates on Fumaric Ethers" (*Trans. Chem. Soc.*, 1881); "Action of Sodium Alkyl Oxides on Ethereal Fumarates" (*ibid.*, 1885); "The Action of Metallic Alkylates on Mixtures of Ethereal Salts with Alcohols" (*ibid.*, 1887). Joint author with W. Marshall, B.Sc., of:—"Action of Alcohols on Ethereal Salts in presence of Small Quantities of Sodic Alkylates" (*Trans. Chem. Soc.*, 1888); "The Addition of the Elements of Alcohol to the Ethereal Salts of Unsaturated Acids" (*ibid.*, 1891). Joint author with J. Wallace Walker, M.A., of:—"Resolution of Lactic Acid into its Optically Active Components" (*ibid.*, 1892); "Optically Active Ethoxysuccinic Acid" (*ibid.*, 1893).

APRIL METEORS.

COMPARATIVELY few meteors of the April shower appear to have been seen this year in consequence of the cloudy weather which prevailed. But if the results are scanty they are interesting, for three fine meteors were observed at more than one station, and their real paths in the atmosphere have been computed.

On April 14, 11h. 44m., a bright first mag. meteor was seen by Prof. A. S. Herschel at Slough, and by the writer at Bristol. It moved rapidly in a rather long path, and left a bright streak. The radiant point is indicated at $316° + 31°$ near ζ Cygni, and the meteor fell from 87 to 71 miles over the English Channel. During its visible career it traversed a course of 107 miles with a velocity of about 49 miles per second. The radiant of this meteor near ζ Cygni is almost identical with that ($314° + 27°$) found for a 1–2 mag. meteor observed on April 20, 1893, also by Prof. Herschel and the writer.

On April 19, 10h. 59m., a fine meteor, variously estimated as = 1st mag., 2 × ♃, = ♀, = 1st mag., was observed by Mr. Corder at Bridgwater, Mr. Blakeley, Dewsbury, Mr. Packer, Birmingham, and the writer at Bristol, respectively. Its motion was moderately slow, and it left a streak. The direction of its flight shows it to have been a Lyrid with a radiant at $269° + 30°$. The meteor descended from 91 to 43 miles over the North Sea and Lincolnshire, and traversed a path of 97 miles with a velocity of 33 miles per second. This object appeared much brighter to the observers at Birmingham and Dewsbury than to those at Bridgwater and Bristol, for the meteor was far more distant from the latter places, and its light much veiled in the mist lying over the stars of Cygnus near the north-east horizon.

On April 19, 11h. 46m., another conspicuous meteor, moving very swiftly, and leaving a bright streak, was seen in Hercules and Boötes by Mr. Corder at Bridgwater, and the writer at Bristol. Its radiant was in Sagitta at $300° + 20°$. The meteor fell from 77 to 71 miles over Wiltshire and Somerset, and travelled along a path of 40 miles in less than one second of time. The radiant in Sagitta furnishes a well-defined meteor shower at the April epoch, and I first detected it in 1877. My positions for the radiant are as follow:

D, 92 ...	1877, April 16–19 ...	$298 + 25$	6 meteors
D, 110 ...	1885, April 18–20 ...	$299 + 24$	5	,,
D, 121 ...	1887, April 19–25 ...	$302 + 23$	4	,,

The mean position is at $300° + 24°$. Mr. Corder saw a shower in April–May 1876–9 from $300° + 20°$ (7 meteors), which presents an excellent accordance. The meteors of this stream are very swift, and commonly germinate streaks; but the shower is not well displayed until the morning hours, the radiant being very low before midnight. W. F. DENNING.

NOTES.

THE following fifteen candidates were selected on Thursday last by the Council of the Royal Society, to be recommended for election into the Society:—Mr. J. Wolfe Barry, Prof. A. G. Bourne, Mr. G. H. Bryan, Mr. J. Eliot, Prof. J. R. Green, Mr. E. H. Griffiths, Mr. C. T. Heycock, Prof. S. J. Hickson, Major H. C. L. Holden, Mr. F. McClean, Prof. W. MacEwen, Dr. S. Martin, Prof. G. M. Minchin, Mr. W. H. Power, Prof. T. Purdie. We give the qualifications of the candidates in another part of this number.

THE memorial of the late Prof. J. C. Adams, at Westminster Abbey, will be unveiled this afternoon by the Duke of Devonshire.

WE are glad to be able to report that Prof. Huxley has been steadily improving in health during the past few days.

DR. P. DANGEARD has been appointed Professor of Botany to the Faculty of Sciences at Poitiers.

AT a meeting of the Court of the Spectacle Makers' Company, on Thursday last, Mr. W. H. M. Christie, the Astronomer Royal, was presented with the honorary freedom of the Company, in recognition of his services to astronomical science.

THE De Candolle prizes have been awarded by the Physical and Natural History Society of Geneva to Dr. O. Warburg for his monograph of the *Myristicaceæ*, and to Dr. R. von Wettstein for his monograph of the genus *Euphrasia*.

DURING the past week, the deaths of several eminent men of science have occurred. Surgeon-Major Carter, who was elected a Fellow of the Royal Society in 1859, and obtained the Royal Medal in 1872, died on Saturday last, the 4th inst., at his residence in Budleigh Salterton. We notice also the death of Mr. A. E. Durham, late Vice-President of the Royal College of Surgeons of England, and the author of numerous works on subjects connected with medicine and surgery. Among the announcements of deaths abroad, we regret to see the name of Prof. K. Ludwig, Professor of Physiology in the University of Leipzig, and Director of the Physiological Institute there. He was seventy-eight years of age. The death is also announced of Prof. Manuel Pinheiro Chagas, General Secretary of the Royal Academy of Sciences at Lisbon. Prof. Chagas was born November 13, 1842.

Dr. Karl Vogt, the eminent biologist, died at Geneva on Monday, at seventy-eight years of age. He was born at Giessen, and studied under Liebig and Agassiz. After residing for a time in Paris, he returned to Germany, in 1847, as Professor of Zoology in the University of his native town, but soon lost his chair for political reasons. In 1852 he became Professor of Geology at Geneva, and from that time identified himself with the civic life of the country of his adoption.

We regret to notice that Sir George Buchanan, formerly medical officer to the Local Government Board, died on Sunday last, at the age of sixty-four. As mentioned in these columns last week, he was chairman of the Royal Commission on Tuberculosis, the report of which has just been published. His contributions to the literature of preventive medicine, hygiene, and sanitation are numerous and of prime importance. He was elected a Fellow of the Royal Society in 1882.

On Monday, May 20, a meeting will be held at the Royal Geographical Society to commemorate the fiftieth anniversary of the sailing of the Arctic Expedition, under Sir John Franklin. The Society's anniversary meeting and the annual conversazione will be held on the following Monday, May 27.

The Earl of Selborne, whose death occurred on Saturday last, was elected a Fellow of the Royal Society in 1860. He was raised to the peerage as Baron Selborne in 1872. The little Hampshire village, from which the title was derived, is that which is immortalised by Gilbert White's "Natural History."

The Department of Science and Art has received, through the Foreign Office, a programme of an Exhibition of Medicinal and Useful Plants, which is to be held at the Hague in July next. Intending exhibitors may obtain further information from Dr. M. J. Greshoff, 97 Laan van Meerdervoort, at the Hague.

Sixty-six natives, and as many as 252 animals, have been brought over from Somaliland by Herr Menges, for the East African Village at Sydenham. Among the animals was a "Waller" antelope, and numerous lions, cheetahs, hyenas, jackals, baboons, and ostriches. A further instalment of twenty lions, eleven elephants, four zebras, nineteen ostriches, six leopards, four pythons, and other animals will shortly arrive.

An International Health Exhibition is to be opened in Paris in a few days, and is to remain open until September 15 next. The exhibits are divided into ten groups, as follow:—(1) Hygiene of the house; (2) the health of towns; (3) treatment of infectious diseases; (4) demography and sanitary statistics; (5) sanitary science; (6) hygiene of infancy; (7) industrial and professional hygiene; (8) food products; (9) the hygiene of clothing—laundry work, sanitary clothing, &c.; (10) physical exercise.

A course of lectures on "Our Edible Sea Fish and the Sea Fisheries," to be delivered by Prof. W. A. Herdman, F.R.S., at University College, Liverpool, has been arranged by the Lancashire Sea Fisheries Joint Committee. The object of the lectures is to interest and inform the general public in a matter of national importance, viz. the present position and future prospects of our fisheries, the need of protection and regulation, and the benefits which may be expected to result from such operations and from fish-hatching and shell fish culture.

The library of the Marine Biological Association's laboratory at Plymouth is in want of a number of volumes to complete sets of those books which form an essential part of the equipment of an institution where scientific investigation is carried on. Among the volumes badly needed are: *Philosophical Transactions* previous to 1878, and the *Proceedings* of the Royal Society previous to 1888. Fellows of the Royal Society, who do not wish to keep their old *Transactions* and *Proceedings*, or the families of Fellows who are dead, could not bestow those volumes more worthily than by giving them to the Plymouth Laboratory. Other volumes which would be welcomed are: *Proceedings* of the Zoological Society previous to 1891, and the *Zeitschrift für Wissensch Zoologie* previous to 1875. Any special monographs on biological subjects, or separate copies of papers, would also be gladly received. Every man of science knows that the literature of a subject should be easy accessible to an investigator, and will therefore recognise the necessity of making the library at Plymouth less deficient in works of reference than it is at present.

We gave last week a list of the new officers of the U.S. National Academy of Sciences, elected at the recent annual meeting. The new members elected at the same meeting were—Dr. William H. Welch of Johns Hopkins University, Dr. William L. Elkin of Yale University, Prof. Charles S. Sargent of Harvard University, and Prof. Charles Whitman of Chicago University. Three foreign associates were chosen—Prof. Rudolph Leuckart of the University of Leipzig, Prof. Julius von Sachs of Wurzburg, and Prof. Sophus Lie, of Leipzig. The Barnard gold medal was voted to Lord Rayleigh for the discovery of argon. The Watson medal and a purse of 100 dollars was presented to Prof. L. C. Chandler for his researches on the variation of latitude and on the variable stars. An account of this award was given in Nature a year ago (vol. 50, p. 157). A list of the papers read at the meeting will be found among our Reports of Societies. The Academy selected Philadelphia as the place for the autumn meeting, and fixed the date at October 29. At that meeting the new president, Prof. Wolcott Gibbs, will be inducted into office, and Prof. O. C. Marsh's term of office will terminate.

A new era of cheap telephoning seems to have followed the expiration of certain patents and the judicial annulment of others in the United States a few months ago. Simultaneous announcements of reduced rates in Connecticut and Illinois coincide with the formation of a new company—the Standard Telephone Company—with ramifications or sub-companies extending all over the United States, and an aggregate capital of 160,000,000 dollars. Preliminary arrangements were very quietly made, but this company now comes forward with rates of 3 dollars a month, instead of many times that amount now charged, in some cases running as high as 240 dollars a year. Efforts have been made, to induce the legislature of the State of New York, to secure a compulsory reduction of rates; but the old companies have opposed such legislation strenuously, on the ground that no cheaper service could be given. The Standard Company, however, claim to have discovered a new principle or method of operating in electricity, which will enable them to converse over unprecedented distances—say from New York to Denver, or even San Francisco—at very moderate cost. The reticence maintained, however, makes it impossible to decide whether or not these extravagant claims are well-grounded.

At the second International Zoological Congress held in Moscow in 1892, a resolution was passed to the effect that the third meeting should take place in Leyden, the oldest University of the Netherlands, and that Dr. F. A. Jentink, Director of the Leyden Natural History Museum, should be its President. A circular informs us that the Netherlands' Zoological Society is making the necessary arrangements for this meeting, which is to be held on September 16–21, under the patronage of the Queen-Regent of the Netherlands. The Ministers of the Interior, of the Public Works, and of Commerce and Industry, will be Honorary Presidents of the Congress. A number of well-known

zoologists have promised to attend the meeting, and to deliver addresses or read papers. The following scheme for the sectional meetings has been arranged :—(1) General zoology ; geographical distribution, including the fossil faunas ; the theory of evolution. (2) Classification of living and extinct vertebrates ; bionomy ; geographical distribution, including fossil vertebrates. (3) Comparative anatomy of living and extinct vertebrates ; embryology. (4) Classification of living and extinct invertebrate animals ; bionomy. (5) Entomology. (6) Comparative anatomy and embryology of invertebrate animals. Intending members may send the subscription (£1) to Dr. P. P. C. Hoek (Helder), the General Secretary, or to Dr. R. Horst (Leyden), Treasurer.

The summer meetings of the Institution of Naval Architects will be held in Paris on Tuesday, June 11, and during the remainder of the week. The Right Hon. Lord Brassey, K.C.B., President of the Institution, will occupy the chair. We are informed that the French Government is taking a warm interest in these meetings, and that, under the honorary presidency of the Minister of Marine, Vice-Admiral Besnard, and under the acting presidency of Vice-Admiral Charles Duperré, a strong and influential Reception Committee has been formed, representing the Ministry of Marine, the French Navy, the Municipality of Paris, the Chamber of Commerce of Paris, the Great French Industries and Steamship Owners, the Railroad Companies, the University of Paris, the Conservatoire des Arts et des Métiers, the French Institution of Civil Engineers, the Society for the Encouragement of National Industry, the French Institution of Naval Architects, and the Union of Yachts. This Committee has already taken active steps to draw up a programme of exceptional interest for the instruction and entertainment of the Institution. Papers have already been promised by M. Émil Bertin, Director of the French Government School of Naval Architecture, and M. V. Daymard. There will also be papers by Sir William White, Mr. B. Martell, Dr. Francis Elgar, Mr. Archibald Denny, and Mr. Mark Robinson.

During the Easter vacation the following naturalists have been at work in the Liverpool Marine Biological Station at Port Erin :—Dr. H. O. Forbes, Mr. F. G. Baily, Mr. P. M. C. Kermode, Dr. J. D. Gilchrist (Edinburgh University), Mr. A. O. Walker, Prof. Herdman, and Mr. J. C. Sumner (curator). Two steamer dredging expeditions have been carried out to the west and south of the Isle of Man. On these a small shank trawl was worked, in addition to the dredge, with considerable advantage on one occasion, in fact, coming up so full that the net burst with the weight on leaving the water, and the contents were lost. A number of fine Echinoderms were obtained with the trawl, including *Luidea*, *Palmipes*, *Porania*, *Stichaster*, *Synapta*, and other Holothurians. Amongst the Crustacea were *Scalpellum*, *Munida bamffica*, *Xantho tuberculata*, *Ebalia tuberosa* and *E. tumefacta*, *Anapagurus hyndmanni*, *Galathea dispersa* with *Pleurocrypta dispersæ*, *Melphidipella macera*, and a number of the rare shrimp *Pontophilus spinosus*, Leach. Floating fish eggs (plaice and another species) were caught in the tow-nets in Port Erin Bay, both in March and April ; and *Aplysia*, *Doris*, *Sepiola*, and other Invertebrates have spawned in the tanks at the Biological Station. The Liverpool Committee is at present considering the possibility of a further extension of the Station in the form of a hatchery and a large tidal pond, such as was contemplated in Prof. Herdman's original scheme of the institution.

An Italian Seismological Society has recently been founded by Prof. Tacchini, the well-known Director of the Central Meteorological and Geodynamic Office at Rome. Its objects are to make known as soon as possible all the seismic and volcanic phenomena occurring either in Italy or in other countries, to publish short notes about them, descriptions of seismic apparatus, &c., and generally to promote the study of geodynamics. The subscription being moderate, and national and foreign members being admitted on nearly the same terms, the new society, it to be hoped, may become practically a European one.

A useful innovation, that we hope is to be continued, has been started by the Geological Society of London, in the publication of a catalogue of geological literature added to the Society's library during the half-year ended December 1894. This is equivalent to a list of all important books and papers on geology published in that period. Every paper is catalogued separately, under the author's name, and there is a subject-index. The whole is a distinct improvement on the list hitherto published annually in the November *Quarterly Journal* ; and in spite of the improvements, this list for the half-year is less than half the bulk of the last annual one. The only important omission is that of maps. The work will be most useful to all geologists who wish to keep abreast of recently published works.

The science of oscillations has been enriched by some simple and instructive elementary experiments, due to Dr. H. J. Oosting, which are described in the *Zeitschrift für den Physikalischen Unterricht*. That the velocity of a pendulum is greatest when the bob reaches its mean position is shown by means of a pendulum with a mirror attached to it at its axis of suspension, the upper end of the pendulum-rod being attached to a stout wire bridge, the feet of which take the place of the knife edge. When a beam of light is reflected from this mirror, a line of light is formed upon the screen if the pendulum vibrates rapidly enough. The light from the lamp is made intermittent by a uniformly revolving disc provided with holes bored at equal intervals near the edge. A series of points are then produced on the screen, which are crowded together towards the ends, and further apart towards the middle of the line of light, the distance being proportioned to the velocity of the bob.

Another neat contrivance designed by the Dutch physicist is one for producing Lissajou's curves resulting from the combination of two vibrations at right angles to each other. The simplest form of vibrating mirrors consists of two small mirrors attached to wires stretched in a vertical and horizontal position respectively. The periods of vibration are adjusted by screws carrying nuts mounted behind the mirror at right angles to the wire. The vibration is made slower by screwing the nuts outwards ; or, if a pendulum is to be used, it is attached to the bottom of a U-shaped wire bent out and down at the upper ends, so as to oscillate about the ends of the wire. A horizontal circle is attached to the U at the centre of suspension, carrying a precisely similar suspension for a second and smaller pendulum, except that a horizontal mirror takes the place of the horizontal circle. The periods are adjusted by weights movable along the rods, and the resulting curves may be thrown upon the ceiling, or back upon a screen just in front of the lantern with a hole for letting the light through. In this case the beam must be twice reflected from a mirror at 45° to the horizon.

Within the last year or two, the number of methods for observing the characteristics of an alternating current which have been described is considerable. The latest step in this direction is due to M. J. Pionchon (*Comptes rendus*, April 22, 1895), who uses an optical method. The alternating current is passed through a coil, surrounding a tube filled with carbon bisulphide or a saturated solution of mercuric and potassium iodides. This tube is placed between the polariser and analyser of a half-shadow polarimeter. Under these circumstances the plane of polarisation of the light, after its passage through the tube, passes in succession through all the positions between two limits, one of which corresponds to the maximum current in one direction, and the other to the maximum current in the opposite direction.

If, as is the case in practice, the alternations are fairly rapid, the appearance presented is that during the passage of the current the two halves of the field appear equally bright when the analyser is adjusted in the zero position for no current passing. By adopting the stroboscopic method of observation, the author has, however, succeeded in making clear the different phases of illumination through which the field of view passes. By suitably adjusting the difference (ϵ) between the period (T') of the stroboscope and the period (T) of the current, it is possible to see the various phases of the phenomenon pass as slowly as is desirable, the period of the apparent change being to the period of the current in the ratio of T' to ϵ. Hence, by determining the time taken to go through a whole cycle of the apparent changes, the period of the current can be deduced. The maximum value of the current can also be determined. If we denote by μ the rotation of the plane of polarisation of the light corresponding to the maximum current, then, when the principal plane of the analyser is rotated through a less angle than μ, the two halves of the field will appear equally bright twice during each cycle of the apparent changes. If the angle of rotation of the analyser is μ, this equality will only occur once in a cycle, while if the rotation is greater than μ, at no time will the two halves of the field of view appear equally bright. Thus it is quite easy to determine the position of the analyser corresponding to the maximum current. The method also admits of obtaining the current curve, by noting the times at which, when the angle of rotation of the analyser (α) is less than μ, the two halves of the field are equally bright. The current corresponding to the two times observed can be calculated from the value if α, the known dimensions of the coil, and Verdet's constant for the liquid employed.

Prof. Frank Clowes' "Treatise on Practical Chemistry and Qualitative Analysis," adapted for use in the laboratories of colleges and schools, has reached a sixth edition. Messrs. J. and A. Churchill are the publishers of the book.

The *Quarterly Journal* of the Geological Society, just issued (No. 202), contains, in addition to papers read at the meetings, the report of the proceedings of the annual meeting and the anniversary address of the president, Dr. Henry Woodward, on "Some Points in the Life-history of the Crustacea in Early Palæozoic Times."

The very useful pamphlet entitled "Notes on Polarised Light," by Mr. A. E. Munby, which we favourably noticed when it appeared about a year ago, has been translated into Russian by Prof. Glinka, of St. Petersburg University. Students of mineralogy beginning work with the polariscope, will find the contents of the pamphlet of great assistance.

We have received a report of the proceedings of the conference on inland navigation, held in Birmingham in February, by the Federated Institution of Mining Engineers. The report contains some useful information on the important subject of the inland navigation of Great Britain, and a number of valuable suggestions for improving the present inefficient state of our inland waterways.

Messrs. Dulau and Co. have prepared and published a useful catalogue of separate papers from the *Philosophical Transactions* of the Royal Society offered for sale by them. The papers are indexed according to the authors' names. Two other new catalogues which scientific bibliographers will find valuable are R. Friedländer and Son's "Bücher-verzeichniss" (No. 417), containing titles of entomological works, and a list of books issued by Mr. Bernard Quaritch, Piccadilly, S.W.

Science Gossip for May contains several articles of scientific interest. Dr. Dallinger has a note on *Melicerta ringens*, illustrated by drawings of this small though interesting denizen of our ponds. Messrs. Wanklyn and Cooper write on Argon. Mr. Thomas Leighton has an article on "Geology of the Isle of Wight"; and Dr. Guppy writes on "Stations of Plants and Buoyancy of Seeds." Mr. Rudolph Beer has an interesting illustrated article on "Leguminous Plants."

The West Australian Year-Book for 1893–94, issued by the Registrar-General, contains tables showing the results of meteorological observations at the chief observing stations, together with some general remarks on the climate of the colony. The climate varies a good deal in the different parts; in the south and south-west it is excellent, being temperate and cool, with regular and sufficient rainfall. To the eastward the climate is dryer, but little accurate information is available in that direction.

The Report of the Royal Zoological Society of Ireland for the year 1894, shows that the Society is in an exceedingly prosperous condition. Nine lion cubs were born during the year, four of which died, but the five others (all males) were disposed of as exchanges. There are still two lions and five lionesses in the gardens of the Society. The Council has decided to make a donation to the funds of the *Irish Naturalist*, a monthly journal which frequently contains valuable information on the natural history of Ireland.

We have received No. 2 of the Official Guide to the Museums of Economic Botany at the Royal Gardens, Kew, comprising Monocotyledons and Cryptogams. Among the specimens and products belonging to Monocotyledons, by far the larger number are naturally derived from the great order of palms; though the origin is also illustrated of other very important products, such as vanilla, ginger, grains of paradise, arrowroot, the pine-apple, aloes, bananas, the yam, New Zealand hemp, dragon's-blood, and many others. The palms include nearly 100 distinct exhibits, and the grasses upwards of 60. Among Cryptogams, several officinal and other useful articles are obtained from the fibres; while the Algæ and Fungi also yield their quota. A very copious index adds greatly to the value of this publication.

A report, lately issued, on the progress and development of the Manchester Museum, Owens College, during the past four years, shows that the museum is a great power for good. By means of short courses of popular lectures, and informal demonstrations and addresses, the collections have been rendered more interesting and intelligible to the public. Clubs, societies, and classes have paid frequent visits of inspection, and have had the contents of various sections of the museum explained to them by Prof. Boyd Dawkins, or by members of the museum staff. A number of additions have been made in the geological department, one of the most interesting accessions being a model of a glacier, made to scale by Prof. Heim. The zoological and botanical collections have also been benefited by additions, and the specimens in most of the sections have been reduced to law and order.

A recent redetermination of the atomic weight of strontium, by T. W. Richards, confirms the value 87·70 found by Pelouze in 1845. Pelouze employed the method founded on a comparison of anhydrous strontium chloride and silver. The present author finds (1) the ratio between very carefully purified anhydrous strontium bromide and silver in three sets of analyses carried out by different methods, and (2) the ratio $2AgBr : SrBr_2$ in two other series of experiments. Taking oxygen = 16·000, the values obtained for the atomic weight of strontium are respectively (1) 87·644; 87·663; 87·668, and (2) 87·660; 87·650. The mean value from these results may be taken as 87·66.

The additions to the Zoological Society's Gardens during the past week include a Common Squirrel (*Sciurus vulgaris*),

British, presented by Mrs. Herbert Morris; four Yellow-bellied Liothrix (*Liothrix lutens*) from China, presented by Mr. Albert Kettich; a Black-billed Sheathbill (*Chionis minor*), captured at sea, a Water Rail (*Rallus aquaticus*), British, presented by Mr. John Gunn; a Lineolated Parrakeet (*Bolborhynchus lineolatus*) from Mexico, presented by Mr. Edward Hawkins; a Puff Adder (*Vipera arietans*) from South Africa, presented by Mr. J. E. Matcham; a Lear's Macaw (*Ara leari*) from South America, four White-backed Pigeons (*Columba leuconota*) from the Himalayas, a Rock-hopper Penguin (*Eudyptes chrysocome*) from New Zealand, deposited; two Alpine Choughs (*Pyrrhocorax alpinus*), European, purchased; an English Wild Cow (*Bos taurus*, var.), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Relative Densities of Terrestrial Planets.—Attention is drawn to an interesting relation between the diameters and densities of the terrestrial planets, by E. S. Wheeler (*Science*, April 19). The planets are plotted with their diameters in miles as abscissæ, and their densities (the earth being taken as unity) as ordinates, and it is then seen that the points located in this way lie approximately in a straight line. Such a line passes within the limits of the probable errors of all except Venus. If this relation should prove to represent a natural law, the mass of a planet or satellite could be determined from its diameter. Venus is the only one of the five planets (the moon being included) that is any more discrepant than might be expected from its probable error; to make it accordant, either its mass must be increased by one-tenth, or its diameter decreased by one-thirtieth. A sufficient increase in the mass of Venus is stated to be all that is necessary to explain the movement of the perihelion point of the orbit of Mercury; but some of the irregularities of Mercury may be accounted for by the small mass which it is now supposed to have, namely, one-thirtieth that of the earth. In plotting the planetary curve, the density of Mercury adopted, is that derived by Backlund from a discussion of the movements of Encke's comet.

The Orbit of Comet 1893 IV (Brooks).—An investigation of the path of this comet, by Signor Peyra, seems to suggest that it is one of a series travelling in the same elliptic orbit (*Ast. Nach.* No. 3281). This conclusion is based on the similarity of the orbit with those of comets 1864 I and 1822 I, the periods of the comets rendering actual identity impossible. The elements of the orbit are as follows:

T = 1893 Sept. 19·25954 Berlin M.T.

Longitude of perihelion	162° 22′ 19″	1893
,, ,, node	... 174 55 12	1893
Inclination ...	... 129 50 14	1893
Eccentricity ...	... 0·9964886	
Log q	... 9·909551	
Period	... 3516 years.	

The Spectrum of Mars.—A very practical contribution to the recent discussion as to the spectroscopic indications of aqueous vapour in the atmosphere of Mars is afforded by the investigations of Mr. Jewell as to the amount of vapour necessary to produce effects which can be observed with instruments of specified power. (*Astrophysical Journal*, April.) Expressing the amount of vapour present in the air of Baltimore by the depth in inches of a layer of water, the observed monthly mean for January is 0·73, June 3·25, October 1·56, the maximum occurring in June. He concludes that "unless the amount of water in the atmosphere of Mars is greater than that in the earth's atmosphere in October at Baltimore, it is useless to look for the presence of water vapour in the spectrum of Mars, unless our instrumental means are much superior to any hitherto used for that purpose." Since instruments of greater dispersion are unsuitable, because of the lack of sufficient light, there seems but little chance of obtaining any very decisive direct evidence of the presence of water vapour in Mars. It will be remembered that Dr. Janssen and others satisfied themselves as to the indications of water vapour bands in the spectrum of Mars, whilst Prof. Campbell has more recently failed to detect them.

The chances of detecting the presence of oxygen, however, if present, do not seem so hopeless, as the B group is readily seen with small dispersion.

It is also suggested by Mr. Jewell that attempts should be made to observe the chlorophyll bands in the spectrum of the green areas of the planets, since one of the bands is quite strong in the vegetation spectrum.

The Astronomical Society of France.—During the eight years of its existence, this Society has attained a membership of nearly 1000. At the annual meeting held recently, Dr. Janssen was elected president, and M. Camille Flammarion general secretary for the current session. The progress of astronomy in 1894 formed the subject of an address by M. Tisserand, the Director of the Paris Observatory. Among other matters he referred to the reappearance of De Vico's comet (Nature, vol. li. p. 542), which he regarded as further evidence of the fact that, at certain epochs, comets are subject to increases of brightness which they are incapable of maintaining, the increased activity being probably due to internal disturbances, the nature of which are not yet understood. It will be remembered, however, that Mr. Lockyer explains these fluctuations in brilliancy by collisions with meteor-swarms lying in the track of the comet. Referring to minor planets, M. Tisserand believed it not improbable that those appearing as bright as 12th magnitude stars have an average diameter of about 130 kilometres; that is, about one-hundredth of the earth's diameter; at that rate, even a thousand of them would not have a total mass equal to a thousandth part that of the earth, assuming that their mean density is not greater than that of the earth. (*Bull. Mens. Soc. Ast. de France*, May.)

THE ROYAL SOCIETY CONVERSAZIONE.

THE annual Royal Society conversazione, to which gentlemen only are invited, was held in the Society's rooms on Wednesday of last week.

Many branches of science were represented in the exhibits, either by apparatus or by results of research. An exhibit that attracted much attention was the electrical furnace as used for the melting of chromium, titanium, platinum, and other metals, with high melting-points, shown by Prof. Roberts-Austen, C.B. The furnace consisted of a fire-clay case lined with magnesia, and contained a magnesia crucible. The carbon poles were horizontal, the arc being deflected by means of a magnet on to the material to be heated. For purposes of exhibition, an image of the molten contents of the furnace was projected, by means of a lens and mirror, on to a screen; the current employed is usually about 60 or 70 amperes at 100 volts.

Some very valuable metals of the platinum group were exhibited by Messrs. Johnson, Matthey, and Co., among them being a platinum nugget, weighing 158 ozs.; palladium ingot, of 1000 ozs.; rhodium ingot, 72 ozs.; osmium, melted and sponge; ruthenium melted by the electric arc; and pure iridium rolled sheet.

A magnet, showing the effects of currents in iron on its magnetisation, was exhibited by Dr. Hopkinson. A large electromagnet had buried in its substance two coils of comparatively small dimensions, one around the centre of the magnet, the other half-way between the centre and the surface. These coils were connected to two galvanometers. On reversing the current round the magnet it was seen that a considerable time elapsed before either galvanometer showed any substantial current, and that the current in the central coil occurred much later than in that at a less depth in the mass of iron.

Prof. J. A. Fleming showed a synchronising alternating current motor and contact maker, for the delineation of the form of alternating current and electromotive force curves, and a form of resistance of small inductance for use with the apparatus.

An instrument for analysing primary and secondary volts and amperes simultaneously was exhibited by Prof. W. M. Hicks.

Mr. R. E. Crompton had on view electrically heated apparatus, showing the method of applying electricity for heating tools and appliances used in trade; also for domestic purposes. Wires of high resistance composed of nickel, steel, or other suitable alloys, were embedded in an insulating enamel, and by it attached to the various articles to be heated. By this means loss of heat was obviated. Connection was made with the circuit by means of safety connectors, in which the contacts were automatically protected. The perfect flexibility of the system was exemplified in the electric oven, which was heated on all sides top, and bottom, and the temperature of which could be regulated

by turning on or off any part, or the whole of the current. Electrically heated hot-plates, flat-irons, and radiators were shown constructed on the same principle. Mr. Crompton also exhibited the latest form of Crompton potentiometer, for ratio measurements (accuracy 1 in 1,000,000), and simple forms of platinum thermometers for use with potentiometer.

A new instrument for testing the quality of iron in regard to magnetic hysteresis was exhibited by Prof. Ewing (Fig. 1). Its special use is to test sheet-iron for transformers and dynamo armatures. A few strips of the iron to be tested are cut to the length of three inches. These are clamped in a carrier, which is then caused to revolve between the poles of a magnet. The magnet is suspended on a knife-edge, and becomes deflected in consequence of the work expended in overcoming the magnetic hysteresis of the sample. The deflection is observed by means of a pointer, and serves as a measure of the hysteresis. The apparatus is so designed as to make the induction nearly the same in all specimens, notwithstanding differences of permeability. This makes its indications strictly a test of hysteresis.

Mr. L. Pyke showed an arrangement by means of which it is possible to obtain a greater efficiency in the reduction of the highly electro-positive metals from aqueous solutions, into and forming an amalgam with a mercury cathode.

A system of electric meters, viz., voltmeters, ammeters, and wattmeters, suitable for either direct or alternating currents, formed Major Holden's exhibit.

Prof. George Forbes exhibited a torsion model of submarine cable. A thread vertically suspended in oil represented the cable; the torsion (E.M.F.) being applied at the top by vanes and a positive or negative air blast (battery). The whole was suspended at the top by a spring (sending condenser); at the bottom was a mirror to reflect spot of light. This was controlled by a magnet (receiving condenser). Fluid friction represented resistance. Twist represented charge. The model gave signals compared with those of a cable 2000 miles long.

Specimens of the deposit or incrustation on the insulators of the electric light mains at St. Pancras, in which metallic sodium

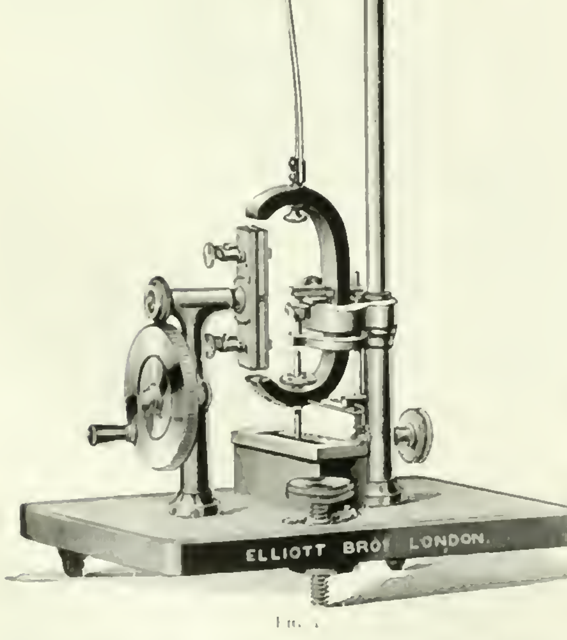


FIG. 1.

and potassium have been found, and of the insulators and wood bearers, which were in use on these mains, were exhibited by Major Cardew, R.E. The deposit was found to have been caused by the passage of alkaline salts in solution to the negative main, the salts being chiefly derived from the neighbouring soil, with which the end fibres of the wood bearers were in contact. Electrolysis of these salts took place with liberation of the metals at the negative main, the metals being oxidised and slowly carbonated in air. During this process nodules of the metal seem to have become embedded in the oxides, and preserved from oxidation.

Mr. Francis Galton showed enlarged finger prints, with descriptive notation, and a print of the hand of a child eighty-six days old.

Prof. J. B. Farmer had on view examples of heterotypical nuclear division in reproductive tissues of plants.

Microscopic specimens illustrating some appearances of nerve-cells were exhibited by Dr. Gustav Mann; and wandering cells of the intestine were shown by Dr. Wesbrook and Mr. W. B. Hardy.

Mr. W. T. Burgess showed the results of experiments in connection with the transmission of infection by flies. Flies having been placed in momentary contact with a cultivation of *Bacillus prodigiosus* (or other suitable chromogenic organism) were allowed to escape into a large room. After some time they were recaptured and caused to walk, for a few seconds, over slices of sterile potatoes, which were then incubated for a few days. The experiments showed that the flies' tracks on the potatoes were marked by vigorous growths of the chromogenic organism, even when the flies spent several hours in constant activity before they were recaptured. The use of pathogenic organisms in these experiments would be attended with obvious dangers, but the results obtained with harmless microbes indicated the constant risks to which flies expose us.

Prof. Gotch and Dr. H. O. Forbes showed a living specimen of the *Malapterurus electricus* from the River Senegal; Mr. Stanley Kent, a new bacterial species; and Mr. D. Sharp, F.R.S., examples of variation in the size of beetles. In some beetles there is great difference in the size of adult individuals of the same species and sex. In one of the cases exhibited—*Brenthus anchorago*—this difference was, in length alone, nearly as five and one. It is believed that these extreme cases occur chiefly in forms in which the males are ornamented with "useless" appendages, *e.g.* the families Scarabæidæ, Lucanidæ, Brenthidæ.

The exhibit of the Marine Biological Association consisted of (1) marine organisms preserved in formic aldehyde, which, in dilute solutions, is specially useful for the preservation of transparent organisms as museum specimens; (2) a new method of fixing methylen-blue preparations. The methylen-blue preparations are fixed with ammonium molybdate. This method, due to Dr. Berthe, of Berlin, has the advantage of retaining the original blue colour of the preparations, and also of allowing the object to be mounted in balsam, or imbedded in paraffin in the usual way; (3) the action of light on the under sides of flat fishes. The flat fishes exhibited were reared in a tank with a flat slate bottom and glass front. Those portions of the under side of a fish which were not in contact with the slate, and to which light was accessible—this point being demonstrated by the exposure of a photographic plate upon which the fish lay—have become pigmented, whilst the remaining portions are without pigment; (4) living representatives of the Plymouth fauna.

A gradient indicator was exhibited by Mr. J. Wimshurst; and Sir Benjamin W. Richardson showed an electrical cabinet, for use in the wards of a hospital.

There were only two astronomical exhibits. Mr. J. Norman Lockyer, C.B., showed an enlargement of a photograph of the spectrum of α Orionis, taken with a 6-inch telescope and an objective prism of 45°.

Mr. Sidney Waters exhibited charts showing the distribution of the nebulæ and star-clusters, and their relation to the Milky Way. These charts, upon which are recorded the position of the 7840 objects of the New General Catalogue of 1888, were designed to show the distribution of the nebulæ and star-clusters, more especially in relation to the Milky Way. The resoluble and irresoluble nebulæ are shown to be most densely scattered in the poles of the galactic circle, and avoid the track of the Milky Way, while the star-clusters follow its course with great fidelity. The evidence derived from this distribution seems to point to some general connection between the nebular system and the system of the stars.

Prof. Ramsay had a spectroscope and Plucker tubes arranged to give ocular demonstration of the spectra of argon extracted from air, and of a mixture of argon and helium extracted from cleveite. It is hardly necessary to say that the spectroscope was in great demand all the evening.

Students' simple apparatus for determining the mechanical equivalent of heat was exhibited by Prof. Ayrton. The apparatus enables the heat equivalent of a watt-second to be experimentally ascertained with an error of less than 1 per cent., without any allowance having to be made for heat lost by conduction, convection, or radiation. It will give the result when 2000 c.c. of water are warmed for two minutes with a current of about 30 amperes, at a pressure of about 10 volts. The conductor consisted of 10 feet of manganin rolled into a thin strip to give off heat rapidly, and formed into a double grid so as to be used as an efficient water stirrer. The cross section of the flexible leads was such that practically no flow of heat occurred between them and the grid when a current of about 30 amperes is used.

Photographs of sections of gold nuggets etched to show crystalline structure, were exhibited by Prof. A. Liversidge. Gold nuggets, on being cut through or sliced and polished, and etched by chlorine water, were found to exhibit well-marked crystalline structure, closely resembling the Widmanstatt figures shown by most metallic meteorites, except that, in the nuggets, the crystals are more or less square in section, and show faces which evidently belong to the octahedron and cube.

Phenomena associated with the formation of cloud were experimentally illustrated by Mr. W. N. Shaw. Clouds formed by mixture of two currents of air of different temperatures were shown in a large glass globe. The currents were due to convection. The motion of the clouds, gave an indication of the motion of the air. Under suitable conditions the motion assumed a gyratory or "cyclonic" character. A second globe was arranged to show the formation of a cloud by the dynamical cooling of air, consequent upon a sudden expansion equivalent to an elevation of about 10,000 feet. The water globules could be seen to fall slowly. A light was arranged at the back of the globe to show (under favourable circumstances) coloured coronæ surrounding a central bright spot. Two other globes were used in conjunction to demonstrate the modification which cloud formation introduces into the dynamical cooling of air. In one of the pair condensation diminished the fall of temperature incidental to sudden expansion, and the difference was indicated by the final pressure-difference between the globes.

There were two barometric exhibits, one a mechanical device for performing temperature corrections in barometers, by Dr. John Shields, and a new form of barometer, exhibited by Dr. J. Norman Collie.

The preparation of acetylene from calcic carbide was shown by Prof. V. B. Lewes. The combustion of acetylene for illuminating purposes attracted great attention. Calcic carbide, formed by the action of carbon on lime at the temperature of the electric furnace, was decomposed by water with evolution of acetylene. The remarkable brilliancy of the flame produced may be judged by the fact that the acetylene when consumed in suitable burners develops an illuminating value of 240 candles per 5 cubic feet of gas.

Generalised frequency curves were exhibited by the Applied Mathematics Department of University College, London, and also compound frequency curves, a harmonic analyser, and a bi-projector.

Mr. T. Clarkson showed his circlographs for drawing and measuring circular curves of any large radius without requiring the centre, with examples of curves. The construction of these instruments is based upon a recent discovery that it is possible to cut a flat plate of steel (of uniform thickness and temper) into a certain form, which imparts to it the property of bending always into circular curves.

Mr. R. Inwards had on view examples of curious mortise joints in carpentry, all made without compression or veneering, and Mr. Hermann Kühne exhibited Junkers' patent calorimeter.

The radial cursor, a new addition to the slide rule, was shown by Mr. F. W. Lanchester. This cursor added to the slide rule makes the rule applicable at once to the calculation of whole or fractional powers, and renders it specially useful for the solution of problems in thermodynamics.

The Cambridge Scientific Instrument Company showed a new form of rocking microtome and a new form of spectrometer, and an improved form of Donkin's harmonograph. This was a modification of Donkin's harmonograph, and draws, on a moving strip of paper, a curve compounded of two simple harmonic motions.

During the evening demonstrations by means of the electric lantern took place in the meeting room.

Prof. A. C. Haddon showed lantern slides illustrating the ethnography of British New Guinea. The slides illustrated the physical characters of different tribes inhabiting British New Guinea, some of the occupations of the people, several kinds of dances, and the distribution of dance-masks. Evidence was given in support of the view that British New Guinea is inhabited by true dark Papuans, and by two distinct lighter Melanesian peoples, one of whom may have come from the New Hebrides, and the other from the Solomon Islands.

Lord Armstrong showed some of the results of his recent experiments on the electric discharge in air. The figures exhibited by means of the lantern, showed various phases, hitherto unobserved, of the brush discharge accompanying the electric spark. They showed also the remarkable modifying effect of induction on the results obtained. The luminous effects were delineated by instantaneous photography, and the mechanical effects by the electric action on dust plates. The spark itself had to be taken in a dark box on a shunt line, as its strong light and violent action would otherwise have been incompatible with the photographic and mechanical methods used in the experiments; but nearly the same tensions were obtained outside the box as within.

THE RARER METALS AND THEIR ALLOYS.[1]

II.

NOW turn to more complex curves taken on one plate by making the sensitised photographic plate seize the critical part of the curve, the range of the swing of the mirror from hot to cold being some sixty feet. The upper curve (Fig. 4) gives the freezing point of bismuth, and you see that surfusion, *a*, is clearly marked, the temperature at which bismuth freezes being 268°. The lower point represents the freezing point of tin, which we know is 231° C., and in it surfusion, *b*, is also clearly marked. The lowest curve of all contains a subordinate point in the cooling curve of standard gold, and this subordinate point, *c*, which you will observe is lower than the freezing point of tin, is caused by the falling out of solution of a small portion of bismuth, which

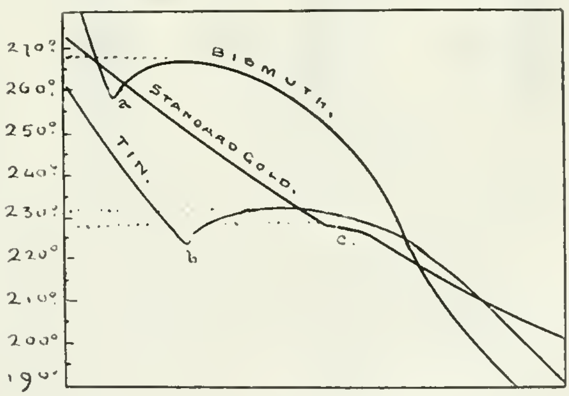


Fig. 4.

alloyed itself with some gold atoms, and "fell out" below the freezing point not only of bismuth itself but of tin. Now gold with a low freezing point in it like this is found to be very brittle, and we are in a fair way to answer the question why $\frac{2}{10}$ per cent of zirconium doubles the strength of gold, while $\frac{2}{10}$ per cent of thallium, another rare metal, halves the strength. In the case of the zirconium the subordinate point is very high up, while in the case of the thallium it is very low down. So far as my experiments have as yet been carried, this seems to be a fact which underlies the whole question of the strength of metals and alloys. If the subordinate point is low, the metal will be weak; if it is high in relation to the main setting point, then the metal will be strong, and the conclusion of the whole matter is this. The rarer metals which demand for their isolation from their oxides either the use of aluminium or the electric arc, never, so far as I can ascertain, produce low freezing points when they are added in small quantities to those metals which are used for constructive purposes. The difficultly fusible rarer metals are never the cause

[1] A Friday evening discourse, delivered at the Royal Institution on March 15, by Prof. Roberts-Austen, C.B., F.R.S. (Continued from p. 18.)

of weakness, but always confer some property which is precious in industrial use. How these rarer metals act, why the small quantities of the added rare metals permeate the molecules, or, it may be the atoms, and strengthen the metallic mass, we do not know; we are only gradually accumulating evidence which is afforded by this very delicate physiological method of investigation.

As regards the actual temperatures represented by points on such curves, it will be remembered that the indications afforded by the recording pyrometer are only relative, and that gold is one of the most suitable metals for enabling a high, fixed point to be determined. There is much trustworthy evidence in favour of the adoption of 1045° as the melting point hitherto accepted for gold. The results of recent work indicate, however, that this is too low, and it may prove to be as high as 1061·7, which is the melting point given by Heycock and Neville[1] in the latest of their admirable series of investigations to which reference was made in my Friday evening lecture of 1891.

It may be well to point to a few instances in which the industrial use of such of the rarer metals, as have been available in sufficient quantity, is made evident. Modern developments in armour-plate and projectiles will occur to many of us at once. This diagram (Fig 5) affords a rapid view of the progress which has been made, and in collecting the materials for it from various sources, I have been aided by Mr. Jenkins. The effect of projectiles of approximately the same weight, when fired with the same velocity against six-inch plates, enables comparative results to be studied, and illustrates the fact that the rivalry between artillerists who design guns, and metallurgists who attempt to produce both impenetrable armour-plates and irresistible projectiles, forms one of the most interesting pages in our national history. When metallic armour was first applied to the sides of war vessels, it was of wrought iron, and proved to be of very great service by absolutely preventing the passage of ordinary cast-iron shot into the interior of the vessel, as was demonstrated during the American Civil War in 1866. It was found to be necessary, in order to pierce the plates, to employ harder and larger projectiles than those then in use, and the chilled cast iron shot with which Colonel Palliser's name is identified proved to be formidable and effective. The point of such a projectile was sufficiently hard to retain its form under impact with the plate, and it was only necessary to impart a moderate velocity to a shot to enable it to pass through the wrought-iron armour (A, Fig. 5).

It soon became evident that in order to resist the attack of such projectiles with a plate of any reasonable thickness, it would be necessary to make the plate harder, so that the point of the projectile should be damaged at the moment of first contact, and the reaction to the blow distributed over a considerable area of the plate. This object could be attained by either using a steel plate in a more or less hardened condition, or by employing a plate with a very hard face of steel, and a less hard but tougher back. The authorities in this country during the decade, 1880–90, had a very high opinion of plates that resisted attack without the development of through-cracks, and this led to the production of the compound plate. The backs of these plates (B, Fig. 5) are of wrought iron, the fronts are of a more or less hard variety of steel, either cast on, or welded on by a layer of steel of an intermediate quality cast between the two plates. Armour-plates of this kind differ in detail, but the principle of their construction is now generally accepted as correct.

Such plates shown by plate B, resisted the attack of large Palliser shells admirably, as when such shells struck the plate they were damaged at their points, and the remainder of the shell was unable to perforate the armour against which it was directed. An increase in the size of the projectiles led, however, to a decrease in the resisting power of the plates, portions of the hard face of which would at times be detached in flakes from the junction of the steel and the iron. An increase in the toughness of the projectiles by a substitution of forged chrome-steel for chilled iron (see lower part of plate B), secured a victory for the shot, which was then enabled to impart its energy to the plate faster than the surface of the plate itself could transmit the energy to the back. The result was that the plate was overcome, as it were, piecemeal; the steel surface was not sufficient to resist the blow itself, and was shattered, leaving the projectile an easy victory over the soft back. The lower part of plate, B (in Fig. 5), represents a similar plate to that used in the *Nettle* trials of 1888.[1] It must not be forgotten in this connection, that the armour of a ship is but little likely to be struck twice by heavy projectiles in the same place, although it might be by smaller ones.

Plates made entirely of steel, on the other hand, were found, prior to 1888, to have a considerable tendency to break up completely when struck by the shot. It was not possible, on that account, to make their faces as hard as those of compound plates; but while they did not resist the Palliser shot nearly so well as the rival compound plate, they offered more effective resistance to steel shot (see lower part of plate C, Fig. 5).

It appears that Berthier recognised, in 1820, the great value of chromium when alloyed with iron; but its use for projectiles, although now general, is of comparatively recent date, and these projectiles now commonly contain from 1·2 to 1·5 per cent. of chromium, and will hold together even when they strike steel plates at a velocity of 2000 feet per second,[2] (see lower part of plate D); and unless the armour-plate is of considerable thickness, such projectiles will even carry bursting charges of explosives through it. [The behaviour of a chromium-steel shell, made by Mr. Hadfield, was dwelt upon, and the shell was exhibited.]

It now remained to be seen what could be done in the way of toughening and hardening the plates so as to resist the chrome steel shot. About the year 1888, very great improvements were made in the production of steel plates. Devices for hardening and tempering plates were ultimately obtained, so that the latter were hard enough throughout their substance to give them the necessary resisting power without such serious cracking as had occurred in previous ones. But in 1889, Mr. Riley exhibited, at the meeting of the Iron and Steel Institute, a thin plate that owed its remarkable toughness to the presence of nickel in the steel. The immediate result of this was that plates could be made to contain more carbon, and hence be harder, without at the same time having increased brittleness; such plates, indeed, could be water hardened and yet not crack.

ATTACK OF 6-INCH ARMOUR-PLATES BY 4·72-INCH SHELLS, WEIGHING 57·2 LBS.

A	B	C	D	E	F
Wrought iron. 1888.	Compound plate. 1888.	Steel. 1888.	Steel. 1894.	Nickel-steel. 1894.	Harveyed, nickel-steel. 1894.

FIG. 5. The upper series of projectiles are Palliser chilled-iron shells, and the lower are chrome-steel. In each case the velocity of the projectile is approximately 1640 foot-seconds, and the energy 1070 foot-tons.

[1] Trans. Chem. Soc., vol. lxvii., 1895, p. 160.

[1] *Proceedings* Institution of Civil Engineers, 1889, vol. xcviii. p. 1, *et seq.*
[2] *Journal* U.S. Artillery, 1894. Vol. [illegible], p. 197

The plate E (Fig. 5) represents the behaviour of nickel-steel armour. It will be seen that it is penetrated to a much less extent than in the former case; at the same time there is entire absence of cracking.

Now as to the hardening processes. Evrard had developed the use of the lead bath in France, while Captain Tressider[1] had perfected the use of the water-jet in England for the purpose of rapidly cooling the heated plates. The principle adopted in the design of the compound plates has been again utilised by Harvey, who places the soft steel or nickel-steel plate in a furnace of suitable construction, and covers it with carbonaceous material such as charcoal, and strongly heats it for a period, which may be as long as 120 hours. This is the old Sheffield process of cementation, and the result is to increase the carbon from 0·35 per cent. in the body of the plate to 0·6 per cent., or even more at the front surface, the increase in the amount of carbon only extending to a depth of two or three inches in the thickest armour.

The carburised face is then "chill-hardened," the result being that the best chrome-steel shot are shattered at the moment of impact, unless they are of very large size as compared with the thickness of the plate. The interesting result was observed lately[2] of shot doing less harm to the plate, and penetrating less, when its velocity was increased beyond a certain value, a result due to a superiority in the power of the face of the plate to transmit energy over that possessed by the projectile, which was itself damaged, when a certain rate was exceeded. At a comparatively low velocity the point of the shot would resist fracture, but the energy of the projectile is not then sufficient to perforate the plate, which would need the attack of a much larger gun firing a projectile at a lower velocity.

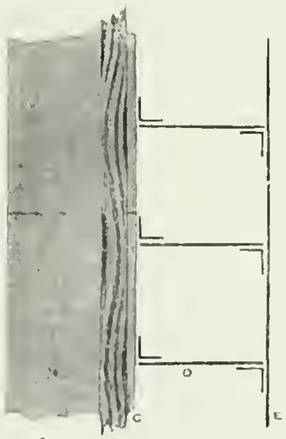


FIG. 6.—Section of Barbette of the *Majestic*.

The tendency to-day is to dispense with nickel, and to use ordinary steel, "Harveyed;"[3] this gives excellent six-inch plates, but there is some difference of opinion as to whether it is advantageous to omit nickel in the case of very thick plates, and the problem is now being worked out by the method of trial. Probably, too, the Harveyed plates will be much improved by judicious forging after the process, as is indicated by some recent work done in America. The use of chromium in the plates may lead to interesting results.

Turn for a moment to the "*Majestic*" class of ships, the construction of which we owe to the genius of Sir William White, to whom I am indebted for a section representing the exact size of the protection afforded to the barbette of the *Majestic*. [This section was exhibited and is shown as reduced to the diagram Fig. 6.] Her armour is of the Harveyed steel, which has hitherto proved singularly resisting to chromium projectiles.

In this section, A represents a 14-inch Harveyed steel armour-plate; B, a 4-inch teak backing; C, a 1½-inch steel plate; D, ½-inch steel frames; and E, ½-inch steel linings.

It will, I trust, have been evident that two of the rarer metals, chromium and nickel, are playing a very important part in our national defences; and if I ever lecture to you again, it may be possible for me to record similar triumphs for molybdenum, titanium, vanadium, and others of these still rarer metals.

Here is another alloy, for which I am indebted to Mr. Hadfield. It is iron alloyed with 25 per cent. of nickel, and Hopkinson has shown that its density is permanently reduced by two per cent. by an exposure to a temperature of −30°, that is the metal expands at this temperature.

Supposing, therefore, that a ship-of-war was built in our climate of ordinary steel, and clad with some three thousand tons of such nickel-steel armour, we are confronted with the extraordinary fact that if such a ship visited the Arctic regions, it would actually become some two feet longer, and the shearing which would result from the expansion of the armour by exposure to cold would destroy the ship. Before I leave the question of the nickel-iron alloys, let me direct your attention to this triple alloy of iron, nickel and cobalt in simple atomic proportions. Dr. Oliver Lodge believes that this alloy will be found to possess very remarkable properties; in fact, as he told me, if nature had properly understood Mendeleef, this alloy would really have been an element. As regards electrical properties of alloys, it is impossible to say what services the rarer metals may not render; and I would remind you that "platinoid," mainly a nickel-copper alloy, owes to the presence of a little tungsten its peculiar property of having a high electrical resistance which does not change with temperature.

One other instance of the kind of influence the rarer metals may be expected to exert is all that time will permit me to give you. It relates to their influence on aluminium itself. You have heard much of the adoption of aluminium in such branches of naval construction as demand lightness and portability. During last autumn Messrs. Yarrow completed a torpedo boat which was built of aluminium alloyed with 6 per cent. of copper. Her hull is 50 per cent. lighter, and she is 3½ knots faster than a similar boat of steel would have been, and, notwithstanding her increased speed, is singularly free from vibration.

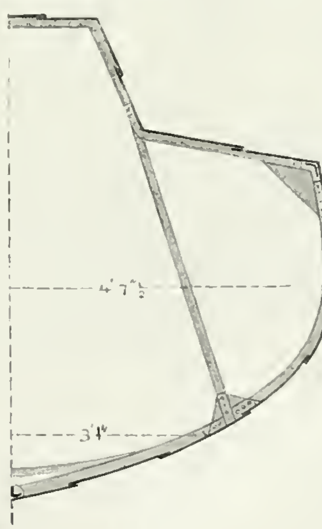


FIG. 7. Half-section Midship of Aluminium Torpedo-boat

Her plates are $\frac{1}{16}$th inch thick, and $\frac{1}{8}$th inch where greater strength is needed. It remains to be seen whether copper is the best metal to alloy with aluminium. Several of the rarer metals have already been tried, and among them titanium. Two per cent. of this rare metal seems to confer remarkable properties on aluminium, and it should do so according to the views I have expressed, for the cooling curve of the titanium-aluminium alloy would certainly show a high subordinate freezing point.

Hitherto I have appealed to industrial work, rather than to abstract science, for illustrations of the services which the rarer metals may render. One reason for this is that at present we have but little knowledge of some of the rarer metals apart from their association with carbon. The metals yielded by treatment

[1] Weaver, "Notes on Armour." *Journal* U.S. Artillery. Vol. iii, 1894, p. 417.
[2] Brassey's *Naval Annual*, 1894, p. 367.
[3] *Engineering*, vol. lvii., 1894, pp. 465, 530, 595.

of oxides in the electric arc are always carbides. There are, in fact, some of the rarer metals which we, as yet, can hardly be said to know except as carbides. As the following experiment is the last of the series, I would express my thanks to my assistant, Mr. Stansfield, for the great care he has bestowed in order to ensure their success. Here is the carbide of calcium which is produced by heating lime and carbon in the electric arc. It possesses great chemical activity, for if it is placed in water the calcium seizes the oxygen of the water, while the carbon also combines with the hydrogen, and acetylene is the result, which burns brilliantly. [Experiment shown.] If the carbide of calcium be placed in chlorine water, evil smelling chloride of carbon is formed.

In studying the relations of the rarer metals to iron, it is impossible to dissociate them from the influence exerted by the simultaneous presence of carbon; but carbon is a protean element—it may be dissolved in iron, or it may exist in iron in any of the varied forms in which we know it when it is free. Matthiessen, the great authority on alloys, actually writes of the "carbon-iron alloys." I do not hesitate therefore, on the ground that the subject might appear to be without the limits of the title of this lecture, to point to one other result which has been achieved by M. Moissan. Here is a fragment of pig iron highly carburised: melt it in the electric arc in the presence of carbon, and cool the molten metal suddenly, preferably by plunging it into molten lead. As cast iron expands on solidification, the little mass will become solid at its surface and will contract; but when, in turn, the still fluid mass in the interior cools, it expands against the solid crust, and consequently solidifies under great pressure. Dissolve such a mass of carburised iron in nitric acid to which chlorate of potash is added; treat the residue with caustic potash, submit it to the prolonged attack of hydrofluoric acid, then to boiling sulphuric acid, and finally fuse it with potash, to remove any traces of carbide of silicon, and you have carbon left, but—in the form of *diamonds*.

If you will not expect to see too much, I will show you some diamonds I have prepared by strictly following the directions of M. Moissan. As he points out, these diamonds, being produced under stress, are not entirely without action on polarised light, and they have, sometimes, the singular property of flying to pieces like Rupert's drops when they are mounted as preparations for the microscope. [The images of many small specimens were projected on the screen from the microscope, and (Fig. 8, E.) shows a sketch of one of these. The largest diamond yet produced by M. Moissan, is 0·5 millimetre in diameter.]

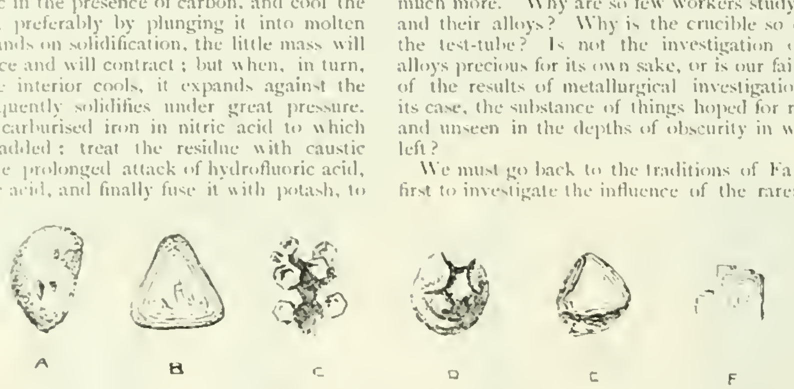


Fig. 8.—Preparations for the microscope of diamonds and other forms of carbon obtained from carburised iron.

A (Fig. 8) represents the rounded, pitted surface of a diamond, and B a crystal of diamond from the series prepared by M. Moissan, drawings of which illustrate his paper.[1] The rest of the specimens, C to F, were obtained by myself by the aid of his method as above described. C represents a dendritic growth apparently composed of hexagonal plates of graphite, while D is a specimen of much interest, as it appears to be a hollow sphere of graphitic carbon, partially crushed in. Such examples are very numerous, and their surfaces are covered with minute round graphitic pits and prominences of great brilliancy. Specimen E (which, as already stated, was one of a series shown to the audience) is a broken crystal, probably a tetrahedron, and is the best crystallised specimen of diamond I have as yet succeeded in preparing. Minute diamonds, similar to A, may be readily produced, and brilliant fragments, with the lamella structure shown in F, are also often met with.

The close association of the rarer metals and carbon and their intimate relations with carbon, when they are hidden with it in iron, enabled me to refer to the production of the diamond, and afford a basis for the few observations I would offer in conclusion. These relate to the singular attitude towards metallurgical research maintained by those who are in a position to promote the advancement of science in this country. Statements respecting the change of shining graphite into brilliant diamond are received with appreciative interest; but, on the other hand, the vast importance of effecting similar molecular changes in metals is ignored.

We may acknowledge that "no nation of modern times has done so much practical work in the world as ourselves, none has applied itself so conspicuously or with such conspicuous success to the indefatigable pursuit of all those branches of human knowledge which give to man his mastery over matter."[1] But it is typical of our peculiar British method of advance to dismiss all metallurgical questions as "industrial," and leave their consideration to private enterprise.

We are, fortunately, to spend, I believe, eighteen millions this year on our Navy, and yet the nation only endows experimental research in all branches of science with four thousand pounds. We rightly and gladly spend a million on the *Magnificent*, and then stand by while manufacturers compete for the privilege of providing her with the armour-plate which is to save her from disablement or destruction. We as a nation are fully holding our own in metallurgical progress, but we might be doing so much more. Why are so few workers studying the rarer metals and their alloys? Why is the crucible so often abandoned for the test-tube? Is not the investigation of the properties of alloys precious for its own sake, or is our faith in the fruitfulness of the results of metallurgical investigation so weak that, in its case, the substance of things hoped for remains unsought for and unseen in the depths of obscurity in which metals are still left?

We must go back to the traditions of Faraday, who was the first to investigate the influence of the rarer metals upon iron, and to prepare the nickel-iron series of which so much has since been heard. He did not despise research which might possibly tend to useful results, but joyously records his satisfaction at the fact that a generous gift from Wollaston of certain of the "scarce and more valuable metals" enabled him to transfer his experiments from the laboratory in Albemarle Street to the works of a manufacturer at Sheffield.

Faraday not only began the research I am pleading for to-night, but he gave us the germ of the dynamo, by the aid of which, as we have seen, the rarer metals may be isolated. If it is a source of national pride that research should be endowed apart from the national expenditure, let us, while remembering our responsibilities, rest in the hope that metallurgy will be well represented in the Laboratory which private munificence is to place side by side with our historic Royal Institution.

[1] *Comptes rendus*, [illegible] p. [illegible]

ELECTRICITY AND OPTICS.

A MEMOIR of singular interest, and one of which it would be well if the contents could be made more readily accessible to students in this country, has lately been published by Prof. Righi.[2] Among the numerous papers published during the last twenty years by Prof. Righi there are several (on electric discharges, on electric shadows and photo-electric phenomena) which indicate his interest in the relations between light and electricity. Since Hertz succeeded in obtaining rays of electric force, and demonstrated the reflection, refraction and interference of electric radiation, other experimenters have endeavoured to extend and complete the analogy between electromagnetic and luminous vibrations. Thus Lodge and Howard showed that electric radiation could be concentrated by means of large lenses;

[1] *The Times*, February [illegible], 1895.
[2] "Sulle oscillazioni elettriche a piccola lunghezza d'onda e sul loro impiego nella produzione di fenomeni analoghi ai principali fenomeni dell' ottica." (Bologna: 1894).

Boltzmann appears to have performed an experiment similar to Fresnel's with inclined mirrors; Trouton has drawn attention to phenomena similar to those of thin plates; and others have experimented with wire gratings like those by means of which Hertz demonstrated the polarisation of electric radiation: but the great wave-length (about half a metre) of the oscillations used has been a stumbling-block in the way of more delicate experiments. Prof. Righi has succeeded in producing oscillations having a wave-length as small as 2·6 cm., and has devised a novel form of resonator made by taking a strip of silvered glass, dissolving away the varnish from the back, and drawing a diamond-line across. He has thus been able to demonstrate the analogy with other phenomena of optics, among which may be mentioned:—Fresnel's interference-experiments with inclined mirrors and biprism; interference by reflection from thin plates and by transmission through them; diffraction by various means (slits, edges, Fresnel's diaphragm); elliptic and circular polarisation; and total reflection. The description of the experiments is accompanied with full theoretical discussions; and if Prof. Righi does not aim at the general treatment which is suitable to a treatise like Poincaré's, he, at any rate, succeeds admirably in showing how the border-land between electricity and optics is being actually explored.

In another memoir,[1] Prof. Righi develops Hertz's equations so as to find the electromagnetic disturbance produced by the combination of two small rectilinear electric oscillations at right angles, say along the axes of z and y, having equal amplitudes but differing in phase by a quarter wave-length. Each of these might be replaced by the mechanical movement of equal and opposite electric charges, oscillating with pendular motion about the origin along one of the axes. Two such mechanical motions at right angles, differing in phase by a quarter wave-length, would compound into a motion of uniform rotation in a circle about the origin in the plane of zy. The disturbance due to such a circular motion of equal and opposite charges would, with certain limitations, be the same as the disturbance produced by the combination of the two rectilinear oscillations first considered. Prof. Righi shows that it takes the form of a spherical wave having its centre at the origin of coordinates. The vibrations are in general (to use the language of optics) elliptically polarised: in the neighbourhood of the axis of x they are circularly polarised: in the equatorial plane zy they are plane-polarised.

In a third memoir, by Prof. H. A. Lorentz,[2] an attempt is made to establish a theory of electrical and optical phenomena in connection with moving bodies. This naturally involves a discussion of the relation between the ether and ponderable bodies in motion, and of the theories proposed by Fresnel and Stokes respectively. After weighing the evidence on both sides, the Leyden professor is of opinion that Fresnel's conception offers fewer difficulties than its rival. The question is of importance in electricity as well as in optics; it is necessarily raised by a rigid examination of any electrical phenomenon, such as the motion of a charged body or of a conductor carrying a current. Prof. Lorentz bases his explanation of electrical phenomena on the hypothesis that all bodies contain small electrically charged particles, and that all electrical processes depend upon the position and motion of these "ions." This conception of ionic charges is universally accepted for electrolytes, and also forms the most probable explanation of the convective discharge of electricity in gases. It is here extended to ponderable dielectrics, the "polarisation" of which is ascribed to the existence of such particles in positions of equilibrium from which they can only be displaced by external electrical forces. The periodically changing polarisations which, according to Maxwell's theory, constitute light-vibrations, here become vibrations of the ions.

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SCIENCE IN THE MAGAZINES.

A MOST interesting account of Madame Kovalevsky's eventful life is contributed to the *Fortnightly* by Mr. E. W. Carter. The sketch is based upon that gifted mathematician's own published recollections, and Madame Edgren-Leffler's biography of her lamented friend. As there are some who are not familiar with the career of the subject of Mr. Carter's article, a short summary of the chief points may be of interest. Sophie Kovalevsky was born at Moscow about 1850, where the first five years of her life were spent. Her father then removed to Palibino, in the government of Vitebsk. It was there that her bent for mathematics first showed itself. A room had been papered with old disused printing paper, amongst which were several sheets of Ostrogradski's lectures on the differential and integral calculus. "This room possessed a strong fascination for the little seven-year-old maiden. Here she was to be found daily, her attention riveted on these walls, striving to understand something of the strange figures and stranger formulas. 'I remember,' says Madame Kovalevsky, 'that every day I used to spend hours before these mysterious walls, struggling to understand some of the sentences, and to find the order of the sheets. By dint of long contemplation, some of the formulas became firmly fixed in my memory, and even the text, though I could comprehend nothing of it at the time, left its impression on my brain.' When several years later, her father was prevailed on to let her have some instruction in mathematics, the results were a surprise and a revelation to all concerned; not least to the little pupil herself. The mysteries of the walls now grew clear, and her progress was made by leaps and bounds. The differential calculus presented no difficulties to her, and her tutor found that she knew the formulas by heart, and arrived at solutions and explanations quite independent of his aid."

In October, 1868, Sophie Kroukovsky contracted the romantic marriage with Vladimir Kovalevsky, and the two went to Heidelberg as students at the University. After two terms spent at Heidelberg, she moved to Berlin, where she worked for four years under the direction of Prof. Weierstrasse, "the father of modern mathematical analysis." During this period, she was occupied in writing the three important treatises which subsequently gained for her the degree of Doctor in Philosophy at Göttingen. Passing over the next few years in Madame Kovalevsky's life, during which her husband died, we come to the winter of 1883-84, when she went to Stockholm as the "Docent" of Prof. Mittag-Leffler. A course of lectures delivered during the winter session led to her appointment to the chair of higher mathematics at the University of Stockholm, in July, 1884, a post which she occupied until her death. The crowning scientific labour of her life was the treatise which gained for her the Bordin prize of the Paris Academy in 1888. The subject proposed was "To perfect in one important point the theory of the movement of a solid body round an immovable point," and in recognition of the extraordinary merits of Mdme Kovalevsky's work, the judges raised the amount of the prize from three thousand to five thousand francs. But the distinguished authoress did not live many years to enjoy the high position she had gained. In February, 1891, she was attacked by an illness which ended fatally after three or four days. So passed away a woman of magnificent gifts, who, "Taking the direction of her life into her own hands, and choosing for herself one of the steepest paths to fame, she traversed it with swift and steady steps."

Mr. W. H. Hudson contributes to the *Fortnightly* an article on "The Common Crow," a bird which he finds from inquiries, "is no longer to be found as a breeder, or is exceedingly rare, in districts where game is very strictly preserved; but that in the wilder counties where game is not strictly preserved, in wooded hilly places, he still exists in diminished numbers as a breeding species." Another article in the same magazine, on "Danish Butter Making," by Mrs. Alec Tweedie, furnishes instructive reading for British agriculturists.

The remarkable growth of electric railroad mileage in the United States, during the past five years, is brought out in an article by Mr. Joseph Wetzler, in *Scribner*. "At the present time," he says, "there are over eight hundred and fifty electric railways in the United States, operating over 9000 miles of track and 23,000 cars, and representing a capital investment of over four hundred million dollars. What stupendous figures, when we consider that in 1887 the number of such roads amounted to only thirteen, with scarcely one hundred cars!" A quotation from a paper in the series on "The Art of Living," contributed by Mr. Robert Grant to the same magazine, is worth giving here. "There are signs that those in charge of our large educational institutions all over the country are beginning to recognise that ripe scholarship and rare abilities as a teacher are entitled to be well recompensed pecuniarily, and that the breed of such men is likely to increase somewhat in proportion to the size and number of the prizes offered. Our college presidents and

[1] "Sulle onde electromagnetiche generate da duo piccole oscillazioni elettriche ortogonali oppure per mezzo di una rotazione uniforme." (Bologna: 1894).

[2] "Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Körpern." (Leyden: 1895).

professors, those at the head of our large schools and seminaries, should receive such salaries as will enable them to live adequately. By this policy not only would our promising young men be encouraged to pursue learning, but those in the highest places would not be forced by poverty to live in comparative retirement, but could become active social figures and leaders."

Evolution, and problems belonging to it, crop up periodically as subjects of magazine articles. In the *Contemporary*, A. Fogazzaro, "writer of verses and novels," devotes a number of pages to the polemic battles that have been fought over the evolutionary idea, from the time of Lamarck. "For the Beauty of an Ideal" is the title of his article, which mostly aims at showing how the new wine of evolution may be put into old bottles of Catholic doctrine." A paper on "Evolution and Heredity" is contributed by Dr. G. Symes Thompson to the *Humanitarian*. An introduction to a series of articles on "Professional Institutions," by Mr. Herbert Spencer, appears in the *Contemporary*. The articles will, in their eventual form, constitute part vii. of the "Principles of Sociology."

Two papers in the *Century* call for brief notice. In one, Mr. W. E. Smythe shows how parts of the great arid region to the west of the one-hundredth meridian in the United States have been benefited by careful irrigation. "The work of reclamation has been going forward silently, but gradually and surely, for the better part of a generation. Between ten and twenty millions of acres are now under ditch, and some slight rivulets of population have begun to trickle in upon the lands. But the threshold is scarcely passed. The arid region as a whole comprises more than 800,000,000 acres. Of this empire more than half a billion acres is still the property of the Government." The second paper to which reference has been made, is a short description of three reproductions from photographs of the tree beneath which was buried the heart of Dr. Livingstone. The tree was found near the site of the deserted village of Chitambo, on the south shore of Lake Bangweolo. Upon it, Jacob Wainwright, the Nassick boy who read the Burial Service, chiselled the words, still plainly visible, "Dr. Livingstone, May 4, 1873. Jazuza. Mniasere. Vchopere."

The *Reliquary and Illustrated Archæologist* (April) contains an account, by Mr. Miller Christy, of the exploration of "Deneholes" in Essex and Kent, conducted by the Essex Field Club. Deneholes are ancient artificial caverns in the chalk, having deep, narrow, vertical entrances. They are found in various parts of England, but especially along the banks of the Thames, in Essex and Kent. Mr. Christy has explored many of them, and his opinion as to their origin is: "On the whole, the only conclusion which it seems as yet safe to arrive at is that the mystery surrounding the origin of the Deneholes and the purposes of their makers still constitutes one of the most interesting and perplexing problems yet remaining unsolved in British archæology, perhaps we may say in prehistoric British archæology."

Mr. A. Symons Eccles, in the *National*, writes on "Headaches," and, in the course of his paper, gives the opinion of a distinguished neurologist, that almost every man of science of distinction in London suffers from sick-headache, or migraine, on account of excessive intellectual activity. Mr. Eccles says if they "will sit down to dinner in a state of nervous exhaustion, or do brain work directly after taking food, they can hardly hope to escape from an attack of migraine." In the same review, Miss Balfour concludes the account of her journey through the British South Africa Company's territory, in 1894.

A brief notice will suffice for the other articles in the magazines and reviews received by us. A previously unpublished paper of Richard Jefferies' appears in *Longman's Magazine*, and also a poem by the late Dr. G. J. Romanes. In the *English Illustrated*, the articles from which natural knowledge may be gathered are "Mountaineering in Westmoreland," by Mr. J. F. Fraser; "Stalking the Haplocerus in the Selkirks," by Mr. W. A. Baillie-Grohman; and a "Moorland Idyll," by Mr. Grant Allen. In the *Quarterly Review*, the recently published biographies of Buckland and Owen are used as the basis for an article on advances in the science of geology during this century. *Good Words* contains a short illustrated paper on the Dandelion, by Dr. Hugh Macmillan, and one on "The Sea Birds of the Cape," by the Rev. W. Greswell. Another readable article on birds is Mr. C. J. Cornish's "Birds of the Cliff," in the *Sunday Magazine*. *Chambers's Journal* has the usual complement of instructive articles on more or less scientific topics. Finally, the *Westminster Quarterly Review* contains contributions by Dr. W. T. Greener and Sir Henry Howorth.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Term is now in full swing, and the usual courses of lectures are being delivered in the various departments of Natural Science. The changes from last Term's list are, that Sir J. Conroy and Mr. Frederick Smith have returned to Oxford, and are lecturing on Radiation and Mechanics, respectively, at Balliol and Trinity Colleges. In the Physiological Department, Prof. Gotch has begun his duties as Waynflete Professor, and is lecturing on Mondays and Tuesdays on the Physiology of the Central Nervous System.

Mr. H. Balfour, Curator of the Pitt-Rivers Museum, has been seriously ill, and is absent from Oxford for this Term, being obliged to go abroad for the sake of his health.

In a Congregation, held on Tuesday, May 7, the proposed Statute on Research Degrees was again under discussion, having reached what is technically known as the twelve-member amendment stage. The House reaffirmed by the narrow majority of 39 against 37, the clause which was passed by a large majority last Term, which states that Science shall be held to include Mathematics, Natural Science, Mental and Moral Science. Other clauses, mostly of consequential importance, were added or rejected, amongst them being one of some importance to intending Candidates, which allows residence in the Vacation to count towards the residence of eight terms required by the Statute.

In the same Congregation, Dr. E. B. Tylor, Reader in Anthropology, was constituted Professor in Anthropology during the tenure of his office as Reader in Anthropology.

The seventh summer meeting of University Extension and other Students will be held this year in Oxford. The meeting, as in previous years, will be divided into two parts: the first part will last from Thursday evening, August 1, to August 12, the second from August 12 to August 26. There will be lectures during both parts of the meeting on Natural Science, with classes for practical work. Among the lecturers will be Prof. Green, Prof. Odling, Dr. Kimmins, Dr. Fison, Mr. Carus-Wilson, Mr. J. E. Marsh, Mr. P. Groom, Dr. Wade, and Mr. G. C. Bourne.

The fourth "Robert Boyle" lecture of the Oxford University Junior Scientific Club will be delivered by Prof. Crum-Brown, F.R.S., on Monday next. His subject will be "The Relation between the Movements of the Eyes and the Movements of the Head."

CAMBRIDGE.—Mr. W. G. P. Ellis, of St. Catharine's College, has been appointed a Demonstrator in Botany.

Applications for permission to occupy the University's tables at the Naples Zoological Station, and the Marine Biological Laboratory at Plymouth, are to be sent to Prof. Newton, Magdalene College, by May 23.

The Syndicate for Advanced Study and Research have proposed new statutes for carrying out the scheme recently approved by the Senate, and have extended the scheme so as to include advanced students in law who are graduates of other Universities.

The honorary degree of Doctor of Science is to be conferred on Mr. Francis Galton, F.R.S.

MR. A. E. TUTTON has been appointed Inspector of Schools and Classes under the Science and Art Department.

THE Report of the Council of the City and Guilds of London Institute, upon the work of the Institute during the year 1894, has just been issued. The Council expressed their satisfaction at the renewal of the contribution of the Corporation of London to the funds of the Institute. Special subscriptions have been received, or promised, from the Salters' Company, in addition to their annual subscription, for the encouragement of chemical research; from the Cordwainers' Company, in addition to their annual subscription to the Institute, and the Leather Trades' School, for the inspection of classes in boot and shoe manufacture in connection with the Technological Examinations Department, and, for the first time, from the Tylers' and Bricklayers' and the Coachmakers' Companies. The proposal of the Salters' Company to place at the disposal of the Institute a sum of £150 a year to be applied to founding one or more Fellowships, to be entitled the Salters' Company Research Fellowships for the encouragement of higher research in Chemistry in its relation to manufactures, has already been referred to in these columns. The scheme for the

administration of this grant, prepared by a Special Committee of the Institute and adopted by the Executive Committee, has since received the sanction of the Court of the Salters' Company. The first award was made in January of the present year to Dr. Martin O. Forster. A sum of £333 4s. 3d. has also been received from the Committee of the Siemens Memorial Window Fund, "as an endowment to furnish a small sum to the recipient of the Siemens Memorial Medal, which is awarded annually to the student of the greatest merit in the Department of Electrical Engineering at the Central Technical College of the City and Guilds of London Institute." The Report deals in detail with the operations of the several colleges, schools, and departments of the Institute's work.

MISS GRACE CHISHOLM has just taken the degree of Doctor of Philosophy at Göttingen, this being the first degree obtained by a lady since Göttingen became a Prussian University. Miss Chisholm was a scholar of Girton College, Cambridge, and was placed between the 22nd and 23rd Wranglers in Part I. of the Mathematical Tripos in 1892, and in Class 3 of the Mathematical Tripos, Part II., in 1893. In 1892 she also took a first class in the Final Mathematical School at Oxford. After leaving Girton, she proceeded to Göttingen, and, receiving permission to attend the mathematical lectures, was in residence there about a year and a half. It was with the express sanction of the Prussian Minister of Education that the doctor's degree was conferred on her, and it is thought that the precedent thus established will probably lead to a substantial development in the opportunities offered for the higher education of women in Germany.

SILVER MEDALS have been awarded to Mr. R. H. Turnbull, Mr. G. F. Mair, and Mr. And. Robertson, of the Glasgow and West of Scotland Technical College. The medals were purchased with funds placed at the disposal of Prof. A. H. Sexton, by the West of Scotland Iron and Steel Institute, for the award of prizes for knowledge of the metallurgy of iron and steel.

SCIENTIFIC SERIALS.

American Meteorological Journal, April.—Recent foreign studies of thunderstorms: Switzerland, by R. De C. Ward. The systematic study of thunderstorms has been regularly carried on in Switzerland since 1883, and the results have been published yearly in the *Annalen* of the Central Meteorological Office, but there has been no general summary of the whole data. The general conditions of thunderstorm development in Switzerland are the presence of cyclonic depressions over Northern Europe, high temperatures, southerly winds and secondary depressions over Switzerland. Note on Croll's glacial theory, by Prof. W. M. Davis. This is a reprint from the *Transactions* of the Edinburgh Geological Society (vol. vii.). The author thinks that the recent studies of Dr. J. Hann, on the origin of cyclones and anti-cyclones, suggest an amendment to Croll's physical explanation of the climate of the glacial period.

Symons's Monthly Meteorological Magazine, April. Earth temperatures and water-pipes, by the Editor. A table shows the earth temperatures at nineteen stations in various parts of the country, from which it is seen that frost penetrated to 1 foot at eleven stations, to 1 foot 6 inches at three stations, to 2 feet at one station, and nowhere reached 2 feet 6 inches. The fact that ice formed in many pipes buried 2 feet 6 inches, and probably lower, is indisputable, but the explanation is not given of the apparent discordance between the temperature of the water and that shown by the earth thermometers.—The great gale in the Midlands on March 24, by H. A. Boys and A. W. Preston. This appears to have been, locally, one of the heaviest gales for many years. In a park near East Dereham, it is said that 1100 trees were uprooted. The worst part of the hurricane was from 1h. 30m. to 2h. 15m. p.m., and both observers state that the gusts were little short of force 12 of the Beaufort scale, which is equivalent to a velocity of ninety miles in the hour.—Snow from a cloudless sky, by C. L. Prince. The author states that at Crowborough, Sussex, on February 6, some snow crystals and minute spiculæ of ice fell at intervals, without any visible cloud.

L'Anthropologie, 1895, tome vi. No. 1.—Note sur l'âge de la pierre en Ukraine, par M. le Baron de Baye. The author collected the materials for this article while residing in the province of Kiev, during the years 1893 and 1894. Little Russia contains three kinds of tumuli of the Stone age: (1) Small tumuli each containing a single skeleton resting on clay or white sand, and wrapped in birch bark; and in which small stone arrow-heads are found, but no stone implements of large size. (2) Cists, constructed of stone slabs, containing vases filled with ashes and burnt bones, with which are associated polished stone weapons. (3) Tumuli containing skeletons, certain parts of which, particularly the bones of the head, are coloured red. Opinions differ as to whether this colouration has been produced naturally or artificially; but the interments may probably be referred to the end of the Stone age, as only three bronze relics have been found in sixty of these tombs opened by Prof. Antonowitch.—La sculpture en Europe avant les influences gréco-romaines, par M. Salomon Reinach. In this number the author describes and figures relics of the Bronze age, chiefly swords and dagger hilts, many of them of great beauty.—De l'art du potier de terre chez les Néo-Calédoniens, par M. Glaumont. The pots of the New Caledonians are made of clay; they are spheroidal in shape, and have large mouths, the lips of which are turned over and pierced with two, or sometimes four, holes, through which a cord is passed to facilitate transportation from one place to another. They never have feet, but, when used for cooking, are either supported on two or three stones fixed in the ground, or they are suspended from a branch driven obliquely into the earth so as to project over the hearth. The ornamentation is usually very simple, consisting merely of lines, but on one vase from the north of the island, figured by M. Glaumont, there appears a human face in relief.—Les races de l'Ogooué. Notes anthropologiques, par M. Liotard. It is now fully recognised that the population of the Gaboon consists of several peoples of different types, each having special characteristics. M. Liotard has had exceptional opportunities of studying these people, and here records some of the results of his observations.

IN Nos. 1–4 of the *Bullettino* of the *Società Botanica Italiana* for 1895 is an article by Sig. P. Voglino, on the part played by snails and toads in the propagation of certain fungi. In the digestive canal of these animals he found abundance of the spores of species of *Russula*, *Tricholoma*, *Lactarius*, and other species of Agaricini. The faculty of germination of these spores had not been destroyed by passing through the body of the animal. Sig. A. De Bonis contributes a paper on the cleistogamous flowers of *Portulaca grandiflora*, *Salpiglossis sinuata*, and *Lamium amplexicaule*. The production of these flowers he attributes to unfavourable vital conditions, especially sterility of the soil. The remaining articles are chiefly of interest to Italian botanists.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, April 26.—Mr. Walter Baily, Vice-President, in the chair.—Prof. S. P. Thompson read a note on a neglected experiment of Ampère. Ampère, in 1822, made an experiment which, if it had been properly followed up, must have led to the discovery of the induction of electric currents nearly ten years before the publication of Faraday's results. While attempting to discover the presence of an electric current in a conductor placed in the neighbourhood of another conductor, in which an electric current was flowing, Ampère made the following experiment. A coil of insulated copper strip was fixed with its plane vertical, and a copper ring was suspended by a fine metal wire, so as to be concentric with the coil, and to lie in the same plane. A bar magnet was so placed that if an electric current was induced in the suspended ring, a deflection would be produced. No such deflection, however, was observed. In 1822, in conjunction with de la Rive, Ampère repeated this experiment, using in place of the bar magnet a powerful horse-shoe magnet. He describes the result in the following words: "The closed circuit under the influence of the current in the coil, but without any connection with this latter, was attracted and repelled alternately by the magnet, and this experiment would, consequently, leave no doubt as to the production of currents of electricity by induction if one had not suspected the presence of a small quantity of iron in the copper of which the ring was formed." This closing remark shows that they were looking for a permanent deflection. When, however, Faraday's results were published in 1831, Ampère, after again describing the experiment made in 1822 by himself and de la Rive, says:—"As soon as we connected a battery to the terminals of the conductor the ring was attracted

or expelled by the magnet, according to the pole that was within the ring, which showed the existence of an electric current produced by the influence of the current in the conducting wire. Verdet, when describing the above experiment, falls into a curious error. He says the apparatus consisted of a ring of fine copper wire, suspended by a silk thread in front of the pole of an electromagnet in such a way that the plane of the ring was parallel to the plane of the turns of wire on the electromagnet. On "making" the current the ring is said to have been repelled, but this deviation did not persist, and on "breaking" the current the ring was attracted, also only momentarily. Mr. Blakesley did not feel quite confident that in Verdet's form of the experiment there could ever be attraction. He also pointed out that with an alternating current the disc would tend to set itself parallel to the lines of force of the electromagnet. With reference to repulsion by alternating currents in one of Elihu Thompson's experiments where a sphere is supported over an alternating pole, a screen being placed so as to partly shield this sphere, there is generally a misstatement as to the direction in which the sphere rotates. It rotates in such a direction that the part of its surface next the magnet moves towards the edge of the screen. Dr. Burton said that from the fact that when the current in the electromagnet in Verdet's experiment is broken, the induced current in the ring is in the same direction as the current in the magnet, the ring will be attracted. Mr. Boys confirmed Dr. Burton's statement. He recommended setting the ring at an angle of 45° to the lines of force, under which circumstances a rotation would be obtained. A distinction must, he pointed out, be drawn between such an experiment as that of Verdet and those of Elihu Thompson. The repulsions observed in these latter were only due to the "lag" in the induced currents caused by self-induction. The best materials to use for all such experiments were magnesium and aluminium, since for a given mass these had the highest conductivity. – Mr. W. G. Rhodes read a paper entitled "A theory of the Synchronous Motor." The object of this paper is to give as simple a treatment as possible of the mathematical part of the subject, and to give theoretical proofs of some experimental facts. Starting from the energy equation

$$p + c^2R = cE\cos\psi$$

where p is the output of the motor, R the resistance of the armature, c the current through the armature, E the E.M.F. applied to the motor terminals, and ψ the phase difference between c and E the cases of maximum output, zero output, minimum current at zero power, and maximum phase difference between c and E are considered. These results are, for the most part, obtained directly from the energy equation. The latter part of the paper is devoted to a discussion of the phase relationships between the current and the E.M.F.s in a plant consisting of a generator and motor, and to the variations in the armature reactions in both generator and motor. A theoretical proof is given of the fact, observed by Prof. Silvanus Thompson and others, that an over-excited synchronous motor acts as a condenser, and tends to make the current lead before the generator's E.M.F. Prof. S. P. Thompson said that the mathematical part of the paper was much simpler than that in previous investigations on this subject, and the method of arriving at the results by rejecting imaginary roots of the equations was particularly neat and instructive. The part of the paper relating to armature reactions and phase relationships was quite new. Two results deserved special attention: first, that the maximum current at zero power was the same as if the circuit was non-inductive; second, that the maximum current zero power was double the current corresponding to maximum output. Mr. Blakesley said that the paper did not consider the stability of the system, and he thought some of the results corresponded to regions of unstability. A paper by Mr. Bryan, "On a simple graphical interpretation of the determinantal relation of dynamics," was, in the absence of the author, read by Dr. Burton. The relation is worked out for two specially simple systems possessing one degree of freedom: (1) a particle moving in a straight line with uniform acceleration; (2) a particle moving to and fro along a straight line with an acceleration directed towards a fixed point on the line, and proportional to the distance from that point (simple harmonic motion). On constructing a diagram in which the abscissæ represent values of the single coordinate of the particle, and the ordinates the corresponding values of the momentum, the determinantal relation becomes equivalent to the constancy of the area of a certain elementary parallelogram. In case (1) this parallelogram moves along a parabola, experiencing a shear as it goes, while in case (2) there is no distortion, the (rectangular) parallelogram revolving about the origin of the diagram as if rigidly attached to an inextensible radius vector.

Linnean Society, April 18. Mr. C. B. Clarke, F.R.S., President, in the chair.—In view of the approaching anniversary meeting, the election of auditors was made, when Mr. A. D. Michael and Prof. J. R. Green were nominated on behalf of the Council, and Messrs. E. M. Holmes and H. Groves on behalf of the Fellows.—Mr. T. B. Blow exhibited specimens of the river-weed *Mourera fluviatilis*, Aublet, from the River Essequibo, with observations on its life-history, and lantern slides illustrating the natural haunts of the plant.—Mr. J. E. Harting exhibited and made remarks upon a collection of West African Lepidoptera which had been collected and forwarded by Mr. J. T. Studley from Old Calabar, and was to be presented to the British Museum.—Mr. Howard Saunders exhibited a specimen of the European white-winged Crossbill, *Loxia bifasciata*, which had been shot in co. Fermanagh in February last, and was lent for exhibition by Mr. C. Langham.—Some photographs of English Red-deer heads, showing successive growths of antlers in the same stag by comparison of the shed horns, were exhibited on behalf of Mr. Lucas, of Warnham Court, Horsham.—A paper was then read by Mr. F. W. Keeble, entitled "Observations on the *Loranthaceæ* of Ceylon," in which country the author had made a short sojourn in 1894. After remarking that in Ceylon many species of *Loranthus* have large and conspicuous flowers, with the corolla-tube brightly coloured, more or less tubular and lobed, he pointed out that certain deviations from the typical regularity of the corolla-tube were correlated with the mode of fertilisation of the flower by Sun-birds (*Nectarineæ*), and this was made clear by diagrams and some excellent coloured drawings. Discussing the mode of distribution of the seeds, Mr. Keeble first quoted the views of Engler and Prantl, and the remarks in Kerner's "Pflanzenleben" (English edition), on the dissemination of the European Mistletoe, and then detailed his own observations in the case of tropical *Loranthaceæ*. The modes of germination of various species of *Loranthus* and *Viscum* were then described, as well as the curvature and growth of the hypocotyl, and the effect of contact on the latter, and on its suctorial disc; the paper concluding with some remarks on the forms of fruit and seed of Cinghalese species of *Loranthaceæ*.—Mr. A. Trevor-Battye exhibited and made remarks upon a collection of plants obtained during his sojourn on the Island of Kolguev.

Entomological Society, May 1. – Prof. Raphael Meldola, F.R.S., President, in the chair. Dr. C. G. Thomson, of the University, Lund, Sweden, was elected an Honorary Fellow, to fill the vacancy in the list of Honorary Fellows caused by the death of Pastor Wallengren. – Mr. Waterhouse exhibited a living larva of a Longicorn Beetle. This larva was found in a boot-tree which had been in constant use by the owner for fourteen years, the last seven of which were spent in India. The specimen was brought to the British Museum on May 6, 1890, and was put into a block of beech wood in which it had lived ever since; it did not appear to have altered in any way during these five years. It had burrowed about eight inches, and probably made its exit accidentally. Mr. Blandford referred to a similar case which had come under his notice. – Mr. C. G. Barrett exhibited a long series of the dark and strongly-marked varieties of *Agrotis cursoria* and *Agrotis tritici*, taken on the sand-hills of the north-east coast of Scotland by Mr. Arthur Horne, of Aberdeen. – Mr. Dale exhibited a specimen of a Sesia – supposed to be a new species – from the New Forest. Mr. O. E. Janson exhibited a remarkable species of *Curculionidæ* from the island of Gilolo, having exceedingly long and slender antennæ and legs; it was apparently an undescribed species of the genus *Talanthia*, Pascoe. Mr. Nelson Richardson called attention to a paper by himself, in the *Proceedings* of the Dorset Natural History and Antiquarian Field Club, on the subject of Dorset Lepidoptera in 1892 and 1893. Mr. W. L. Distant communicated a paper entitled, "On a probable explanation of an unverified observation relative to the family Fulgoridæ." In the discussion which ensued, Mr. Blandford said he thought further evidence was required on the subject of the alleged luminosity in the Fulgoridæ before the statement contained in Mr. Distant's paper could be accepted. Mr. J. J. Walker, R.N., contributed a paper entitled, "A preliminary list of the Butterflies of Hong Kong, based on observations and captures made during the winter and spring months of 1892 and 1893." Prof. Meldola

commented on the interesting character of the paper from an entomological point of view, and the value of the observations therein on the geology, botany, and climate of Hong-Kong.

Geological Society, April 24.—Dr. Henry Woodward, F.R.S., President, in the chair.—On the shingle beds of Eastern East Anglia, by Sir Henry H. Howorth, F.R.S. The author has carefully examined the country around Southwold, where the beds known as Westleton beds (which might well have been associated with the name of Southwold) are developed. He alluded briefly to the recent shingle, the pebbles of which are derived from the ancient shingles of the cliffs; the formation of this shingle, he maintained, may belong to a time not far removed from our own day. Turning to the Westleton beds, he noticed that they were essentially "drifts," the component pebbles not having been shaped on the spot, but brought as pebbles from elsewhere; and he gave reasons for supposing that they were derived from pebbly beds in the Lower London Tertiary group and in the Red Crag. He also maintained that the shells of the Westleton beds and Bure Valley beds were derived from crag deposits. Reasons were given for supposing that the pebbles of the Westleton shingle of East Anglia came from the west, and that this moved eastward from the plateau of Suffolk towards the sea. It was considered that these beds can only be explained by a tumultuous diluvial movement.—Supplementary notes on the systematic position of the Trilobites, by H. M. Bernard. Since the publication of a paper by the author in the *Quarterly Journal of the Geological Society* for 1894, two important papers by Dr. Beecher have appeared, giving details as to the structure and appendages of *Triarthrus*. The author, therefore, returned to the subject, and discussed in detail the more recent discoveries in the light of the affinity between *Apus* and the trilobites. He endeavoured to show how the results obtained by Dr. Beecher bear on the larger question as to the suggested origin of both of these animals from a chaetopod annelid modified in adaptation to a new manner of feeding. An experiment to illustrate the mode of flow of a viscous fluid, by Prof. W. J. Sollas, F.R.S. The author, recognising that it is by a knowledge of the laws of viscous flow that we must seek to extend our information concerning the movements of flowing ice, conducted an experiment, the details of which were described, with a model of a glacier composed of the modification of pitch usually known as "cobbler's wax." In the model the pitch moved under its own weight over the horizontal floor of a trough, which was crossed by a barrier to represent an opposing mountain or the rising end of a lake. The results of the experiment showed that the movement of the pitch-glacier was not confined to that portion of it which rose above the barrier, but extended throughout its mass, and that an upward as well as forward movement took place as the barrier was approached. Thus the transport of stones by glaciers from lower to higher levels was by no means an incredible phenomenon, but a necessary concomitant of such simple conditions as those assumed in the experiment.

Malacological Society, April 19.—Dr. H. Woodward, F.R.S., Vice-President, in the chair. In addition to specimens in illustration of authors' papers, the following were shown: Mr. A. S. Kennard exhibited a series of Mollusca from a Pleistocene deposit at Crayford; Mr. S. Pace exhibited two species of *Estheria* from Persia and S. Algeria; Mr. W. M. Webb exhibited mollusca from a Pleistocene deposit at Chelmsford; Mr. E. R. Sykes exhibited a distribution chart of *Clausilia*.—The following communications were read: On some new species of British Mollusca from the *Triton* Expedition, by H. K. Jordan.—The Anatomy of *Natalina caffra*, Fer, by M. F. Woodward.—Descriptions of new species of Mollusca of the genera *Bullia*, *Mangelia*, *Trochus*, &c., from the Mekran Coast, by G. B. Sowerby.—List of Land and Freshwater Mollusca from New Providence Isle, Bahamas, by W. Bendall.—Notes on two cases of the transport and survival of Terrestrial Mollusca in the New Forest, by T. Leighton.

Royal Microscopical Society, April 17.—Mr. A. D. Michael, President, in the chair.—The Secretary said they had received a valuable donation from the South London Microscopical and Natural History Club, in the shape of a lantern with microscope attachment.—Mr. A. Letherby read a short paper upon the structure of the Podura scale.—The President read a paper on the structure of the brain in the Oribatidæ and in some other Acarina.

CAMBRIDGE.

Philosophical Society, April 29.—Exhibition of *Palophus tiaratus* (a stick-insect from Mashonaland), by Dr. D. Sharp.—A modified method of finding the specific gravities of tissues, by Dr. Lazarus-Barlow. The author showed an improved method of finding the specific gravity of tissues. In a research on the pathology of the œdema which accompanies passive congestion, published in the *Philosophical Transactions* of the Royal Society, he used the solutions made up with glycerine introduced by Roy for the estimation of the specific gravity of blood, but found that difficulty arose from the large quantity of muscle used in obtaining the correct specific gravity, and from the fact that the glycerine abstracts water from the muscle with such rapidity that after a very few seconds the piece of muscle invariably sank. He therefore has used for the past year solutions of various specific gravities made with gum arabic, which he arranges in a wide test-tube in their order of density. Alternate layers are coloured blue. Diffusion occurs with extreme slowness, so that 48 hours after arranging the test-tube the various layers are quite evident. The special advantages of the method are that one piece of muscle is sufficient for an estimation, as it sinks through the layers of lower specific gravity until it reaches that layer with which it is identical; that water is abstracted from the muscle by gum much more slowly than by glycerine, and that, as has been shown by Heffter, the vitality of cardiac muscle is better maintained by gum arabic solutions than by any other solution.—Crania of native tribes of the Panjab, by Prof. Macalister.

PARIS.

Academy of Sciences, April 29.—M. Marey in the chair.—A projected balloon expedition to the Arctic regions, by M. S. A. Andrée. The author defines the conditions necessary to be fulfilled by a balloon destined for Arctic exploration, and shows that such conditions can be fulfilled. He has succeeded in obtaining a certain amount of directive power by using a rope drag to retard the progress of the balloon relatively to the wind, and then using a sail in the ordinary way. By this device a mean deviation of 27° has been secured. Sometimes a deviation of nearly 40° has been obtained. M. Émile Blanchard in connection with this paper calls attention to the probability of existence of an open polar sea, and points out the support this view receives from the many flocks of web-footed birds observed making their way northwards by explorers when nearest to the pole.—On the double points of a group of algebraical surfaces, by M. G. B. Guccia.—On the types of groups Ω of substitutions, of which the order equals the degree, by M. R. Levavasseur.—On an application of M. Darboux's method (mathematical analysis), by M. Beudon.—On the rotation of solids, by M. R. Liouville.—On a class of periodic solutions in a particular case of the problem of three bodies, by MM. J. Perchot and J. Mascart.—Measurements of the intensity of gravity in Russia, by M. G. Defforges. Data are given for Pulkowa, Tiflis, Ouzoun Ada, Bokhara, and Tashkend, from which it is shown that the negative continental anomaly is very pronounced at Ouzoun Ada and Tashkend, and at Bokhara is of the same order as at Paris; the positive anomaly is greater than previously observed at Pulkowa.—On the specific heat of superfused liquids, by M. Louis Bruner. Thymol and paracresol give specific heats increasing with the temperature range when cooled without solidification to approximately the same extent below their melting-points for each experiment. Menthol and bromal and chloral hydrates cannot be obtained superfused by cooling.—On the solidification of some organic substances, by M. Louis Bruner.—On the regularity of luminous movement, by M. Gouy.—On the electric resistance of saccharine liquids, by MM. Gin and Leleux. Expressions are given showing the relationships between resistance and the concentration and temperature of saccharine solutions. The resistance is shown to be a function of the current density. This result is explained on the Arrhenius hypothesis as due to the state of ionisation of the badly conducting electrolyte.—New researches on the heats of combination of mercury with the elements, by M. Raoul Varet.—On the action of the halogen compounds of phosphorus on metallic copper, by M. A. Granger. Copper phosphide, CuP_2, is produced by passing phosphorus trichloride vapour in carbon dioxide over slightly heated copper; cuprous chloride is formed at the same time and deposited at the end of the tube. PBr_3 and PI_2 give the same compound. PF_3 needs a red-heat, and produces Cu_5P_2.—Researches on manganese, by M. Charles Lepierre. The manganic-ammonium

sulphate is described in addition to hydrated and anhydrous ammonium-manganous sulphate.—Campholenic acids and amides, by M. A. Behal. Isomeric acids and amides have been obtained. The solid acid was thought to be the racemic form of the liquid acid, but all attempts to separate optical isomers have failed.—Double combinations of anhydrous aluminium chloride with nitro-compounds of the aromatic series, by M. G. Perrier. A series of compounds of the type $Al_2Cl_6 . 2C_6H_4 . CH_3 . NO_2$ (1 : 4) are described, and it is shown that with nitro-derivatives of the type of paranitrotoluene, Friedel and Crafts' reaction fails.—On a possible error by the use of Fehling's solution for the estimation of sugar in urine from persons submitted to treatment with sulphonal, by M. Ph. Lafon. On the panification of brown bread, by M. James Chappuis.—On the causes which produce the colour of brown bread, by M. Léon Boutroux. Gluten may give the colour in bread by desiccation, but not by fermentation. By oxidation with air in presence of water, bran may produce the colouration of bread; but, again, fermentation has no such effect. The acidity of the yeast is a protection against browning.—On the ethology of the genus *Thaumaleus Kroyer*, by M. Alfred Giard.—Observations on the hornets, by M. Charles Janet.—New researches on "la brunissure," by M. F. Debray.—Action of static sparks on the local temperature of regions submitted to this method of franklinisation, by M. H. Bordier.—Treatment of a case of sarcoma by serotherapy, by MM. J. Hericourt and Ch. Richet. The catastrophe of Laibach, April 14, 1895, by M. Ch. V. Zenger.

Washington.

National Academy of Sciences, April 16–20. On some variations in the genus Eucope, by A. Agassiz and W. McM. Woodworth; notes on the Florida reef, by A. Agassiz; the progress of the publications on the expedition of 1891 of the U.S. Fish Commission Steamer *Albatross*, Lieut.-Commander Z. L. Tanner commanding, by A. Agassiz; on soil bacteria, by M. P. Ravenel; a linkage showing the laws of the refraction of light, by A. M. Mayer; on the colour relations of atoms, ions and molecules, by M. Carey Lea; mechanical interpretation of the variations of latitude, by R. S. Woodward; on a new determination of the nutation-constant, and some allied topics, by S. C. Chandler; on the secular motion of a free magnetic needle, by L. A. Bauer; on the composition of expired air, and its effect upon animal life, by J. S. Billings; systematic catalogue of European fishes, by Th. Gill; the extinct cetacea of North America, by E. D. Cope; on the application of a percentage method in the study of the distribution of oceanic fishes (1) definition of eleven faunas and two sub-faunas of deep sea fishes, (2) the relationships and origin of the Carribeo-Mexican and Mediterranean sub-faunas, by G. Brown Goode; on the two isomeric chlorides of ortho-sulpho-benzoic acid, by Ira Remsen; on some compounds containing two halogen atoms in combination with nitrogen, by Ira Remsen; presentation of the Watson Medal to Mr. Seth C. Chandler, for his researches on the variation of latitudes, on variable stars, and for his other works in astronomy; biographical memoir of Dr. Lewis M. Rutherfurd, by B. A. Gould; relation of Jupiter's orbit to the mean plane of four hundred and one minor planet orbits, by H. A. Newton; orbit of Miss Mitchell's Comet, 1847 VI, by H. A. Newton.

New South Wales.

Linnean Society, March 27.—Prof. David in the chair. The President delivered the annual address, in the course of which reference was made to the recent suit in the Equity Court, in which the Society was defendant, brought by the University of Sydney to obtain the declaration of the Court as to the construction of so much of the will of the late Sir William Macleay as relates to his bequest of £12,000 for the endowment of Bacteriology; and the full text of the judgment of his Honour the Chief Judge in Equity was read. After summarising the contributions to science made during the year by the various local scientific institutions and departments, the President passed on to consider at some length the subject of recent research in the Arctic and Antarctic regions, and especially the important position outlined by Dr. John Murray, namely, that of the desirability of a thorough and systematic exploration of the whole Antarctic region with all the appliances of the modern investigator. The following gentlemen were elected office-bearers and Council for 1895. President: Henry Deane. Vice-Presidents: Dr. James C. Cox, Prof. W. A. Haswell, Prof. T. W. E. David. Treasurer: The Hon. James Norton. Council: John Brazier, Cecil W. Darley, Thomas Dixson, J. R. Garland, Arnold U. Henn, A. H. S. Lucas, J. H. Maiden, C. J. Martin, Perceval R. Pedley, P. N. Trebeck, Thomas Whitelegge, Prof. J. T. Wilson.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Elements of Health: Dr. L. C. Parkes (Churchill).—A Treatise on Practical Chemistry: Dr. F. Clowes, 6th edition (Churchill).—Chemical Technology, edited by Groves and Thorp. Vol. 2. Lighting (Churchill).—Fern-Growing: E. J. Lowe (Nimmo).—Le Centenaire de l'École Normale (Paris, Hachette).—Le Cause dell' Era Glaciale: L. de Marchi (Pavia, Fratelli Fusi).—Die Lehre von der Elektrizität und deren Praktische Verwendung: Th. Schwartze (Leipzig, Weber). Physikalische Krystallographie: P. Groth, Dritte Auflage, 3 Abthg. (Leipzig, Engelmann).—Low's Chemical Lecture Charts (Low).—Die Photographie ein Handbuch für Fach-und Amateur-Photographen: A. Hertzka (Berlin, Oppenheim).—Object-Lessons in Botany: E. Snelgrove, Book 2 (Jarrold).—Dakota Grammar, Texts and Ethnography: S. R. Riggs (Washington).—Eleventh and Twelfth Annual Reports of the Bureau of Ethnology: J. W. Powell (Washington).

Pamphlets.—Royal Gardens, Kew: Official Guide to the Museums of Economic Botany, No. 2 (London). The Franklin Institute: W. H. Wahl (Philadelphia). Royal Gardens, Kew: Hand-List of Ferns and Fern-Allies cultivated in the Royal Gardens (Eyre and Spottiswoode). Myodes lemmus, its Habits and Migrations in Norway (Christiania).—List of the Publications of the Bureau of Ethnology, &c. (Washington).—An Ancient Quarry in Indian Territory: W. H. Holmes (Washington).

Serials.—Humanitarian, May (Hutchinson).—Record of Technical and Secondary Education, April (Macmillan).—British Moss-Flora: Dr. R. Braithwaite, April (the Author, Clapham Road).—Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie, Zwanzigster Band, 3 Heft (Leipzig, Engelmann). Fortnightly Review, May (Chapman).—Internationales Archiv für Ethnographie, Band viii. Heft 2 (Leiden, Brill).—Philosophical Society of Washington, Bulletin Vol. xiii. pp. 31–76 (Washington). Zeitschrift für Physikalische Chemie, xvi. Band, 4 Heft (Leipzig, Engelmann). L'Anthropologie, tome vi. No. 2 (Paris, Masson).—Scribner's Magazine, May (Low).—Geological Magazine, May (Dulau). Quarterly Journal of the Geological Society, Vol. li. Part 2, No. 202 (Longmans).—Geographical Journal, May (Stanford).

CONTENTS.

THURSDAY, MAY 16, 1895.

HYGIENE AND METEOROLOGY.

Hygienische Meteorologie. Für Ärtze und Naturforscher Von Prof. Dr. W. J. van Bebber, Abtheilungs Vorstand der deutschen Seewarte in Hamburg. (Stuttgart: Ferd. Enke, 1895.)

NO long preface is needed to prove that meteorology and hygiene have a close and intimate connection, or that the study of both sciences may be mutually helpful. The exhibition of a small death-rate does not exhaust the whole of the problems with which hygiene busies itself. All that tends to ameliorate the condition of the human race, all that ministers to the comfort or promotes the well-being of the individual, is cared for by the student of hygiene. That climate and the phenomena, which we recognise under the comprehensive term "weather," have an intimate connection with the health and comfort of the race, will not be seriously denied, whatever different views may be held as to the precise manner, and to what degree, the condition of the atmosphere can operate on individual cases. Some knowledge of meteorology has hitherto been demanded from candidates for diplomas in sanitary science, public health, or State medicine; and, judging from the rules adopted by the Council, December 1, 1893, the conditions of the examination will in future demand a still closer acquaintance, since the applicant is required to show the possession of a "distinctively high proficiency, scientific and practical, in all the branches of study which concern the public health." To those who seek something more than a bare superficial knowledge of meteorology, this book will be very welcome, and not only to those who desire diplomas, but to the larger, though less specially instructed, class who desire the welfare of the human family.

Coming from one the direction of whose scientific studies is distinctly meteorological, it might be anticipated that the book would deal more with this subject than with hygiene; and to some extent this is the case, and possibly the interest in the book will on this account be diminished. We have a collection of facts, admirably arranged, though drawn, of course, mostly from German sources; and such a collection will be of the greatest value to some student of sanitary and social science, who, trained in physiological schools, will produce a work of greater interest, more closely connected with the spread and mitigation of disease as affected by climate or meteorological conditions of a more or less temporary character. In one important respect, however, the book deviates from the generality of meteorological treatises, and at the same time removes an objection which has frequently been urged by physicians, who assert that weather statistics are not given in the form which is most convenient or most instructive. To take a mean of his observations is too frequently the sole aim of the meteorological observer, and consequently mean results for temperature, for example, are given, where the range of variation is the more important element from the medical point of view. This fact is fully recognised by the author, and he deals not only with the mean values, but also with the amount of variation from the arithmetic mean and the frequency with which such variations occur.

The book is divided into eight sections. The two first treat of the physical properties and of the various ingredients of the air. Elementary physics characterises the first, chemistry the second. In this latter section are described somewhat fully the gases which enter into the atmosphere, not excepting those which are present in minute quantities. Accidental ingredients, such as dust and micro-organisms, are also considered. One does not meet with anything very new, but the facts are well and pleasantly arranged, and would give any student all the information necessary for fully comprehending the successive chapters. It might have been expected that the constituents of water would have been treated with the same degree of fulness. Free oxygen in water may not be of the same importance as in the air, but the aëration of water is not insignificant, whether regarded as an important withdrawal from the atmosphere itself, or the part it plays in the oxidation of organic material, be it in the form of ozone or hydrogen dioxide, or other efficient oxidiser.

The chapter on Temperature is admirable. From a vast collection of material with which intimate study has made the author closely familiar, he is able to systematise and arrange those facts which have the greatest and most obvious bearing on the subject. It is a graphic digest of all that affects the temperature of the world, and is amply illustrated by tables compiled from many sources. We wish we could pay him a compliment on his maps. In the map on page 110 it is only with great difficulty that Europe is recognised, and the one on page 174 is very little better. The tables are, however, so very well arranged, that this slight defect is of little consequence.

As an illustration of the minuteness into which the author enters, we may quote the measures of the temperature of different parts of clothing when worn. The figures have been reduced to Fahrenheit scale, in which form, if less scientific, they may be of use to some of the commercial firms who are interested in such matters.

	Temp. 50°	Temp. 79°
Temp. on the coat	71°·2	82°·4
,, between coat and vest ...	73·6	83·8
,, ,, vest and linen shirt	75·9	84·7
,, ,, linen shirt and woollen shirt ...	77·4	85·3
,, ,, woollen shirt and skin	90·9	89·8

The loss of temperature which the body experiences at a temperature of 59° is diminished by clothing in the following proportions:—

Radiation from the bare skin	100
,, when covered with wool	73
,, when covered with wool and linen ...	60
,, when covered with wool, linen, and vest	46
,, when fully dressed	33

It does not appear whether the velocity of the wind has been taken into account in deriving these figures. The importance of clothing comes, however, again to the fore

as affected by moisture, where the author computes and illustrates the amount of heat abstracted from the body in order to convert into vapour the water which a saturated suit of clothes is capable of containing.

This latter remark has reference naturally to the chapter on Precipitation, which, with the following one on Thunder-storms, does not call for any special remark. Emphasis is laid on the purifying influences that rain and snow have on the atmosphere; but little is said, perhaps, because little is known with certainty, of cleansing influences on water. The question how far water once contaminated can be restored to its original organic impurity, without the processes of evaporation and reprecipitation, has exercised the minds of chemists and sanitarians in this country with some severity. Information is still necessary both as to the processes at work and the agents by which impurities are removed, as they admittedly are, by some self-cleansing method. The author is understood to recommend filtration as especially necessary to eliminate *auszuscheiden* bacteria, presumably bacteria of a pathogenic character. He does not seem to recognise the fact, if it be a fact, that a filter-bed covered with bacteria has still the power of arresting in a very considerable degree the bacteria in the water that filters through it. How this is accomplished is another matter, which may not concern meteorology, but the large questions of sedimentation and percolation of water in its passage through the ground comes naturally into the treatment by Dr. Bebber, more especially as he enters with some degree of detail into ground water, and the conditions which make it potable or otherwise.

Wind and the motion of cyclones are subjects that the author has made peculiarly his own, and are dealt with here at considerable length. Considering the important results that follow the transport of masses of air from place to place, and the mingling and purification of the atmosphere that is thus effected, it is not suggested that the subject receives an undue amount of attention. The connection between cyclonic paths (*Zugstrasse*) and hygiene, however, is not so immediately evident; but the subject is one that has long interested Dr. Bebber, and he naturally has much to say. It is meteorology pure and simple, and has this defect, that it is scarcely full enough for the student of that science, and in too great detail for the sanitarian.

Perhaps the most interesting chapter in the book is the last, on Climate, and in which is treated diseases under various climatic conditions. On page 275 is given a table showing the annual mortality per thousand in various parts of the world. This table is apparently thrown together haphazard, and does not exhibit that careful arrangement by which Dr. Bebber in other parts of his book has illuminated his work and instructed the student. But the bald facts, as written down, gain by that very absence of symmetry, and are both interesting and gratifying. It is true, as the author is careful to point out, that the facts have been gathered under very various circumstances, under various authorities and systems, and are not strictly comparable; but making every allowance for inexact compilation, they do exhibit a manifest improvement in the health of nations, and bear a gratifying testimony to the successful study and practical enforcement of sanitary laws. The few samples we can extract illustrate best the increased adaptibility of individuals to meet those conditions that are generally regarded as adverse to health and longevity. Take the case of British troops in India:

From 1800–1830.	Annual death rate per thousand		...	84·6
,, 1830–1856.	,,	,,		57·7
,, 1869–1878.	,,	,,		19·3
,, 1879–1887.	,,	,,		16·3

From the West Indies the evidence is of the same character:

From 1820–1836.	European Troops,	Jamaica,	Mortality		121
,, 1817–1846.	,,	,,	West Indies	..	75
,, 1879–1887.	,,	,,	Jamaica	..	11·0
,, 1820–1830.	Negro Troops,		Jamaica	..	30·0
,, 1879–1887.	,,	,,	,,	..	11·6

On the Gold Coast, the figures are so remarkable that that they can only be explained by supposing some different method of computation to have been employed in the two circumstances:

From 1829–1836.	European Troops,	Gold Coast		...	483!!
,, 1879–1885.	,,	,,	,,	...	68

Possibly a similar source of error will explain the only retrograde case to be met with, for which the insalubrious climate of Cayenne is responsible:

From 1819–1849.	Troops,	Mortality	...	27·2
In 1855	,,	,,	...	90·8

Of course some of these beneficent results may be attributed to greater care in the selection of men to be sent to these regions; but it would be distinctly wrong to deny also that much is due to insistence on improved conditions of residence, of clothing, of food and drink, especially in the maintenance of uncontaminated sources of drinking water, in fact an insistence on those conditions which sanitary science has shown to be of the utmost importance to individuals and nations.

Possibly, enough has been said here to show that we have to do with a very interesting book, and one far reaching in its aims. If we have to make any complaint, it is only to express the regret that it is not more so. It is the omissions that are sometimes disappointing, the contents never are. We give, in conclusion, one last illustration. Remembering that the book is issued from Hamburg, and that this town suffered severely from the scourge of cholera in 1892, one cannot but feel that the Observatory is in possession of facts which could not but be of interest in discussing the vexed question of the spread of this disease. Beyond the slightest possible mention on p. 287, the author does not refer to it. Yet it is suggested that he could have told us authoritatively what meteorological conditions coincided with the greatest spread of the disease, that he could have given us details of the temperature of the ground and of the Elbe water (see p. 147) that presumably favoured the increase of the bacillus, if it did not come within his province to discuss any differences of morphology, of virulence, or reproductive faculty in the vibrio.

MECHANICAL ENGINEERING.

A Text-book of Mechanical Engineering. By Wilfrid J. Lineham, Head of the Engineering Department at the Goldsmiths' Company's Institute, New Cross. (London: Chapman and Hall, Limited, 1894.)

MR. LINEHAM says that the desirability of writing his book was suggested to him by the initiative of the City and Guilds of London Institute in providing an examination in mechanical engineering. In preparing students for this examination he was led, he says, "to consider seriously (1) whether the whole theory and practice of mechanical engineering, or even a *précis* of it, could be compressed into one volume; and (2) whether it was desirable so to compress it." After examining Mr. Lineham's book, we must confess to feeling grave doubt whether the second question, at least, should not have been answered in the negative before he set about the execution of so very large a task. The ambition of the attempt is, perhaps, more conspicuous than its success; at the same time the book has good features, and students of engineering may learn from it much that will be valuable to them. It is a novel contribution to engineering literature; by no means wholly satisfactory, but still one that should take a useful place.

Mr. Lineham deprecates in advance the criticism which he expects will be made on the compression of a vast subject into a single volume, by citing "the examples of great and successful writers—to wit, Rankine, Ganot, Deschanel, and others." We do not know whether both adjectives are intended to apply to Ganot and Deschanel, who, in any case, did not write on a subject which has a practice as well as a theory. As to Rankine, who certainly did write great and successful treatises on engineering, the citation seems particularly unfortunate. To compress everything into one volume was exactly what Rankine did not do. He wrote four or five large books dealing with various branches of the subject, and did not hesitate to repeat certain portions in more than one book whenever that was necessary to make each intelligible apart from the rest. Rankine's method and the author's are as wide apart as the poles; and of the two we prefer Rankine's. Moreover, Rankine, in his great series of text-books, dealt almost wholly with the *rationale* of engineering; but here, in a single volume, more than half the space is occupied by a description of the processes of the workshop.

It is in the descriptive portions that Mr. Lineham is at his best. Probably no better general account of hand and machine tools, and of the way to use them, has been published. The pattern shop and foundry, the smithy, the machine shop, fitting and erecting shops, all come in for their due share of attention. The construction of a horizontal engine is selected as a typical case, and is described from start to finish with minuteness of detail and with the aid of many admirable drawings. The illustrations of the book are indeed excellent throughout, both in style and matter. They are illustrations that really illustrate. There are 732 of them, and all are engineers' drawings. They have been prepared with obvious care, and it would seem with unsparing labour on the author's own part. They are treated in a way which allows of their liberal introduction without much expenditure of space. In a word, they are everything that the illustrations in such a text-book ought to be. The descriptive section of the book concludes with a useful chapter on boiler-making and plate work, with a somewhat extended account of hydraulic rivetting processes, and with a short notice of electric welding. In setting forth so much descriptive matter as this first part includes, it is of course difficult to preserve in all parts a proportion to which exception may not be taken. We could wish to have seen more space given to the milling processes, which take so prominent a place in modern workshops. Nine or ten pages for hydraulic rivetting, and a mere page and a half for the universal milling machine, seems less happy a proportion than the author has generally maintained. This, however, is a small matter; and it may safely be said that any engineering pupil or apprentice will have his outlook widened, and his knowledge considerably increased, by reading the first part of Mr. Lineham's book.

To the study of the second part, however, he will do well to bring some independent criticism. The first chapter is on the strength of materials, and we had not penetrated far without finding the ground shaky. Dealing with the nature of shear stress, the author uses the symbols f_t, f_c, and f_s to indicate intensities of tensile, compressive, and shearing stress respectively, and resolves shearing stress into normal stresses inclined at 45° to it by the equation

$$f_c^2 + f_t^2 = f_s^2$$

$$\therefore \quad f_c \text{ or } f_t = \frac{f_s}{\sqrt{2}} = \frac{f}{1{\cdot}414}.$$

This is a bad start in a chapter which is to include references to such subjects as the strength of thick cylinders, the torsion of square shafts, and the effects of combined bending and twisting in crank-shafts.

Immediately after this error is the following paragraph:

"On account of the cup or wedge fracture exhibited when a specimen is broken by tearing or crushing, and for other reasons, Prof. Carus-Wilson argues that rupture takes place by shear stresses at 45°, either wholly or partially. Certain it is that the three stresses are intimately connected, and assist each other in destroying the cohesion of the particles."

We have not an intimate acquaintance with the contributions which Prof. Carus-Wilson has made to this subject; but there is no evident reason why his authority should be invoked in support of an idea which is surely as old as the testing of materials.

Turning to the paragraph headed "Strength of square shaft," we find a geometrical construction described at some length, which is apparently based on Coulomb's erroneous theory. The student who has taken the trouble to follow this will feel excusably confused or irritated when he goes on to read the subsequent lines:

"St. Venant showed, however, in 1856 that Coulomb's ring theory was not strictly applicable to any but circular sections, and gave the following:

$$\text{Moment of square section} = f_s \; {\cdot}208 s^3 \;\cdot$$

because the greatest stress does not occur at the corners. To illustrate St. Venant, Thomson and Tait have

magined the shaft to be a box full of liquid, which, if rotated, would leave the latter behind somewhat, and the apices would cause two stresses—tangential and centripetal—to act on the particles, the former only being of momental value."

Now what is the student, whether at the New Cross Institute or elsewhere, to make of this without further explanation? To introduce St. Venant and say no more than this, is surely giving either too much or not enough. The same criticism might be repeated at many other places. Under the heading of "Pillars and Struts," we are told that Euler is pronounced Oiler (this, at least, is nothing if not practical), and his formula for the stability of long columns is quoted without explanation. Gordon's formula and constants are also quoted, and the subject is dismissed with the dictum:

"Claxton Fidler says a pillar-strength cannot be an absolute quantity, but may be anywhere between Euler and Gordon's results."

The theory of heat engines is treated in an equally scrappy and inconclusive fashion. The student will not find it easy to reconcile what he is told on p. 609 as to the efficiency of the engine not depending on the working substance, with the statement, on p. 613, that "in practice it is difficult to find a sufficiently perfect substance"—which is given as a reason why the efficiency of a real engine is less than the efficiency in Carnot's cycle. He will find himself also at a loss to understand the statement that "in adiabatic expansion external work is done at the expense of internal heat, and is therefore negative"; or to see why the dryness fraction of steam is necessarily "a whole number" (p. 594). Again, to take a matter of first-rate importance in regard to the action of steam in the cylinder, initial condensation is spoken of as if it affected the efficiency merely by the trifling alteration it produces in the form of the expansion curve, and we do not find a hint as to the real reason for its highly prejudicial effect.

It would be unfair to conclude that all the theoretical portions of the book are equally unsatisfactory. But at the best, their brevity, and the narrow limits of mathematical knowledge which the author assumes on the part of his readers, make this part of the work more like an engineering pocket-book than a treatise, the purpose of which ought to be to educate the student to reason about the application of mechanical principles to engineering. If the book, in this aspect, is representative of the teaching which the new Polytechnics are giving, it suggests the inquiry whether what Lord Armstrong once called "the vague cry for technical education" has met with the best possible response. We have no sympathy with those who would exclude either engineer apprentices or any other workmen from the highest education they are capable of. But the question may fairly be asked whether a good deal of what is apparently taught, and taught for the express purpose of enabling pupils to pass a specified examination, is in any just sense education at all. The mental discipline which would be obtained by making a real study of problems such as are touched on in this book, would be of the highest value as an education to the engineer. But there is no royal road to the comprehension of elasticity and thermodynamics. If the young apprentices and working lads, who, much to their credit, flock to the new Polytechnics, will take the trouble to truly master any of these things, they will gain an intellectual possession which will make them better men, if not directly better workmen. We would be the last to set a bound to their aspiration, or to discourage the study of Euler and St. Venant. But as a preparation for any such task, they must first, let us say, learn what is the meaning of a differential coefficient. To offer them scraps of conclusions which have to be taken on trust, and "reasons" which can carry conviction to no one except perhaps a jaded examiner, is giving stones to children who presumably cry for bread. If this represents the "theoretical" side of technical education as the new technical schools understand it, or as examiners accept it, we are still some way from a satisfactory solution of the much-vexed problem. For a great deal of this does not usefully instruct, and does not effectually educate: it is, as we have said, either too much or not enough.

THE LAKE OF GENEVA.

Le Léman Monographie Limnologique. By F. A. Forel. Tome second. (Lausanne: F. Rouge, 1895.)

THE first volume of Prof. Forel's work on the Lake of Geneva appeared in 1892, and was reviewed in these pages (vol. xlvii. p. 5). It dealt chiefly with the physical history of the lake-basin, while the present one, containing parts 6–10 of the whole work, begins with "Hydraulics," or the currents, waves, *seiches*, and other deviations of the surface from the normal form of a fluid at rest. It passes on to thermal questions, such as the temperature at different depths, freezing of the surface, &c.; next to optical questions, such as the colour, occasional iridescence and other peculiarities of the water, and the phenomenon of the *Fata Morgana*; then to acoustics (briefly); and lastly, to the chemistry of the water.

As it is impossible, in the limits of a comparatively short notice, to deal with the numerous subjects included in the present volume, we shall restrict ourselves to those which, perhaps, may be more widely interesting than the rest. The first one concerns those curious oscillations of the level of the lake, which locally are called *seiches*. This phenomenon, as defined by Prof. Forel, consists in an alternate rise and fall of the surface of the water; the movement being roughly comparable with that of a balanced plank, when set swinging by a slight disturbance. These oscillations are more or less rapid; their amplitude varying much. Commonly it is only a very few inches; but it may amount, though rarely, to about six feet, the disturbance sometimes lasting for twenty or twenty-five minutes. The whole question is discussed by Prof. Forel in its various aspects, and a history given of the different explanations which have been advanced. He attributes it neither to the effect of storms, nor to that of wind, nor to that of varying atmospheric pressure, but to a disturbance of the whole mass of water by earth-tremors, and compares it to the effect which may be produced on a fluid contained in a flat dish by tapping the bottom. In this hypothesis, however, he frankly admits the existence of a difficulty; namely, that earthquakes and

seiches are not always associated, for in some cases the former have not been accompanied by the latter. The difficulty is undoubtedly a serious one, and it is thus met by Prof. Forel. In an earthquake the undulatory movement is variable in character. In some cases it affects a pendulum seismograph, in others it does not; much depending on the rate at which the shock travels. If this be quick, it will not produce a perceptible undulation to a mass of water; if it be slow, it will set up a very sensible movement. Thus an earthquake of the latter type will produce a *seiche*, but not one of the former. There is much to be said in favour of this hypothesis; but further seismographic observations are required to show that there is a real coincidence between the nature of the earthquakes and the occurrence of the *seiches*.

More than one point of interest is discussed in the section dealing with optical questions. The Swiss lakes, as is well known, vary in colour, some having a distinctly green tint, but others, and especially the Lake of Geneva, being noted for the exquisite blue of the water. To facilitate comparative observation, Prof. Forel has constructed a scale of colours, beginning with sulphate of copper, as the pure blue, and representing the effects of chromate of potash added in proportions commencing with 2 and ending with 65 per cent. After a careful study of the whole question, he comes to the conculsion that the colour of the water depends not merely on the quantity of minute mineral matter present in a state of suspension, but also on the amount present in solution.

The third point, the chemistry of the water, is also very interesting. The author has collected together a large number of analyses already published, has added some others, and discusses the whole. These exhibit differences more considerable than we might have expected; for instance, the residue after evaporation varies from 160 to 218 mgs. per litre. These differences, allowing for possible errors, are probably due primarily to the affluents of the lake, the waters of which are long in becoming completely mixed with the main mass. The principal constituents of this residue are carbonate of lime, sulphate of lime, and carbonate of magnesia, the amounts being variable. Evidently they depend partly upon the time of the year, for in two samples, drawn from the same locality in January and in May, the numbers in the one case were as 3·3 : 2·6 : 1, in the other 3·7 : 1·4 : 1.

The volume, in short, is full of valuable matter, and worthy of its predecessor. As we said of that, it is a little too diffuse for a scientific treatise, but it was necessary, as the author then explained, to write it so as to attract a larger circle of purchasers.

T. G. BONNEY.

OUR BOOK SHELF.

A Catalogue of the Books and Pamphlets in the Library of the Manchester Museum. By W. E. Hoyle, M.A., F.R.S.E., Keeper of the Museum. (Manchester: J. E. Cornish, 1895.)

THIS catalogue, of 292 pp., owes its appearance in print to private enterprise, and is noteworthy as being classified according to the "Dewey Decimal System," under which each digit composing the registration number of a book marks a distinct narrowing in its significance, and for the arrangement under each class by Cutter's "Decimal Author Table," whereby each book receives a number which is virtually an abbreviation of its author's name. Thus, that "597·0941 Ya 21" denote the second, and "597·0941 Ya 2" the original edition of Yarrell's "History of British Fishes," may appear perplexing; but it is claimed by the advocates of the Dewey-Cutter systems that however much the library may grow, these numerical combinations will remain, and that they allow for maximum extension with minimum disturbance.

The classified catalogue upon which we have commented covers 230 pp., and is followed by a supplementary "author catalogue." The author modestly remarks in his preface, that the volume is "the work of one who is not a professional librarian." The labour of compilation has been great; and this catalogue, like all else that its author has put before the world, bears strongly the stamp of thoroughness and accuracy. We cordially recommend it to our university and public librarians, not, however, without a fear that they may adjudge it dangerous in its over-elaboration.

An index of subjects is appended, and Russian names have been transliterated according to the system advocated in our pages (NATURE, vol. xli. p. 396), and adopted in many of the principal scientific libraries.

A Course of Elementary Practical Bacteriology, including Bacteriological Analysis and Chemistry. By A. A. Kanthack, M.D., and J. H. Drysdale, M.B. (London: Macmillan, 1895.)

A LITTLE volume of 127 pages, primarily intended to carry candidates for diplomas in Public Health through a three months' course in bacteriology, and not pretending to be more than a laboratory guide. The instructions are extremely brief, and for the most part unaccompanied by any theoretical explanation. This entire divorce of theory and practice is, in our opinion, not unattended with danger, often leading the student to unintelligently cram the details of methods without having any proper understanding of the principles involved. It is frequently forgotten that the chief object of laboratory work should be to gain a living knowledge of a science, rather than the acquisition of mere dexterity in its practical technique. The exercises are, as we should anticipate from the experience and standing of the authors, well chosen, thoroughly representative, and cover a large amount of ground. On the other hand, some statements made without qualification may easily give rise to mistakes if accepted without reserve. Thus we are told that it is often possible to give a definite opinion in from eighteen to forty-eight hours, as to the presence or absence of cholera vibrios. Recent researches, however, go more and more to show that it is by no means so easy as was supposed to give a correct "definite opinion" as to the identity of this or any other particular micro-organism. We doubt whether bacteriology is sufficiently advanced to admit of treatment in quite such a final and hard and fast manner as it receives in this text-book; but we are told that these pages are not to supplant the demonstrator, and we would add that they should be carefully supplemented by the teacher. If thus employed, this work should prove a very valuable addition to the bacteriological literature of our country. Especially welcome is the inclusion of the principal methods for the detection of some of the chemical products of bacterial life.

Primer of Navigation. By A. T. Flagg. (London: Macmillan, 1894.)

MR. FLAGG'S little primer can be strongly recommended to all beginners; it is the A B C of the art of navigation. Every step is explained in the most simple and accurate manner; and for students depending upon self-instruction, a better or more clearly written primer would be difficult to imagine.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Origin of the Cultivated Cineraria.

AFTER reading the recent letters on the origin of the cultivated Cineraria, I have consulted the principal authorities cited by Mr. Bateson in NATURE of April 25: I now wish to point out that Mr. Bateson has omitted from his account of these records some passages which materially weaken his case.

Mr. Bateson, as I understand him, considers his letter to prove (1) that modern Cinerarias arose as hybrids from several distinct species; and (2) that the main features of existing varieties were established between about 1830 and about 1846, as a result of the appearance of considerable "sports" among these hybrids or their offspring. I will first discuss the latter half of the letter, in which authorities are quoted to prove two special acts of hybridisation, performed at known dates by known persons, and to show that certain named varieties arose as "sports."

First, as to hybridism. Drummond, of Cork, writing in 1827, is quoted as recommending the cultivation of *C. cruenta* for the production of "fine double and single varieties of different colours." At this date, therefore, *C. cruenta* was apparently variable, and yielded forms worth cultivation without hybridisation.

An article by Mrs. Loudon, written in 1842, is next quoted as evidence that "in or about 1827" Drummond obtained "some handsome hybrids" between *C. cruenta* and *C. lanata*. In this article a list of other hybrids, said to have been produced by unnamed persons between 1827 and 1842, is also given. It is not stated that these hybrids were grown by florists for exhibition, or that they had received definite names. The list is followed by a paragraph, omitted by Mr. Bateson, which is so important that I copy it at length:

"Some of the most beautiful Cinerarias now in our greenhouses have been raised by Messrs. Henderson, Pine-Apple Place; particularly *C. Hendersonii* and the King, both raised from seeds of *C. cruenta*. *C. waterhousiana* was raised by Mr. Tate, gardener to John Waterhouse, Esq., of Well Head, near Halifax, from seed of *C. Tussilaginis*, fertilised by the pollen of *C. cruenta*. Two new ones have lately been raised, of remarkably clear and brilliant colours, apparently from *C. cruenta*, named Queen Victoria and Prince Albert, by Mr. Pierce, nurseryman, of Yeovil, Somersetshire." (*Ladies' Magazine of Gardening*, 1842, p. 112.)

This passage clearly shows that in the writer's belief the hybrids produced by Drummond and others had not given rise to two, at least, of the named varieties of her time: certainly two, and probably two more, were descended from *C. cruenta* alone.

Mr. Bateson refers to this account of *C. waterhousiana*, and also to an earlier one, said to be communicated by Tate himself, the originator of the plant, to a writer in *Paxton's Magazine of Botany*, for 1838. In this account the parents are called *C. cruenta* and *C. tussilagofolia*; and in this, the earliest account, there is no statement as to which species furnished seed and which pollen. I do not know whether *Tussilagofolia* was ever recognised as a synonym of *C. Tussilaginis* or not; since the name does not occur in the *Index Kewensis*, where I find, as the only entry bearing on the subject, "*Waterhousiana* = *Senecio tussilaginis*?" Mr. Bateson has assumed that *Tussilagofolia* is identical with *Tussilaginis*, for while repeating only the statement given by Mrs. Loudon, he cites both her article and that in *Paxton's Magazine* as authorities. Is he sure that there did not exist in 1838 a florist's variety named *Tussilagofolia*?

Again, the writer in *Paxton's Magazine* goes on to express an opinion, not referred to by Mr. Bateson, that several of the florist's varieties known to him are descended from *C. cruenta* alone. He recommends the cultivation of various "species and varieties" (*not* hybrids) of Cineraria, and says "one species especially merits cultivation, namely *C. cruenta*. This may be regarded as the parent of many of those beautiful varieties which are so successfully cultivated by Messrs. Henderson." (*Paxton's Mag. Bot.* iv. p. 220, *not* p. 43.)

Against these specific statements, the only contemporary assertion that all named varieties are hybrids, which is quoted by Mr. Bateson, occurs in the *Journal d'Horticulture*, &c. (Ghent, 1846). This journal contains a general statement that florists' Cinerarias have been produced by crossing and recrossing several species, which are named; but although a list of florists' varieties is given, the exact history and parentage of each variety is not attempted.

Finally Burbidge, who wrote in 1877, is quoted as believing that existing varieties are due to hybridism between three species. It is not mentioned that Burbidge, before giving the systematic list of hybrid plants, in which the passage relied upon occurs, is careful to point out the uncertain nature of much of his evidence, and even writes, by way of caution to his readers, that "the parentage of many of the hybrids enumerated in this book is open to question" (p. 118).

I have only examined one of Mr. Bateson's cases of alleged "sports," namely *C. webberiana*. This plant, as Mr. Bateson says, is described and figured as having flowers of a deep blue, the rays being short and wide as compared with *C. waterhousiana*, for example. I fail to see why Mr. Bateson calls this a "sport." There is no evidence cited by him to show that it is descended from *C. waterhousiana*: and if it is not, then there is nothing remarkable in the shortness of its rays. The colour gives no evidence, without detailed knowledge of its descent; for I find in *Paxton's Magazine*, between 1838 and 1841, varieties recorded which are "lilac tipped with purple," "approaching to a blue," "bright blue," "blue or bluish," and in 1842 comes this "deep blue" variety *webberiana* to complete the gradual series.

Judging only from the documents referred to, it seems clear (1) that *C. cruenta* was cultivated, in what was believed to be a pure state, in 1827, and that it yielded valuable varieties, single and double, at that date; (2) that according to contemporary opinion, many of the varieties cultivated between 1838 and 1842 were directly descended from *C. cruenta*, and were not hybrids; and (3) that in 1842 some florists, at least, were believed to produce new varieties by the continued cultivation of *C. cruenta* alone.

So far as Mr. Bateson's history goes, therefore, it establishes the existence in 1842 of sufficient named varieties, believed to be pure-bred *C. cruenta*, to serve as parents for the flowers of to-day.

As to the actual pedigree of the modern varieties, I am not qualified to express an opinion. All I wish to show is that the documents relied upon by Mr. Bateson do not demonstrate the correctness of his views: and that his emphatic statements are simply evidence of want of care in consulting and quoting the authorities referred to.

W. F. R. WELDON.

University College, London, May 13.

I HAVE read with some interest the communications on this subject which have appeared in NATURE, and I may add that I have examined living plants of the species in question with Mr. Thiselton-Dyer. My memory also serves me sufficiently far back to remember a great variety of different "strains" of *Cineraria*, in which they had not got so far away from the parent *C. cruenta* as they now are. I say the parent *C. cruenta*, because I believe that we have to deal with races or strains, obtained by selection according to the taste of the several selectors, and not with the descendants of hybrids between different species. I think Mr. Bateson has relied too implicitly on the literature of the subject. Many of the records of hybrid productions in the vegetable kingdom are based upon groundless assumptions; mere seminal variations having been mistaken for crosses. It requires some skill and care to raise hybrids in the Compositæ; and when you have raised your hybrid, even assuming a fertile one, you can only propagate it vegetatively. All stability is gone. But it is not so with selected seminal variations of a given species. They will intercross most freely, and give birth to new varieties without end; yet each one of those varieties may be reproduced from seed, by careful isolation, without a single "bastard" appearing. There are several instances among our cultivated plants of this great plasticity combined with stability, but I will give only one—the China Aster. I select this because there can be no question of hybridity; and there is as great, or even a greater, variety than in the herbaceous Cinerarias. But with regard to the latter, I think our experience and the trustworthy literature go to prove that it is an analogous case. Careful selection, year after year, has resulted in the various fixed races or strains offered by florists. I am aware that the letters

on this subject by no means exhaust it; but I think it may be safely asserted that selection has yielded much more than sports.

W. BOTTING HEMSLEY.

Prof. Milne's Observation of the Argentine Earthquake, October 27, 1894.

A FEW days ago I received from Prof. Milne a letter, dated March 15, 1895, in which he sends me a list of earthquake disturbances, compiled from the records he was fortunate enough to rescue from the fire which destroyed his house on February 17. In this list I find no less than three observations of the great Argentine earthquake of October 27, 1894, which was recorded by three different horizontal pendulums. The times given for the beginning of the earthquake—viz. 18h. 0m., 18h. 5m., 17h. 41m.[1]—are not very trustworthy, because they were determined by measuring the linear distance from a break in the curve which was caused regularly every day about noon by taking away the lamp. The exact times of these breaks were noted in a book, which, unfortunately, was destroyed by the fire. Prof. Milne, however, tells me that in the instrument, to which corresponds the first of the above-mentioned times, the lamp was always removed within half a minute or one minute from noon (Japan time). Consequently, the error cannot exceed a few minutes. The duration of the disturbance was between two and three hours in all the three instruments.

If we consider that the error of the first observation is not likely to exceed ten minutes, then we find, by comparing Prof. Milne's observations with those made in Europe, that although the spherical distance between the epicentre of the earthquake and Tokio is *no less than* 17,400 *kilometres*, the earth-motion reached Japan at about the same time, or perhaps even a little earlier, than it arrived in Europe. It is unnecessary to point out the interest which is attached to systematic observations of this kind. Prof. Milne's observation is probably the first in which an earthquake was noticed by seismic instruments at a place so near the antipodes of the earthquake centre. A straight line between the two points is only very little shorter than the earth's diameter; the time required for the motion to pass through the globe was probably less than twenty minutes.

Merseburg, May 1. E. VON REBEUR-PASCHWITZ.

Guanine in Fishes' Skins.

IN a joint paper by Mr. J. T. Cunningham and myself (*Phil. Trans.* vol. clxxxiv., 1893, B, pp. 765–812), we have ventured to question the accuracy of the statement made in many text-books of physiological chemistry, that guanine occurs in combination with calcium in the skin of fishes. We found that the guanine occurs in the free state. In the last number of Hoppe-Seyler's *Zeitschrift für Physiologische Chemie* there is a paper by Herr Albrecht Berthe, dealing with this subject, in which he shows that the calcium so frequently found with the guanine is due to the presence of impurities derived from the tissues and the scales. Its amount depends upon that of the impurities present, and is very variable. Instead of finding 11·76 per cent. required by the formula of "*Guaninkalk*," Berthe finds less than one-third of that percentage present, and even this also varies within wide limits. In the paper referred to above, we found one source of the calcium was due to the presence of comparatively large crystals of calcium phosphate, which are figured on p. 788; but there is no doubt that the bulk of it is derived from the scales.

CHAS. A. MACMUNN.

Oakleigh, Wolverhampton, May 4.

The Oldest Vertebrate Fossil.

NOTICING in your issue of April 11 a reference to the discovery of specimens of *Cyathaspis* in the Silurian of Gotland in strata equivalent to the English Wenlock, and with it the statement that these fossils are "for the present the oldest known vertebrates," I am led to call your attention to the species described by myself from Silurian strata in Pennsylvania in 1885 (p. 48), and again in 1892 (p. 542), in the *Quarterly Journal* of the Geological Society. I forward with this a copy of the paper, from which it will be seen that the Salina (Ononduga) beds that yielded *Palæaspis* are older than the Ludlow (or Lower Helderberg), and that the Clinton are older than the Wenlock (or *Niagara*). Consequently *Onchus Clintoni* of the latter group is thus far the oldest vertebrate. E. W. CLAYPOLE.

Akron, Ohio.

[1] These hours are Japan time, *i.e.* 9h. east of Greenwich, and are reckoned from noon.

TERRESTRIAL HELIUM.

SINCE our last reference to this subject three communications have been laid before the Royal Society. They are as follows:—

HELIUM, A GASEOUS CONSTITUENT OF CERTAIN MINERALS.[1]

An account is given of the extraction of a mixture of hydrogen and helium from a felspathic rock containing the mineral clèveite. It is shown that in all probability the gas described in the preliminary note of March 26 was contaminated with atmospheric argon. The gas now obtained consists of hydrogen, probably derived from some free metal in the felspar, some nitrogen and helium. The density of helium, nearly free from nitrogen, was found to be 3·89. From the wave-length of sound in the gas, from which the theoretical ratio of specific heats 1·66 is approximately obtained, the conclusion may be drawn that helium, like argon, is monatomic. Evidence is produced that the gas evolved from clèveite is not a hydride, and a comparison is made of the spectra of argon and helium. There are four specially characteristic lines in the helium spectrum which are absent from that of argon: they are a brilliant red, the D_3 line of a very brilliant yellow, a peacock-green line, and a brilliant violet line. One curious fact is that the gas from clèveite, freed from all impurities removable by sparking with oxygen in presence of caustic potash, besides other fainter lines, exhibits one, and only one, of the characteristic bright red pair of argon lines. This, and other evidence of the same kind, appears to suggest that atmospheric argon and helium have some common constituent.

Attention is drawn to the fact that on subtracting 16 (the common difference between the atomic weights of elements of the first and second series) from 20, the approximate density of argon, the remainder is 4, a number closely approximating to the density of helium; or, if 32 be subtracted from 40, the atomic weight of argon if it be a monatomic gas, the remainder is 8, or twice the density of helium, and its atomic weight if it too is a monatomic gas.

ON THE NEW GAS OBTAINED FROM URANINITE.[2]

Since my communication on the gas obtained from Uraninite (Broggerite) was sent in to the Society on the 25th ult., I have been employing the method I there referred to in several directions, among them to determine whether the spectrum of the gas indicates a simple or a complex origin.

I was led to make this special inquiry on account of the difference in the frequency of the appearance of D_3 and the other lines to which I referred in the solar chromosphere. For instance, if we take the lines D_3, 4471, and 4302, the frequencies are as follows, according to Young[3]:—

D_3	100	(maximum)
4471	100	,,
4302	3	

Hence, we might be justified in supposing that D_3 and 4471 are produced by the same gas, and that 4302 owes its origin to a different one.

But further experiment has given me one case in which D_3 shows bright, while 4471 is entirely absent. I may now add that an equally important line to 4471, one at 4026·5, appears, with the dispersion employed, in the spectrum of Bröggerite, and both these lines are wide and fluffy, like the lines of hydrogen, and are apparently reversed.

The line 4026·5 has not been recorded by Young, though, as I have stated, the frequency of appearances of 4471 represents the maximum; still, while this is so, the intensity of both these lines in the spectra of the hottest stars is not surpassed, even by those of hydrogen. Hence, opinion as to their representing the same gas must be suspended. Further, I have photographed a line at 4388 apparently coincident with another important line in the same stars. Whether, coming from one source or two, in these three lines seen along with D_3 in the gas obtained by me from Bröggerite, we have, it would seem, run home the most important lines in the spectra of stars of Group III., in which stars alone we find D_3 reversed. Should these results be confirmed, the importance of the gas or gases they represent at a

[1] By Prof. W. Ramsay, F.R.S. (abstract).
[2] Second note. By J. Norman Lockyer, C.B., F.R.S.
[3] See "Solar Physics," Lockyer, p. 612.

certain stage of the evolution of suns and planets can be gathered from an examination of a photograph of the spectrum of Bellatrix.

Another case is afforded by a line at λ 667. This is associated with D_3 in Bröggerite and Cleveite, but the yellow line has been seen in Monazite *without* λ 667. It is almost certain, then, that these two lines represent two gases. Certainty cannot be arrived at till a larger quantity of gas has been obtained.

Again, the red line at λ 6575, close to C, referred to in my previous communication, is seen both in Gummite and Broggerite; but in one case (Gummite) it is seen without D_3, and in the other with it, in one case (Bröggerite) without λ 614, and in the other with it. The above conclusions hold here also.

This line λ 614, possibly coincident with a chromospheric line, has been recorded in Gummite and Broggerite. It has been seen *with* D_3 (in Broggerite) and *without* it (in Gummite).

I have said enough to indicate that the preliminary reconnaissance suggests that the gas obtained from Bröggerite by my method is one of complex origin.

I now proceed to show that the same conclusion holds good for the gases obtained by Profs. Ramsay and Cleve from Cleveite.

For this purpose, as the final measures of the lines of the gas as obtained from Cleveite by Profs. Ramsay and Cleve have not yet been published, I take those given by Crookes,[1] and Cleve,[2] as observed by Thalén.

These are as follows, omitting the yellow line:—

Crookes.	Thalén.
	6677
568.05	
566·41	
516·12	
	5048
	5016
500·81	
	4922
480·63	
	4713·5

The most definite and striking result so far obtained is that, in the spectra of the minerals giving the yellow line, I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of Cleveite by chemical methods is vastly different from that obtained by my method from certain specimens of Broggerite; and since, from the point of view of the blue lines, the spectrum of the gas obtained from Cleveite is more complex than that of Broggerite, the gas itself cannot be more simple.

Even the blue lines themselves, instead of appearing *en bloc*, vary enormously in the sun, the appearances being-

4922 (4921·3) = 30 times
4713 (4712·5) = twice.

These are not the only facts which can be adduced to suggest that the gas from Cleveite is as complex as that from Bröggerite. But while, on the one hand, the simple nature of the gases obtained by Profs. Ramsay and Cleve and by myself must be given up, reasoning on spectroscopic lines; the observations I have already made on several minerals indicate that the gases composing the mixtures are by no means the only ones we may hope to obtain.

This part of the inquiry will be more specially considered in a subsequent communication.

I may remark in conclusion that in this preliminary inquiry no attempt has been made to separate the possibly new gases from the known ones which come over with them; hence, the lines are in some cases very dim, and the application of high dispersion is impossible. The wave-lengths, therefore, especially in the visible spectrum, are approximations only; but the view that we are really dealing with gases operative in the solar atmosphere, like the helium which produces D_3, is strengthened by the fact that of the 60 lines so far recorded as new in the various minerals examined, about half occur near the wave-lengths assigned to chromospheric lines in Young's table. I am aware that most of the chromospheric lines have been recently referred to as due to iron, but I believe this result does not depend upon direct comparisons, and it is entirely opposed to the conclusions to be drawn from the work of the Italian observers, as well as from my own.

[1] NATURE, vol. li. p. 544.
[2] *Comptes rendus*, April 16, p. 835.

ON THE NEW GAS OBTAINED FROM URANINITE.[1]

In my preliminary note communicated to the Royal Society on the 25th ult. I gave the wave-lengths of the lines which had been observed both at reduced and at atmospheric pressure in the gas (or gases) produced by the method to which I then referred of heating the mineral Uraninite (Bröggerite) in vacuo.

As a short title, in future I shall term this the distillation method.

Since then the various photographs obtained have been reduced and the wave-lengths of the lines in the structure spectra of hydrogen observed beyond the region mapped by Hasselberg.

I have further observed the spectra of other minerals besides Uraninite for the purpose of determining whether any of them gave lines indicating the presence of the gas in Uraninite or of other gases.

I now give a table of the lines so far measured in the spectra of 18 minerals between λλ 3889 and 4580 R, the region in which, with the plates employed, the photographic action is most intense.

Lines Photographed in the Spectra of Gases obtained from various Minerals experimented upon up to May 6.

Wave-length. Rowland.	Wave-length. Ångström.	Chromospheric lines (Ångström's scale.)	Eclipse lines (1893), Rowland's scale (1893).	Orion star lines (Rowland's scale).	Remarks.
3889	3888·5	3888·73 H.	3889·1	*	U
3947	3946·5	3945·2 H.	3946·0		U
3982	3981·5		3982·0		
4026·5	4025·9		4026·5	4026·5	U
4142	4141·3				
4145	4144·3		4144·0	4144·0	
4177	4176·3	4178·8	4177·8	4178·0	
4182	4181·3				
4338	4337·3	4338	*	4338·0	
4347	4346·3			4346·0	
4390	4389·3	4388·5	4390	4389·0	
4398	4397·3	4398·5	4398·7		
4453	4452·3		4454		
4471	4470·3	4471·2	4471·8	4471·8	U
4515	4514·3	4514·0	4514·5		
4522	4521·3	4522·0	4522·9		
4580	4579·3				

* Broad hydrogen lines extend over these positions.
U = lines noted frequently in the spectra of Bröggerite.
H = photographed by Hale.

On this table I may remark that, of the lines given in my paper of April 25, the final discussion has shown that the following lines are hydrogen structure lines in the region beyond that mapped by Hasselberg:—

λλ 4479, 4196, 4156, and 4152·5.

The line 4368 is also omitted from this list, as it has not been finally determined whether it coincides with a line of O.

In the table, besides the λλ on Ångstrom's and Rowland's scale, I give lines which have been observed in the sun's chromosphere and chronicled by Young; those photographed during the eclipse of 1893 with a 6-inch prismatic camera, by Mr. Fowler, and those photographed with the same instrument at Kensington in some stars of Group III. of my classification in the constellation of Orion.

This table carries the matter of the relation of the new gases to star and stellar phenomena much further than I ventured to suggest in my second note.

We appear to be in presence of the *vera causa*, not of two or three, but of many of the lines which, so far, have been classed as "unknown" by students both of solar and stellar chemistry: and if this be confirmed, we are evidently in the presence of a new order of gases of the highest importance to celestial chemistry, though perhaps they may be of small practical value to chemists, because their compounds and associated elements are, for the most part, hidden deep in the earth's interior.

The facts that all the old terrestrial gases, with the exception

[1] Third Note. By J. Norman Lockyer, C.B., F.R.S.

of hydrogen, are spectroscopically invisible in the sun and stars—though they doubtless exist there—and that these new gases scarcely yet glimpsed, have already, in all probability, supplied us with many points of contact between our own planet and the hottest part of our central luminary that we can get at, and stars like Bellatrix, are full of hope for the future, not only in relation to the possibility of more closely correlating celestial and terrestrial phenomena, but in indicating that a terrestrial chemistry founded on low density surface products in which non-solar gases largely enter, is capable of almost infinite expansion when the actions and reactions of the new order of gases, almost, it may be said, of paramount importance in certain stages of stellar evolution, shall have been completely studied.

With regard to the differences indicated between the results of the chromospheric and eclipse observations in the above table, it may be useful to remark that Prof. Young's "frequencies," invaluable though they are, must necessarily be of less importance, from the present point of view, than the eclipse observations obtained, it may almost be said, at the same instant of time. There may be, and doubtless are, two perfectly distinct causes for the appearance of the so-called chromospheric lines. First, the tranquil condition of the lower strata of the sun's atmosphere which gives us the pure spectrum produced at a constant—and the highest that we know of in the sun—temperature. Secondly, the disturbed condition which fills the spectrum with lines of a so-called prominence. Formerly it was universally imagined that the prominences were shot up from below; and in that case the lines added would indicate a temperature *higher* than the normal. But I have sent many papers in to the Society indicating the many arguments against this view,[1] and to me, at the present time, this view is almost unthinkable. If these disturbance-lines are produced from above, they may represent the effects of many stages of *lower* temperature. Hence a list of chromospheric lines loses most of its value unless the conditions of each observation are stated, and the phenomena appearing at the same place at the same instant of time are recorded.

Now, this same place and same time condition is perfectly met by eclipse photographs, and hence I attach a great value to them. But the comparison between such eclipse observations and the spectra of certain stars indicates that the latter in all probability afford the best criteria of all.

[1] They are set out at length in the "Chemistry of the Sun," which I published in 1887.

THE MARQUIS OF SAPORTA.

IN the study of palæobotany we may concern ourselves with the various problems of distribution, the geologic sequence of plant types, the value of fossil plants in comparative stratigraphy, and as tests of climatic conditions; or our attention may be concentrated on the important facts revealed by a microscopic study of petrified plant tissues. The latter field of research, in which Prof. Williamson has laboured with remarkable success during the last twenty-five years, is gradually being recognised by botanists as a branch of their science which they cannot afford to neglect in dealing with the wider problems of plant life. Fascinated by the almost incredible perfection in which Palæozoic, and more rarely Mesozoic, species have been preserved, the student of vegetable morphology is apt to take too little heed of the wealth of material which can only be studied in the form of structureless casts or impressions. In the majority of fossil floras the geologist or botanist must perforce confine himself to an examination of the few isolated and imperfect fragments that have escaped destruction in the process of denudation and rock-building, and have been preserved by fossilisation as meagre representatives of a past vegetation. As a specialist in this latter branch of palæobotany, there has been no more ardent worker since the days of Adolphe Brongniart, whom we may regard as the founder of palæobotanical science, than the Marquis of Saporta. Saporta's recent death, at his home in Aix-en-Provence, at the age of seventy-two, has deprived botanical and geological science of an unusually able and vigorous worker.

A perusal of Saporta's numerous contributions to scientific literature affords abundant evidence of critical and detailed investigation during a long period of years; nearly the whole of his published work has been in the domain of fossil botany. The Tertiary vegetation of France forms the subject of several of his contributions to science. From an early stage of his career the Cainozoic plant-bearing strata of Provence have occupied a prominent position in his palæobotanical studies: the Eocene flora of Aix, a valuable monograph on the remnants of an Eocene flora preserved in the tuffs of Sézanne, and various other writings on Tertiary plants, bear eloquent testimony not only to a remarkable power of detailed systematic work, but to a striking aptitude for a broad and philosophic manner of treatment. Students of Mesozoic botany soon learn to appreciate Saporta's memoirs on Cretaceous and Jurassic plants, and especially the splendid series of monographs on the Jurassic flora of France, published as separate volumes of the "Paléontologie Française" from 1873-91; in this profusely illustrated work, dealing primarily with French vegetation, we have to a large extent a general handbook of Oolitic botany. One feature which sets a high value on Saporta's palæobotanical work, is his wide and thorough acquaintance with the facts of distribution and taxonomy of living plants. Palæontological records are often in themselves of no special interest to zoologists and botanists, but if interpreted as indices of plant distribution in past ages, and applied to the wider problems of the evolution and dissemination of plant types, they assume considerable importance. Saporta's knowledge of recent floras, and his keen enthusiasm as an evolutionist, led him to regard fossil plants not simply as convenient aids to the stratigraphical geologist, but as affording indispensable data to the student of plant phylogeny. In "Le Monde des plantes avant l'apparition de l'homme" (Paris, 1879), we have a series of articles originally published in the *Revue des Deux Mondes* and *La Nature*, in which Saporta's encyclopædic information and finished literary style combine to render attractive to the layman and the specialist a retrospect of plant life during the geologic ages. Unfortunately the elaborate frontispiece to this volume, described as the "oldest known land plant," and named *Eopteris Morierei*, is merely a representation of an iron pyrites infiltration on the surface of a Silurian slate, and cannot be retained as a plant impression. In a more recent and smaller volume, "Origine paléontologique des arbres cultivés ou utilisés par l'homme" (Paris, 1888), we have an interesting sketch of the geological history of existing forest trees; and in another and more ambitious work,[1] in collaboration with Prof. Marion, an attempt is made to follow the lines of descent of the several subdivisions of the vegetable kingdom. The palæobotanist who is bold enough to venture on the task of tracing out the ancestry of plant forms, and of attacking the problems of development, is exposed to the very serious danger of allowing unsound links to form part of his chains of life. Saporta's constant desire to treat fossil plants from the point of view of a sanguine evolutionist, who wishes to press into his service all possible pieces of evidence towards the better understanding of the process of plant evolution, has in certain instances been led beyond the limits of accurate scientific reasoning. The majority of the so-called fossil algæ, to which he has devoted considerable attention, have been put out of court by Nathorst and others, as having no claim to consideration as records of thallophytic life; and it is generally agreed that the value of his work in this direction is seriously discounted, by the more than doubtful specimens which are described as vestiges of the lower and more primitive forms of plants. A few months before his death, Saporta completed an exhaustive monograph on

[1] Saporta and Marion: "L'évolution du règne végétal." 3 vols. 1881-1885.

the Mesozoic flora of Portugal;[1] this work marks an important advance in our knowledge of Lower Cretaceous and Upper Jurassic vegetation; and of special interest are the various forms of "archetypal angiosperms" closely resembling similar fossils from the Potomac beds of North America. This last monograph, full of elaborate botanical and stratigraphical work, affords a striking proof of the energy and youthful enthusiasm of the veteran student. Saporta's name will ever be held in respect by succeeding generations as that of a pioneer of palæobotanical science; and by those who were privileged to know him personally, or as a correspondent ever ready to render assistance to younger workers, the death of the Marquis of Saporta must be felt not merely as the termination of the labours of one of the foremost palæobotanists, but as the removal of a generous friend and colleague, whose wide knowledge and untiring devotion to science will stimulate younger investigators to more vigorous efforts in the rich field of palæobotanical study.

A. C. Seward.

SIR GEORGE BUCHANAN.

The death of Sir George Buchanan removes from our midst a leader in that branch of medical science which concerns itself with the prevention of disease. His death came very unexpectedly, for the circumstances of his ill-health were known only to a circle of intimate friends; and his great desire to go on working as long as work was practicable, made him sufficiently cheerful to disguise the suffering which he at times experienced. It is some three years since he resigned the post of medical officer to the Local Government Board, this step having been taken by him on account of failing health. But he still found plenty of pleasurable occupation in connection with the various learned and scientific bodies with which he was associated, and he also served on the Royal Commission on Tuberculosis, of which he became chairman on Lord Basing's death. He was a pupil of University College, of which body he became a Fellow; he graduated B.A. and M.D. at the University of London, and at his second M.B. he distinguished himself by carrying off several gold medals and scholarships. Later on he became medical officer of health to the district of St. Giles, where he laboured hard for years to improve the conditions of public health and to amend the then terribly faulty circumstances under which the people lived. It was here that he attracted the attention of Sir John Simon, then medical officer of the Privy Council, and under him he served both as a temporary and, later on, as a permanent medical inspector. During this period, and subsequently when he himself directed the public health department of the State, the investigations which he carried out, and the reports which he presented to Parliament, embodied the results of work of which England may feel proud. As a type of the class of work we refer to, we may instance his prolonged investigations into the influence on health of large public works, of water-supply and sewerage, and his discovery of the lessening of mortality from pulmonary consumption wherever the construction of sewers had led to a lowering of the sub-soil water. Some of his papers on the subject of vaccination in relation to small-pox are also of the greatest value; they were the result of most careful labour, as well as of an earnest desire to eliminate all possible sources of error, and to arrive at the truth alone; and the more he studied the subject, the more convinced he became of the value of vaccination as a measure of public health. He sought to secure for all the work he did or supervised a truly scientific basis; and he always attached the greatest importance to the auxiliary scientific work for which a special, but only a small, grant is annually made to the medical department of the Local Government Board. He had a marked literary talent, and a conspicuous power of setting out the salient points of the work done by his inspectorial staff; with the result that his annual reports have gradually come into great demand by sanitarians and public health authorities in almost every part of the world. The result of all his labours is by no means accomplished, in some places work on the lines he has indicated has hardly commenced, and it must almost necessarily be that much that he has taught, will, in the lapse of time, fail to be associated with his name. But those who know the nature of his work, and who appreciate the thoroughness which always characterised it, will readily understand how far-reaching and beneficial the results must in the end be. In 1882 he was elected to the Senate of the University of London, and in the same year he was made a Fellow of the Royal Society; but otherwise distinctions came to him mainly at the close of his official career. This was doubtless largely due to all absence of self-seeking in his character. As head of a department he was always trying to promote the welfare of those under him, and it was only when he retired on a comparatively small pension that he asked for some consideration in view of the long services he had rendered to the State before he gave his whole time to his official duties. But the Treasury gave their usual answer, and he said no more. At this date he was made a Knight Bachelor, and in 1893 he received the honorary degree of LL.D. of the University of Edinburgh. He was a past President of the Epidemiological Society, a Censor of the Royal College of Physicians of London, and he acted as adviser in scientific and other matters to several other bodies. If such a characteristic can be deemed a fault, Sir George Buchanan's most prominent failing was an inability to conceal his sense of those who, as he thought, sacrificed principles and, at times, the truth itself in matters relating to the advancement of public health, for purposes of notoriety or of policy. But, on the other hand, no chief of a public department ever won the affection as well as the esteem of his staff better than Sir George Buchanan did; and he made it no secret that in regard to this he was always desirous to recall the example of his own former chief, who, happily, still lives, and to whom he was devotedly attached.

[1] "Flore fossile du Portugal" (Direction des travaux géologiques du Portugal). [illegible]

NOTES.

Our readers will be glad to know that Prof. Huxley continues to improve in health. A telegram received from Eastbourne as we go to press states that he is progressing favourably, and is able to get up daily, but is hardly strong enough yet to leave his room.

The Bill, which was introduced into the House of Lords on Thursday last by Lord Playfair, on behalf of the Government, may be fairly said to bring the reconstruction of the University of London on the lines of the Gresham Commissioners' Report within the sphere of practical politics. The exact terms of the Bill have not yet transpired, but it is understood that the four Commissioners appointed to administer the Act are, in the first place, empowered to make modifications in the scheme if deemed expedient after consultation with the Senate and Convocation of the University of London, and other bodies affected; and in the second, enjoined to adequately safeguard the interests of the external or non-collegiate students. The Government having at last taken action on this question, it is the more satisfactory to note that the attempt made in Convocation on Tuesday last to rescind the resolutions passed at the January meeting (vol. li. p. 298), has completely failed, a resolution to the effect that "if a local Teaching University for London be desirable, it ought to be constituted apart from the existing University of London," being rejected by 238 against 117, or by a majority of 121 votes.

THE unveiling of a memorial tablet to the late Prof. J. C. Adams at Westminster Abbey, on Thursday last, was an event in which all men of science are interested. It might have been made a great occasion, for Adams' name is esteemed throughout the scientific world, instead of which the meeting seems chiefly to have represented the University of Cambridge. The tablet has been placed in the north aisle, close to the graves of Newton, Herschel, and Darwin. It is the work of Mr. Bruce Joy, and bears the following inscription :—"Johannes Couch Adams, Planetam Neptunum Calculo Monstravit. MDCCCXLV."

A BILL incorporating the New York Zoological Society, and providing for the establishment of a zoological garden in New York, has just been approved by Governor Morton. The Act provides that the corporation shall have power to establish and maintain in New York City a zoological garden for the purpose of encouraging and advancing the study of zoology, original researches in the same, and kindred subjects, and of furnishing instruction and recreation to the people.

ON April 26, the Linnean Society of Bordeaux held a meeting devoted to the question of bibliographical reform. The prospectus of the new Bibliographical Bureau for Zoology was approved by all the members present, and the wish was expressed that a similar organisation be at once attempted for the other branches of natural science. In accordance with this wish, it was decided to elaborate a project for the establishment of a Central Bureau for Botany. This project will be presented to the Association Française at its next meeting, by the President of the Botanical Section. M. Mourlan, the Director of the Académie des Sciences of Belgium, proposes similar action for geology. It is hoped that, by the establishment of several federated bureaus, the plan of the Royal Society may be fully realised and without great difficulty. Meantime, the organisation of the Zoological Bureau has made considerable progress, the circular of the French Commission has already appeared, and has been widely distributed by the French Zoological Society ; the American Commission has completed its preliminary study, and will soon send its circular to press. In other countries, notably in Russia, similar progress is reported.

THE programme of arrangements for the Ipswich meeting of the British Association has just been issued. The first general meeting will be held on Wednesday, September 11, when the Marquis of Salisbury will resign the chair, and Sir Douglas Galton, President elect, will assume the presidency, and deliver an address ; on Thursday evening, September 12, a soirée will be held ; on the following evening a discourse will be delivered by Prof. Silvanus P. Thompson on magnetism in rotation ; on Monday evening, September 16, there will be a discourse by Prof. Percy F. Frankland on the work of Pasteur and its various developments ; a second soirée will take place on Tuesday evening, September 17, and the concluding general meeting will be held on Wednesday, September 18. The Sections and their Presidents are as follows :—(*a*) Mathematical and Physical Science—President, Prof. W. M. Hicks, F.R.S. (*b*) Chemistry—President, Prof. R. Meldola, F.R.S. (*c*) Geology—President, W. Whitaker, F.R.S. (*d*) Zoology (including Animal Physiology)—President, Prof. W. A. Herdman, F.R.S. (*e*) Geography—President, H. J. Mackinder. (*f*) Economic Science and Statistics—President, L. L. Price. (*g*) Mechanical Science—President, Prof. L. F. Vernon Harcourt. (*h*) Anthropology—President, Prof. W. M. Flinders Petrie. (*k*) Botany—President, W. T. Thiselton-Dyer, C.M.G., F.R.S. Section I (Physiology) will not meet at Ipswich, but papers on animal physiology will be read in Section D. The delegates of corresponding Societies will meet on Thursday, September 12, and Tuesday, September 17, under the presidency of Mr. G. J. Symons, F.R.S. The acceptance of papers is, as far as possible, determined by organising committees for the several Sections, before the beginning of the meeting. It has, therefore, become necessary, in order to give an opportunity to the Committees of doing justice to the communications, that each author should forward his paper, together with an abstract, on or before August 12, to the General Secretaries of the Association.

SEVERAL summer schools for the practical study of botany will be held during the coming season in the United States—one in connection with Cornell University, and one in connection with the University of Wisconsin, both from July 8 to August 16 ; also one in connection with the Cambridge Botanical Supply Co., Cambridge, Mass., from July 5 for five weeks.

THE *Sitzungsberichte* of the Vienna Academy of Sciences (vol. civ.) contains a discussion of the observations of atmospherical electricity and St. Elmo's Fire on the Sonnblick by Messrs. J. Elster and H. Geitel, being a continuation of the observations to the time of the change of the former observer. The results confirm those previously obtained, and show that the yearly variation of the electrical energy at the summit is small, compared to that at the base, and that the summit of the mountain projects above those strata of the atmosphere in which electrical processes mostly occur. During the fall of fine snow the St. Elmo's Fire is mostly negative, but positive when large flakes of snow and hail are falling.

FROM a paper on early agriculture in Palestine, by Dr. H. Vogelstein, we learn the interesting fact that in the first two centuries of the Christian era, rainfall was measured by means of a receptacle. The Jewish *Mishnah* refers to two seasons, one wet and the other dry. In normal years the early rain fell soon after the autumnal equinox, and its importance to agriculture is frequently referred to in that document. The amount which fell at this season was about 21 inches, which agrees fairly well with the present measurements at Jerusalem, but the total annual fall is not stated by Dr. Vogelstein. Further particulars of this interesting communication will be found in the *Meteorologische Zeitschrift* for April.

PROF. L. H. BAILEY, of Cornell University, Ithaca, N.Y., has recently read before the Biological Society of Washington a paper entitled the "Plant-individual in the Light of Evolution." In this paper, according to the *American Naturalist*, he suggests the idea that both Lamarckism and Darwinism are true, the former finding its expression best in animals, the latter in plants. The plant is, according to him, not a simple autonomy, in the sense in which the animal is, and the parts of the plant are independent in respect to propagation, struggle for existence, and transmission of characters. According to this view there can be no localisation or continuity of germ-plasm in plants, in the sense in which these conceptions are applied to animals.

THE *El Universal* reports that the cold spell in February extended right down the Gulf of Mexico to Vera Cruz. On the 15th and 16th it was freezing over a distance of 80 leagues from Monterey to Ciudad Victoria and Tula in Tamaulipas, and the mountains were covered with snow. In the district of Tancanhuitz, State of San Luis Potosi, the sugar-canes and coffee-trees were all killed, the value of the coffee crop destroyed being estimated at a million dollars. In the Huasteca, State of Vera Cruz, sugar-canes, coffee, and tobacco were similarly killed—a loss of several million dollars—while cattle were dying by hundreds on the frost-bitten pasture lands. Owing to the frost having followed a prolonged drought, prices had risen to famine rates, and there was much sickness, especially croup and small-pox. In the district round Altotonga a very hot south wind set in on February 13, which suddenly cooled, and grew in intensity and cold. On the 14th, snow began to fall and did not cease till the 17th. Ten parishes in the temperate zone were snow-covere

for eighty-four hours, resulting in the destruction of all fruit, vegetables, coffee, and tobacco. The sugar-canes were so ruined as to be unfit even for forage. The twelve parishes of the district situated in the *terra fria* lost everything; the maize had not yet been planted, and would not be ripe till November or December. At Papantla, the vanilla centre, it was snowing on February 17, and the temperature had fallen from 30° C. to freezing point. At Misantla snow fell all night, and many fowls, animals, and cattle died from the cold.

Under the title, "Illustrations of Darwinism, and other Papers," Sir Walter L. Buller, F.R.S., has sent us a reprint of his presidential address to the Wellington Philosophical Society in 1894. Its main subject-matter is a discussion of the various ways in which the peculiarities of structure, colour, distribution and habits of New Zealand birds, serve to illustrate the theory of Natural Selection, and often to afford very strong arguments in its favour. The address is very clear and forcible, full of interesting facts and suggestive observations, and will be read with interest by all naturalists. One or two points only call for any critical observation. Sir W. Buller objects to the Apteryx being classed by Mr. Wallace as among "the lowest birds," because, he says, it is really "an extremely specialised form." But surely the Ratitæ are lower than the Carinatæ; and the Apteryx is specialised so as to be almost the least bird-like of the Ratitæ. If it is not to be classed among the lowest existing birds, where are these to be found? Again, the statement that the larger forms of animals have universally preceded the smaller in geological time (p. 101), is only a half-truth, if so much, since all these large forms have been developed from smaller ones, as shown in the case of the horse, as well as that of the early marsupials of the Meszoic period. Even more open to objection is the statement (p. 102), that the Siberian mammoth "would clearly have required a growth of tropical luxuriance to satisfy the wants of its capacious stomach"; and that its being found by thousands embedded in ice or frozen soil implies "a revolutionary change of climate." A sufficient answer to which theory is the fact that leaves and cones of firs have been found in the stomach, showing that it fed only a few degrees south of the places where it is now embedded.

A valuable addition to the various suggestions for the measurement of geological time is made by Dr. G. K. Gilbert in the *Journal of Geology* (vol. iii. No. 2). He has been struck with the regular, rhythmical cycles of sedimentation displayed over and over again by the shaly beds of the Cretaceous of Colorado (Benton, Niobrara, and Pierre groups). Such regularity, he suggests, can only be due to causal variations of a periodic character, and only astronomical changes have the regularity required. There seem to be only three astronomical cycles that can be reasonably appealed to for an explanation of rhythm in sedimentation: the periods of the earth's revolution around the sun, of the precession of the equinoxes, and of the variation in eccentricity of the earth's orbit. Dismissing the first as too short, and the last as too irregular, Prof. Gilbert considers there are three ways in which the second cycle might influence local sedimentation: (1) By periodic changes in winds, and therefore in marine currents; (2) by alternate glaciation of the two hemispheres, resulting in periodic advance and recession of coast-lines, and hence of sedimentation-boundaries; (3) alternation in terrestrial climate of moist periods—when, through the abundance of vegetation, chemical denudation would be at a maximum, and mechanical at a minimum—and dry periods, when the reverse would be the case. Assuming the rhythm of sedimentation in the case considered to coincide with the rhythm of the equinoxes, Dr. Gilbert estimates the time represented by the Benton, Niobrara, and Pierre epochs as 20,000,000 years, or, allowing the number 2 as a factor of safety, between 10,000,000 and 40,000,000 years.

We have received the Supplement to the Calendar of the Royal University of Ireland for 1895, containing examination papers set last year.

So little attention is generally paid in public libraries to the wants of students of science, that we are glad to give a word of praise to a catalogue of books on mathematics, mathematical physics, engineering and architecture, contained in the two public libraries at Halifax. The list has been compiled by the librarian, Mr. J. Whiteley, and it should be found a useful guide to the scientific literature in the two libraries.

The *Bulletin* of the American Museum of Natural History (vol. vi.) has been received. Among the articles in the volume, we notice one "On the Birds of the Island of Trinidad," by F. M. Chapman; "On the Seasonal Change of Colour in the Varying Hare (Lepus Americanus)" by J. A. Allen; "Fossil Mammals of the Lower Miocene White River Beds," by H. F. Osborn and J. L. Wortman. There are also papers on North American Orthoptera and Moths, by W. Beutenmüller; on some North American Mammals, by J. A. Allen, and by F. M. Chapman; and on new forms of marine algæ from the Trenton limestone; by R. P. Whitfield.

The authorities of the Royal Gardens, Kew, publish a "Hand-list of Ferns and Fern-allies cultivated in the Gardens." This remarkably rich collection consists of 802 species and varieties of ferns, and 48 of fern-allies and natives of this country; besides no less than 586 varieties of British ferns. This latter collection is due to the bequest of Mr. W. C. Carbonell, who left it to the Gardens. It consists of 4261 specimens, found by him at Rhiew Castel, Usk, Monmouthshire. The rest of the collection owes its completeness largely to the zeal and assiduity of the late Mr. John Smith, curator of the Gardens from 1841 to 1863.

The text of a series of six Lowell lectures, by Prof. Gantano Lanza, on "Engineering Practice and Education," which has been appearing in the *Journal* of the Franklin Institute since May 1894, is now concluded. Some interesting examples are given of the engineering works of the world, and the functions of the engineer are passed in review. Prof. Lanza holds sound ideas as to the education of an engineer. "There are two things," he says, "which are absolutely necessary to make a successful engineer: first, a knowledge of scientific principles and of the experience of the past; and second, his own experience. . . . The two fundamental sciences upon which the scientific principles of engineering are especially dependent are mathematics and physics, and no proper course in engineering can be arranged without insisting upon these as fundamentals." He shares the general opinion that the education of the engineer should include some knowledge of the differential and integral calculus, if not of higher mathematics.

We have often found occasion to express satisfaction at the work carried on by many local scientific societies. Labourers in the field of science are not wanting, but their work frequently needs direction. Wisely organised, the multitude of willing amateur observers can greatly assist the growth of natural knowledge. A programme just received from the Yorkshire Naturalists' Union, showing the excursions, meetings, and committees of research for 1895, is a sufficient proof that the operations of the Union are conducted with definite objects in view. There is a boulder committee, appointed to collect information as to the distribution of erratic blocks in the county of York; a committee to observe the present changes and past condition of the sea-coast, in order to determine the rate of erosion; a fossil flora committee, which aims particularly at determining the vertical range of the genera and species of the various formations; a geological photographs committee; a

committee to promote the investigation of the marine zoology of the Yorkshire Coast; a micro-zoology and micro-botany committee; a committee to consider proposals for the legislative protection of wild birds' eggs; and a committee having for its object the investigation of the mycological flora of Yorkshire. Upon each of the committees we notice the names of numerous well-known scientific workers; and, as the committees co-operate, when possible, with British Association committees, the Union forms the connecting link between the local societies and the Association. This kind of organisation seems to be the one calculated to produce the greatest amount of useful work. While referring to natural history societies, we may mention that the West Kent Natural History, Microscopical, and Photographic Society has sent us their report for 1894-95. The report contains an address by the President, Mr. H. J. Adams, on "Colour Photography," and a paper on "The Birds of Blackheath," by Mr. H. F. Witherby.

H. MOISSAN has attempted to produce argon compounds by acting on argon, under various conditions, with some of the rarer elements which unite more or less readily with nitrogen (*Comptes rendus*, May 6). 100 c.c. of the new gas were placed at his disposal by Prof. Ramsay. In a part of this, titanium, boron, and lithium were strongly heated without apparent change. Similarly, uranium (containing 3½ per cent. of carbon) did not absorb an appreciable amount of the gas when heated with it for twenty minutes. A quantity of the gas was conducted into a platinum tube of special construction, and there exposed to the action of pure fluorine, both at the ordinary temperature and in presence of induction sparks; in neither case could any reaction be observed whatever the proportion of argon present. The difficulty of manipulating fluorine has not allowed of the effect of long-continued sparking being observed. The results were entirely negative; under the conditions of these experiments, no compounds of argon have been produced.

BY saturating an ethereal solution of ferric chloride with nitric oxide, and concentrating the product at the ordinary temperature in the vacuum desiccator, V. Thomas has succeeded in obtaining crystals of the composition $FeCl_2.NO.2H_2O$. (*Bull. Soc. Chim.* [3], xiii.-xiv. No. 8). The anhydrous compound may be obtained in smaller yellow crystals by crystallisation at 60° on a porcelain plate. Peligot found that nitric oxide dissolved in ferrous chloride solution in the proportion required to form a compound $2FeCl_2.NO$, and this solution lost all its gas on heating. It is interesting and significant that the new crystalline product dissolves completely in cold water without evolution of gas to form a pale yellow solution, and that the solid compound is quite stable in vacuo at the ordinary temperature. Of considerable interest also is the observation by the same author, that nitric oxide gives abundant crystalline precipitates when passed through solutions of antimony tribromide or antimony trichloride.

A NEW series of iron nitrosocompounds have been discovered, by K. A. Hofmann and O. F. Wiede, which possess interest both from the point of view of the gas-analyst and in consequence of the example they afford of the synthetical production of complex inorganic substances. A current of nitric oxide is passed through a concentrated solution of 200 grams ferrous sulphate and 300 grams of potassium thiosulphate. A compound is precipitated in red-brown leaflets, which has the composition $Fe(NO)_2S_2O_3K.H_2O$. This substance may be dried in the vacuum desiccator without change. It is difficultly soluble in water, and dissolves in concentrated sulphuric acid without decomposition, giving an intensely greenish yellow coloured solution. Ammonium and sodium salts of similar composition and properties have also been prepared. The formation of the new acid, dinitrosoferrothiosulphuric acid, of which these salts are derivatives, is facilitated by the presence of an excess of ferrous salt. It may be considered that the essential reaction in its formation consists of a replacement of the group (KS_2O_3) by NO in ferrous potassium thiosulphate, viewing the latter as $KO_3S_2.Fe.S_2O_3K$. The displaced radical probably forms potassium tetrathionate which does not react further. Cobalt compounds, in which the cobalt replaces the iron in this series, can be obtained, though with much greater difficulty. The connection of these new substances with the tetra- and hepta-nitroso compounds of Pawel and Marchlewski and Sachs is yet under investigation.

THE additions to the Zoological Society's Gardens during the past week include two Arabian Baboons (*Cynocephalus hamadryas*, ♂ ♀) from Somaliland, presented respectively by Mr. Francis G. Gunnis and Mrs. E. Lort Phillips; a Japanese Ape (*Macacus speciosus*, ♂) from Japan, presented by Dr. G. L. Johnston; a Rhesus Monkey (*Macacus rhesus*, ♂) from India, presented by Messrs. A. S. and E. Boatfield; a Naked-footed Owlet (*Athene noctua*), European, presented by Mr. Walter Chamberlain; a Black Tanager (*Tachyphonus melaleucus*) from Brazil, presented by Mr. Edward Hawkins; a Hawfinch (*Coccothraustes vulgaris*), British, presented by Mr. H. G. Devas; two Common Peafowl (*Pavo cristatus*, ♂ ♀) from India, presented by Mr. L. G. Whatman; two Pyrenean Newts (*Molge aspera*) from Lac d. Oncet, Pyrenes, presented by Dr. Jacques de Bedriaga; two Indian Pythons (*Python molurus*) from India, presented by Mr. G. Stephen; a Koodoo (*Strepsiceros kudu*, ♀) from Somaliland, a Kinkajou (*Cercoleptes caudivolvulus*, ♀), a Ring-tailed-Coati (*Nasua rufa*) from Brazil, a Dusky Bulbul (*Pycnonotus obscurus*) from Morocco, deposited; two Ruddy Sheldrakes (*Tadorna casarca*, ♂ ♀), European, a Red-fronted Amazon (*Chrysotis vittata*) from Porto Rico, a Yellow-fronted Amazon (*Chrysotis ochrocephala*) from Guiana, purchased; a Large Red Flying Squirrel (*Pteromys inornatus*) from India, received in exchange; two Japanese Deer (*Cervus sika*, ♂ ♀), a Barbary Sheep (*Ovis tragelaphus*, ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

ALGOL.—The periodic variations in the intervals between the minima of Algol have been explained by Dr. Chandler by supposing that the bright star, with its eclipsing companion, revolves round a distant centre of gravity determined by its relation to another dark body. M. Tisserand, however, considers that the phenomena may be produced by the changes in the line of apsides due to a polar compression of Algol (NATURE, vol. li. p. 328). The latter hypothesis requires that considerable variations in the duration of the minima should be observed; while, on Dr. Chandler's hypothesis, there should be a periodic inequality of the proper motion of Algol. Prof. Lewis Boss has recently discussed the question from the point of view of the proper motion of the star; but since the coefficient is probably less than 0″·7, the investigation is a very delicate one. Taking the result of his computation, apart from any considerations of the behaviour of Algol as a variable star, Prof. Boss is of opinion that there is a preponderance of probability in favour of the real existence of a periodic element in the proper motion, such as is required by Dr. Chandler's hypothesis. Supposing them to be real, they indicate that the apparent motion is in an ellipse, the semi-axis major of which is 0″·522 ± 0″·103 and the semi-axis minor 0″·224; the position angle of the northern end of the major axis is 34, and the inclination of the real orbit to our line of sight is 23°. The computation further indicates that the star passed the major axis of the apparent orbit within three or four years of the most probable date derived from the observated light-changes. Prof. Boss considers the evidence in favour of Dr. Chandler's hypothesis to be sufficient to justify a very thorough investigation of the meridian observations, as well as continued determinations of position. (*Astronomical Journal*, No. 343.)

PARALLAX AND ORBIT OF η CASSIOPEIÆ.—Twenty-seven photographs of the region round this star, taken by Dr. Rutherford

more than twenty years ago, have been utilised by Mr. H. S. Davis for the determination of the parallax (*Astronomical Journal*, No. 343). Six pairs of comparison stars were employed, and the combined results give the value $0''.465 \pm 0''.044$, corresponding approximately to a distance of η Cassiopeiæ from the earth of 43,113,000,000,000 miles, or $7\frac{1}{3}$ light years. Though the new value exceeds previous ones, it is not considered improbably large if the Rutherford plates are subject to no systematic error. Using Grüber's values of the orbital elements, the combined masses of the components is two-tenths as great as that of the sun, and the distance between the components 19 astronomical units, the relative orbit thus being about the same size as that of Uranus. These numbers, however, may require some modification, as Dr. See has recomputed the elements of the orbit, with the results slightly differing from those adopted by Mr. Davis. Dr. See states that during the next ten years the position angle will increase from 204° to 251°, while the distance will diminish from $4''.52$ to $3''.33$.

A Belgian Astronomical Society.—A Société Belge d'Astronomy has been founded at Brussels. The object of the Society is to popularise astronomy and the sciences connected with it (geodesy, meteorology, terrestrial physics, &c.), and to encourage research into the domains of those branches of knowledge. The President of the Society is M. F. Jacobs, and among the Council are General Tilly, Prof. Dusausoy, Prof. Goemans, M. Lagrange, Prof. Pasquier, Prof. Rousseau, and M. Terby. Two of the Secretaries are M. Stroobaut and M. Vincent, both observers at the Royal Observatory, Brussels.

THE IRON AND STEEL INSTITUTE.

THE annual spring meeting of the Iron and Steel Institute was held on Thursday and Friday of last week, in the theatre of the Society of Arts, under the chairmanship of the new President, Mr. David Dale. The following is the list of the papers set down for reading :—

"On Metal Mixers, as used at the Works of the North-Eastern Steel Company," by Mr. Arthur Cooper.

"On the Effect of Arsenic upon Steel," by Mr. J. E. Stead.

"On the Iron Ore Mines of Elba," by Mr. H. Scott.

"On the Manufacture of Steel Projectiles in Russia," by Sergius Kern.

"On Ternary Alloys of Iron with Chromium, Molybdenum, and Tungsten," by James S. De Benneville, of Philadelphia.

The last two papers were taken as read. After the usual formal proceedings, the President presented the Bessemer medal, which had this year been awarded to Mr. H. M. Howe, of Boston, U.S.A. As Mr. Howe was not able to be present, Prof. Roberts-Austen accepted the medal on his behalf.

Mr. Dale next proceeded to read his inaugural address. Those who know the good work done by Mr. Dale in the conciliation of labour disputes will not be surprised to learn that the chief interest of the address was in the domain of economics rather than metallurgy. The address was none the less welcome on this account, as no class are more affected by disturbances in the labour market than the iron and steel makers. Mr. Dale showed very clearly the disastrous effects upon British trade of strikes and industrial disputes, and dwelt upon the ever-enlarging area of competition in the manufacturing markets of the world; for now we have not only the continental nations of Europe to contend with, but have to meet the products of the still cheaper labour of the far East.

Mr. Cooper's paper, though short, supplied a valuable contribution of knowledge to the practical steel maker. Uniformity of product is at once one of the most desirable and most difficult things for the steel maker to secure. No matter what care may be taken, the product of the blast furnace will vary in regard to those minute percentages of alloys which exercise so important an influence on the characteristics of the steel producer. Efforts have been made to equalise the analysis of the pig iron by mixing the ore, but these have been only partially successful. It is desirable, from an economic point of view, that molten iron should be taken direct from the blast furnace and used in the converter; but, in the basic process, the need of uniformity has prevented this course being followed. It has been therefore necessary to follow the original plan of running the molten iron from the blast furnace into pigs, and then remelting it in cupolas. In this way, by using the product of several furnaces, and by a system of careful analysing and selection, uniformity has been generally obtained. In spite of all care taken, however, there will be at times differences in the product of the cupolas, owing to irregularities in working which could not be guarded against, and it would frequently happen that though a standard mixture of pig might be charged into the cupola, the amount of silicon or manganese would vary considerably, owing to larger quantities of these metals being oxidised at one time than another. The mixer is a vessel in appearance like a large Bessemer converter. Into this the molten metal from the blast furnace is run, together with a certain amount of cupola iron in the case of the North Eastern Steel Company's works, with the plant of which the paper deals. The mixer is largely used in America, Mr. Carnegie stating during the discussion that at his works they were about to erect some of 600 tons capacity. The mixers, of which there are two at the North Eastern works, are each 150 tons capacity. For drawing the metal off into the ladle the mixer is tilted, swinging on trunnions like a converter, hydraulic machinery being provided for the purpose. In the discussion which followed the reading of the paper, many steel makers corroborated the account, given by the author, of the excellent results obtained by the use of the mixer.

The chief feature of the meeting was the reading and discussion of Mr. Stead's excellent contribution on the effect of arsenic in steel—a paper we should have described as exhaustive had it not been that the author states he is about to follow up the experiments of which he gives an account by further investigation in the same field. Mr. Stead commenced by a reference to the well-known memoir on the same subject, which Messrs. Harbord and Tucker contributed to the meeting of the Institute held in 1888. In that paper it was shown that a large quantity of arsenic is decidedly injurious to steel; and it has generally been thought that smaller quantities would be similarly injurious in a corresponding degree. Mr. Stead did not consider such an hypothesis necessarily sound, and determined to carry out the elaborate series of experiments, details of which are given in the paper. The results, as we have said, are of the utmost importance to steel makers, for arsenic and phosphorus are frequently bracketed in analyses, as the separation of the two is a long and tedious process. If small quantities of arsenic are not injurious, as would appear from Mr. Stead's investigations, phosphorus is undeniably a deleterious ingredient.

The general conclusions the author drew from his investigations were that between 0·10 per cent. and 0·15 per cent. of arsenic in steel for structural purposes does not have any material effect so far as mechanical properties are concerned. The tenacity is but slightly increased, the elongation is apparently not affected, and the reduction in area of the fractured test-pieces is practically equal to that of the same steel without the addition of arsenic. With 0·20 per cent. arsenic the difference, although slight, is noticeable in samples of acid open-hearth steel tried; but even in this case the only serious difference evidently caused by the arsenic is the inferiority of the bending properties of the pieces cut from the plates across the direction of rolling after they had been tempered. With larger amounts of arsenic the effect is decisive. When 1 per cent. is present the tenacity is increased, and the elongation slightly reduced. The bending properties of the steel are, however, fairly good. When the arsenic amounts to about $1\frac{1}{2}$ per cent. the tenacity is still further increased, and the elongation and contraction of area still further reduced, whilst the bending properties are poor. With 4 per cent. of arsenic the tenacity is increased, and the contraction becomes *nil*. The author points out, however, that the trials with steel containing the higher percentages of arsenic could not be considered quite satisfactory, because the ingots on which the experiments were made were of very small size, and consequently a small amount of work only could be put upon them before testing. Mr. Stead considered it would have been highly probable that had larger masses been dealt with the results would have been more satisfactory. The effect of quenching the steel, after heating to a red heat, in every case where arsenic was in large quantity, was to improve its bending property.

Hot working is not affected by even 4 per cent. of arsenic, such an alloy appearing to stand about as much heat without burning as a steel containing 1 per cent. of carbon. When heated below the burning point such material can readily be hammered and rolled, and appears to be as soft in that state as steel containing about ·05 per cent. carbon. From this the author considers it safe to conclude that arsenic has not the slightest tendency to produce red-shortness. Mr. Stead had

made experiments to ascertain the rate of corrosion of arsenical steel. He had submerged wires in a 2 per cent. solution of sal-ammoniac, had placed others in fresh water, and still another sample to a pile of the wharf at the Middlesbrough Ironworks in such a position as to be alternately covered and exposed as the tide ebbed and flowed. The conclusions arrived at were that arsenical steel is not more liable to corrosion than the same material without arsenical addition; in fact, oxidation is retarded by the presence of small quantities of arsenic.

It is in steel that is to be used in positions where it will require to be welded that arsenic appears most injurious, for that process is rendered more difficult by even very small quantities of arsenic; so that, as Mr. Stead says, when welding material is required, arsenic should be most carefully avoided. In regard to electrical conductivity, too, arsenic is injurious, for the value of the material in this respect is materially reduced by even small quantities of arsenic. A quantity equal to 0·25 per cent. diminishes the conductivity by about 15 per cent.

The paper concludes with an appendix in which the author gives a method he has worked out in detail for determining the arsenic in iron ores, in steel, and in pig iron. It has been the general practice to precipitate the arsenic as sulphide or hydric sulphide from the distillate, and either weigh the pure sulphide after drying at 212° F. or to oxidise it in bromine and hydrochloric acid, and then precipitate the arsenic acid with ammonia and magnesia solution, and weigh the precipitate produced. This process, although accurate, is tedious and takes at least twenty-four hours to complete. Mr. Stead has found that if the distillation is conducted in a special manner the whole of the arsenic may be obtained in the distillate, unaccompanied with any traces of chloride of iron, and that if the hydrochloric acid is nearly neutralised with ammonia and finally completely neutralised with acid carbonate of soda, the arsenic can be determined volumetrically with a standard solution of iodine, using starch solution as an indicator.

Émil Fischer proposed the process of distillation with ferrous chloride and titration of the distillate with iodine solution; but, as the details are not given in "Crookes' Select Methods," Mr. Stead had to work them out for himself. These he gives in full in his paper, to which we must refer our readers, as it would take too much space to describe the process in full. Mr. Stead says that a more simple and accurate device for the determination of small quantities of arsenic it would, he thinks, be impossible to conceive.

The discussion of this paper, although of an interesting nature, did not produce any new facts of importance. The majority of those who spoke were either steel makers or those interested in the production of steel, and they naturally congratulated themselves on the conversion of a long-supposed enemy into a neutral, if not into an ally. It should be remembered, however, that the meeting consisted chiefly of persons only too anxious to reduce the difficulty and cost of steel making; and not likely to accept any explanations tending to that end in a captious spirit. No one is likely to question the scientific accuracy or *bona fides* of so eminent and conscientious an observer and experimentalist as Mr. Stead, yet there may be something to say on the other side. This appears more likely from the remarks of the one user of steel who spoke—Mr. Wigham, the manager of a wire-drawing firm—who had made a report to Mr. Stead, which was quoted in the paper. It should be remembered, also, that Mr Stead himself says that further experiments are necessary.

The only remaining paper that was read was Mr. Scott's contribution on the Iron Mines of Elba. This was not discussed.

The autumn meeting of the Institute will take place in Birmingham, commencing Tuesday, August 12.

THE SCHORLEMMER MEMORIAL LABORATORY.

An interesting ceremony took place at the Owens College, Manchester, a few days ago, when Dr. Ludwig Mond formally opened the Schorlemmer Laboratory for Organic Chemistry, together with a large laboratory for medical students and a room for the preparation and storage of reagents. It may be remembered that, after the death of Prof. Schorlemmer, his friends and pupils, under the lead of Sir H. E. Roscoe, late professor of chemistry at the College, took steps with a view to fittingly commemorate his services to the College and to the advancement of organic chemistry. It was generally felt that the best memorial would be the erection of a laboratory for organic chemistry, to be called after his name, and a subscription list was accordingly opened. The appeal, which was generously headed by Dr. Mond, was so well responded to, both in this country and in Germany, that in a short time a sum of £2500 was subscribed. Meantime the Council of the College had to take into serious consideration the rapid growth of the chemical department. Originally designed for 100 students, the laboratories had for several years been overcrowded, and the private rooms built for research work had to be given up for the general instruction of the students. The number of the students in the chemical laboratories has steadily increased during the past five years, and, in view of this increase, the Council became convinced of the necessity of extending the chemical department. They accordingly accepted the fund raised by the Schorlemmer Memorial Committee, and instructed Mr. Alfred Waterhouse to prepare plans for a "Schorlemmer" Organic Laboratory, and for a new laboratory for elementary students, on a plot of land adjoining the present laboratories acquired by the College for the purpose of their extension. The Schorlemmer Laboratory, designed by Mr. Waterhouse, is at the end of the main corridor in the old chemical building. It measures sixty feet by thirty feet, and has an arched roof thirty feet high. The laboratory is designed to accommodate a professor, two demonstrators, and thirty-six students. It is fitted in the most complete manner with every requisite for the important work which is to be carried on within it, and in some particulars is arranged after the plan of the Munich organic laboratories. The lower laboratory is designed for forty-five students. The fittings are similar to those in the old laboratories designed by Sir Henry Roscoe. The total cost of the new building was £4800.

A full report of the opening ceremony is given in the *Manchester Guardian*, to which source we are indebted for the following condensed account:—

In connection with the inaugural proceedings, a large and representative company gathered in the Chemical Theatre of the College. Messages regretting inability to attend, and wishing prosperity to the laboratory, were received from a number of eminent chemists. Prof. H. B. Dixon referred to the esteem in which Schorlemmer's name was held, and expressed, on behalf of his colleagues and himself, their admiration of the life and character of the man to whose memory the laboratory had been erected.

Sir H. E. Roscoe sketched Schorlemmer's life, and, in the course of his address, said:—Schorlemmer added another name to the list of distinguished foreigners who had found a home in these islands. Never again could it be said that England failed to recognise and appreciate the value of the services of those who sought her shores. The names of Herschel, of Hofmann, of Max Müller, and, lastly, of Schorlemmer indicated that we are not slow to give honour to those who were once strangers in the land, but who had made themselves members of our national family. They might have good hopes that the time would soon come when the leaders in chemical industry would appreciate the necessity of a thorough scientific training, as had long been the case in Germany; and that as Giessen was, under Liebig, the means of raising the standard of chemical education throughout the Fatherland, so the chemical department of Owens College might, under the direction of Prof. Dixon and Prof. Perkin, the director of the new laboratory, be pointed out as the institution in England which had done the same for this great empire.

Dr. Ludwig Mond next addressed the meeting. He remarked that the opening of the first laboratory solely devoted to the study of organic chemistry, at the only University in England which could boast of a professor of that science, was a distinct step forward in the development of science in this country. He considered it a great step in advance to have a special laboratory and special professors appointed for the study of the chemistry of carbon, because the subject-matter of chemistry now covered so vast a domain, and was increasing at such an immense rate, that for any one desiring to further contribute to it, it had become a necessity, after mastering the main facts of the science, to give his attention specially to the details of one or other part of it. While it was true that carbon was only one out of many elements, it possessed such very special properties that the multitude of its compounds probably outnumbered those of all the rest of the elements together, and it had the unique interest that all the innumerable substances that were found in plants and animals, which built up their tissues, and by their constant changes produced the phenomenon we called life, were all

compounds of carbon. It was for this reason that they called the chemistry of these compounds organic chemistry, and it was very natural that that branch of their science should be nearer to their hearts than any other branch. But there was another and stronger reason for having special laboratories of organic chemistry. The methods of investigation and the way of analysing organic compounds differed considerably from those applied to inorganic chemistry. In the latter, if we had ascertained by an accurate analysis of a pure substance its percentage composition, that, together with the determination of a few simple physical properties, was usually sufficient to give us a perfect insight into its chemical composition and behaviour. The laboratory methods required for that study were simple and most of them well known, so that they could be acquired by sufficient experience. In organic compounds the matter was very different. The percentage composition and the physical properties told them very little of their chemical individuality and behaviour. Many substances of exactly the same percentage composition possessed widely different qualities, which were not explained by their physical properties. They must find out how these compounds, many of which were very complex, were built up. They had to unravel the structure of those substances to attain their end, which in chemical investigation always meant to give an explanation of all the various properties of a substance through its chemical constitution. To ascertain its structure they had to break the organic substance down by degrees, to take it gradually to pieces; and even that was not enough, but to make sure of the actual arrangement of those pieces in the substance they had to put them together again, to rebuild the substance from its proximate constituents, and only after having accomplished that could they consider that they knew its constitution. The methods employed in that work were entirely different from those of ordinary analysis. They were very manifold. The investigator had to make his own choice which of them to apply in any individual case, and wherever he broke new ground and undertook the study of a new series of compounds, he had to discover and work out new methods before he could achieve success. It was evident that a student who aimed at qualifying himself for such high-class work should enjoy special facilities, and should, after having gone through a regular course of analytical chemistry, have a chance of prosecuting special organic work in a laboratory fitted specially for it, and where he was undisturbed by the army of beginners who thronged an analytical laboratory. And there he might point out that in his opinion the reason why this country had not advanced in organic chemistry as fast as other countries, the reason why Hofmann's prediction in his report on the Exhibition of 1862 that "England will be unquestionably at no distant date the greatest colour-producing country in the world," had not been fulfilled, and that Germany had almost entirely taken this industry out of her hands, although it was inaugurated by an Englishman (Dr. W. H. Perkin), had been that so few English students of chemistry had devoted sufficient time to the prosecution of their studies. It was evident, therefore, in order to attain the necessary experience and certainty in carrying out original investigation in organic chemistry, that four to five years of close study and attention, under the leadership of a competent professor, were a necessity: and for carrying on successfully the manufacture of artificial colours it was indispensable that the chemist should be able to carry out independent original research because new colours had year after year to be discovered and manufactured, and the processes for their production had to be constantly improved in order to compete successfully with rival manufacturers. The success of an industrial enterprise depended not, indeed, upon the workman, not the foreman, as so many people in this country still believe, but upon the leading mind who directed the manufactory, who had a thorough grasp of scientific principles and had been trained to habits of scientific thought. He agreed that it was desirable to cultivate physical chemistry and inorganic chemistry much more than had been done, and he was very glad that the great supremacy which organic chemistry had enjoyed more particularly in Germany, the home of chemistry was now being contested by other and equally important branches of the science. But great, and very great, as had been the progress of organic chemistry, it had greater and more important problems still to solve; and in this country, which had given birth to so many of the most important steps in advance of that science, it had not received that amount of general attention which it had deserved in the past, and which it still deserved in the future. He therefore specially and heartily welcomed the opening of the first laboratory exclusively devoted to it in England. Prof. Schorlemmer, in his excellent and most suggestive little work "On the Rise and Development of Organic Chemistry," after giving a lucid review of the steps by which the great edifice of that science had been built up, gave in his concluding remarks a perspective of the problems still to be solved wide enough for the most expansive imagination of any searcher after truth. If to-day we still could not make morphine, quinine, and similar bodies artifically, the time was near at hand. If we could not make quinine, we had already found a partial substitute in antipyrine, and its introduction into therapeutics had lowered the price of quinine considerably. Another important problem was the synthesis of the ingredients of our daily food, such as sugar, gum, and starch. Those bodies were nearly related to each other, for we could convert the two latter into different kinds of sugar, and sugars again into gums. That the synthesis of sugar was imminent had already been stated. But it was quite different with those important parts of our food which had been called the albuminous bodies. Kekulé, in discussing the scientific aims and achievements of chemistry, brought forward the idea that if ever chemists should succeed in obtaining albuminoid bodies artificially it would be in the state of living protoplasm, perhaps in the form of those structureless beings which Haeckel called the "Monera." All attempts hitherto made for the purpose of producing living matter artificially had failed. The enigma of life could only be solved by the synthesis of an albuminous compound. Prof. Fischer, in a lecture delivered not long ago in Berlin, also expressed himself full of confidence that the time would arrive when we might attack successfully even the problem of the constitution and synthesis of the albuminoids, and might thus approach the problem of the origin of life. Surely with such a prospect before them as the ultimate result of the pursuit of organic chemistry, no amount of work, no amount of thought, no amount of time and trouble devoted to that study would be too much if it was well employed in leading successfully to the great end in view, although the goal might not be reached for generations to come.

The company afterwards adjourned to the new laboratory, which was declared open by Dr. Mond.

THE MIGRATIONS OF THE LEMMING.

UNDER the title "Myodes Lemmus, its Habits and Migrations in Norway," Prof. R. Collett, of Christiania, gives a valuable account of his researches into the habits and migrations of that interesting little rodent, the lemming, which has become so notorious on account of its periodic wanderings in vast hordes down the Scandinavian valleys. Prof. Collett finds the earliest notice of the lemming in an old Norse manuscript dating from the latter end of the thirteenth century, and reproduces a curious and striking woodcut from the great history of Olaus Magnus (1555), in which is graphically figured the descent of the lemmings from the clouds according to the prevalent belief. But the most valuable parts of the memoir are those which depend upon the author's personal knowledge of the lemming. The nature and habits of the lemming are clearly described, and much light is thrown upon the causes which from time to time lead such vast numbers of these animals to leave their native uplands and to begin their suicidal wanderings. The migrations seem to be directly due to over-population. In certain years, termed by the writer "prolific years," an abnormal fecundity is exhibited by the lemming; this phenomenon is not, however, confined to this species, but is shown also in numerous families of mammals, birds, and insects. The consequences of this great multiplication in the case of the lemming are thus described by Prof. Collett. "The enormous multitudes require increased space, and the individuals, which, under normal conditions, have each an excessively large tract at their disposal, cannot, on account of their disposition, bear the unaccustomed proximity of the numerous neighbours. Involuntarily the individuals are pressed out to the sides until the edge of the mountain is reached. In a short time they enjoy themselves there, and the old individuals willingly breed in the upper regions of the forests, where, at other times, they are entirely wanting. New swarms, however, follow on; they could not return, but the journey proceeds onwards down the sides of the mountains, and when they once reach the valleys, they meet with localities which are quite foreign to them. They then continue blindly on, endeavouring to find a home corresponding to

that they have left, but which they never regain. The migratory individuals proceed hopelessly on to a certain death." Sooner or later all the wanderers meet their death—thousands are drowned in rivers or fjords, thousands are attacked by beasts and birds of prey, and thousands perish from the effects of cold and damp; but the greater number die from the effects of a peculiar epidemic which attacks them in the lowlands. It is pointed out by the writer that the wandering instinct developed during migratory years is probably of distinct service to the species in reducing the surplus population.

THE AUSTRALASIAN ASSOCIATION.

WE gave, a fortnight ago, the presidential address delivered by the Hon. A. C. Gregory to the Australasian Association for the Advancement of Science at this year's meeting in Brisbane. Full reports of the proceedings in the different Sections have reached us, from the General Secretary, Mr. J. Shirley, but limits of space prevents us from printing more than a brief summary of them.

The public proceedings of the meeting were opened by a popular lecture on "Star Depths," by Mr. H. C. Russell. Mr. Russell traced the growth of knowledge concerning the distance of the stars, and the structure of the stellar universe, and illustrated his description by a selection from the excellent photographs of celestial scenery taken at Sydney Observatory.

We give a synopsis of the work of the various sections.

ASTRONOMY, MATHEMATICS, AND PHYSICS.

Mr. Alexander M'Aulay, as President of Section A, delivered an address "On Some Popular Misconceptions on the Nature of Mathematical Thought."

Mr. P. Baracchi, contributed a paper on "the most probable value and error of Australian longitudes, including that of the boundary lines of South Australia with Victoria and New South Wales." Dr. Ralph Copeland sent "Some Notes on the New Royal Observatory, Edinburgh," and Mr. H. C. Russell read a paper "On a Photographic Transit Instrument."

CHEMISTRY.

Mr. J. H. Maiden delivered the presidential address in this Section, entitled, "The Chemistry of the Australian Indigenous Vegetation." Mr. E. A. Weinberg contributed a paper on the refractory gold ores of Queensland: their sources and treatment. Prof. A. Liversidge, F.R.S., read a paper on "Variations in amount of Ammonia in Water on keeping." He also described the internal structure of some Australian nuggets, of different sizes, which had been closely examined and photographed. The etching was conducted according to the following plan:—A clean section was made and highly polished, and acted upon by chlorine water or bromine water, tincture of iodine or potassium cyanide, or sodium chloride mixed with nitric acid. The crystals less readily soluble stand up in relief and resemble the well-known figures seen in metallic meteorites when etched. One curious fact observed was that when the nuggets were subjected to heat, bubbles or blebs were formed on the surface, which burst with a sharp report, probably due to water included in the nugget being converted into high-pressure steam. Several beautiful photographs showing the crystalline nature of the nuggets were exhibited. Other papers read were: "On the Corrosion of Aluminium," and "Contributions to the Bibliography of Gold," by Prof. Liversidge; "Pharmacy as a Science and its Future," by Dr. W. Finselbach; "Notes and Analyses of some of the Artesian Waters of New South Wales," by John C. H. Mingaye; "On the Economic Treatment of Gold Ores," by Geo. H. Irvine; "Queensland Native Astringent Medicines," by Dr. Joseph Lauterer; "Portland Cement after Fifty Years," by W. M. Doherty; "Some Remarks on the Teaching of Elementary Chemistry," by A. J. Sach; "Analysis of Eucalyptus Gums," by Dr. Wilton Love; "The Ointments of the British Pharmacopœia," by F. W. Simmonds; "Notes on the Poisonous Constituents of Stephania Hernandiiafolia," by Prof. Edward H. Rennie; "Preliminary Notes on the Bark of Carissa Ovata, *R. Br.* v. Stolonifera, *Bail*," by H. G. Smith; "On a Method of Shortening certain Chemical Calculations," by W. A. Hargreaves.

GEOLOGY AND MINERALOGY.

Prof. T. W. Edgeworth David, in his address to this Section, reviewed briefly some recent geological discoveries of special interest. A paper by Mr. E. F. Pittman, Assoc. R.S.M., entitled "Notes on the Cretaceous Rocks in the North-western Portion of New South Wales," gave the results of a recent geological journey by him over 1150 miles of country. The geological examination was made chiefly with the object of determining approximately the area and boundaries of the artesian water formation.

Among other papers read before this Section were:—"Anticlines and Synclines and their Relation to Mining," by Ernest Lidgey; "On the Nomenclature of Crystals," by Prof. A. Liversidge, F.R.S.; "The Development and Progress of Mining and Geology in Queensland," by William Fryar; "On the Present State of our Knowledge of the Older Tertiaries of Southern Australia," by G. B. Pritchard; "The Antiquity of Man in Victoria," by W. H. Ferguson; "The Glacial Deposits of Victoria," by G. Officer, L. Balfour, and E. G. Hogg; "Notes on Tin Mining at Herberton," by John Munday.

BIOLOGY.

Prof. A. Dendy took for the subject of his presidential address, "The Cryptozoic Fauna of Australasia." Mr. F. M. Bailey read a paper on peculiarities of the Phanerogamic Flora of Queensland. The paper chiefly contained descriptions of indigenous fruits recommended for cultivation. Mr. D. Le Souef furnished a paper on the Tree Kangaroo (*Dendrolagus Bennettianus*), describing its mode of climbing, its food, and the way it is captured by the natives. In a paper on the eating of earth by the larger Macropodidæ, by J. Douglas Ogilby, evidence was given of the eating of earth by kangaroos in the Bourke district, New South Wales. This habit does not appear to have been previously recorded, though in the district under notice it plays no unimportant part in the economy of the larger marsupials.

Dr. M. C. Cook sent a paper entitled "Pestiferous Fungi and their Modes of Attack." Dr. Charles Chilton gave a general account of history, occurrence, distribution and habits of the blind Amphipoda and Isopoda found in the underground waters of the Canterbury Plains in New Zealand. Miss Lodder furnished a revised list of the Marine Mollusca of Tasmania. Some plants peculiar to the Burnett Basin were described by James Keys. In a paper entitled "Notes and Observations on the Genus Nephila," W. J. Rainbow dealt with—(1) the localities in which spiders of the genus Nephila abound; (2) the strength and elasticity of their webs, in the sticky meshes of which certain birds of weak wing-power are caught; (3) the question as to whether the Nephilæ eat birds thus captured; (4) the mode by which silk may be obtained from these spiders by artifical means, and the experiments made by certain naturalists with a view to ascertaining the amount that could be obtained from individuals of this genus in a season, the object of which was to endeavour to prove that the product might be used for economic purposes.

Dr. J. Müller, of Geneva, Switzerland, contributed a paper on the Pyrenocarpeæ of the Lichen family. Mr. A. J. Campbell described the nests and eggs of Australian Hawks. Mr. A. G. Hamilton, in a paper entitled "The Fertilisation of some Australian Plants," gave many of his own observations as to the mode by which fertilisation is effected. Mr. W. M. Maskell gave a synoptical list of the Coccidæ reported from Australasia and the Pacific Islands up to December 1894.

Mrs. W. Martin gave the life-history of the vegetable growth known as Native Bread (*Mylitta Australis*). Australian mosses were enumerated by Richard A. Bastow, and some notes on the poisonous constituents of *Stephania hernandiæfolia* were read by E. H. Rennie and E. F. Turner. Picrotoxine and an alkaloid possessing strongly poisonous properties and marked chemical characteristics have been isolated from an extract from the plant.

"Economic Entomology" was the title of a paper by the Rev. E. H. Thompson, who pointed out the great benefit resulting to a country from a properly conducted Government Entomological Department, and urged, in order to increase its usefulness: (1) the formation of a federal entomological department with a head staff and field observers in each of the colonies; (2) a federal agricultural and scientific journal for all the colonies, subsidised by all; (3) elementary entomology to be taught in the State schools, special reference being given to the insect pests peculiar to each district or colony; and (4) the

formation of school museums and prizes given for the best collections.

Mr. G. E. Barton gave a concise historical account of the first discovery of the Eucalyptus, including the names and nationalities of those to whom the honour has been ascribed by various writers.

A paper by Dr. J. Lauterer contained physiological and microchemical researches on the Eucalyptus, and contributed some new items with regard to the life-history of those trees connected with the origin of the gum exuded by their bark.

Geography.

The President of the Section, Baron von Mueller, was absent, but his address, on "The Commerce of Australia with Neighbouring Countries in Relation to Geography," was read.

Mr. C. L. Wragge gave an account of his investigations of ocean currents by means of bottles thrown into the sea. He was of opinion from the results obtained that many of the bottles had been influenced more by winds than by ocean currents; but if this were not the case, the bottles cast adrift in the Australian Bight distinctly indicated that a strong current sets from the neighbourhood of Kangaroo Island towards the head of the Bight and Israelite Bay. The most interesting of the bottle papers is one that was cast adrift near the Cocos Islands, in the north-eastern portion of the Indian Ocean, and which was found a few months afterwards on the shores of German East Africa. Papers cast adrift by Mr. Wragge during a voyage to England, in the neighbourhood of the Sargasso Sea, were picked up at Hayti, on the Alabama coast, and on the Louisiana coast. Others thrown overboard with a view to testing Rennel's current, which sets towards the coast of Ireland, from the neighbourhood of Cape Finisterre, were certainly influenced by the strong west-south-west winds which were experienced on that occasion between the Western Islands and the English Channel. None of these appear to have followed the current, but went straight across it, some being found on the west coast of France, and near the islands of Sein, while one was picked up at Brighton. It appears to be highly desirable, judging from the results obtained, that the bottles should be weighted with sand or other material, with a view to more completely sinking them in the water, and thus minimising the influence of the winds.

Among other papers contributed to this Section were:—"The Southern Alps of New Zealand," by Mr. A. P. Harper; "The Bissagos Islands," by M. Max Astrie; and "Physiography of the Victorian Gold Fields," by James Stirling.

Ethnology and Anthropology.

Mr. Thomas Worsnop, President of the Section of Ethnology and Anthropology, delivered an address upon the prehistoric arts of the Australian Aborigines. Messrs. W. J. Enright and R. H. Matthews described the aboriginal drawings in the Wollombi Caves, New South Wales. A paper was contributed by Mr. Thomas Petrie, on the habits and customs of the wild tribes as he saw them in 1837, from Brisbane to Maroochy. "Foods of North-west Aboriginals" was the title of a paper by J. Coghlan. Mr. John F. Small contributed a paper on customs and traditions of the Clarence River aboriginals. The paper dealt with the traditions, funeral ceremonies, marriage laws, and the Bora ceremony. Mr. J. Thorne read a paper entitled "Curious Aboriginal Marriage Custom." The paper was the result of investigations made by the author in the Laguna Bay.

The other papers communicated to this Section included: "'Boomerang' and 'Woomera,' Evolution, Varieties, and Distribution," by Mr. A. Weston; "The Ancient Government of Samoa," by Rev. S. Ella; "Notes on Tokelau, Gillbert, and Ellice Islands," by Rev. J. E. Newell; "A Comparative View of some Samoan Customs," by Rev. J. B. Stairs; "Early Samoan Voyages and Settlements," by Rev. J. B. Stairs; and "Gaelic Contributions to Folk Lore," by Rev. A. C. Sutherland.

Agriculture.

In a paper on the teaching of agricultural botany, Mr. C. T. Musson said that the object to be aimed at by instructors in agricultural botany should be to impart such information to the prospective cultivator as would make him acquainted with plant structure and the more important useful plants. Practice alone would not make a good farmer, but practice, when based upon a knowledge of the animate and inanimate objects he was dealing with, and their surroundings, would make the man of resources best fitted for his work. Mr. T. B. Guthrie contributed a paper on examinations of different varieties of wheat grown in New South Wales. He also read a paper on "soil analysis," in which the value of soil analysis to the farmer was discussed, and different methods for the determination of the available plant food in soils were reviewed. The paper embodied a suggestion for a scheme of soil analysis, the results of which should be of practical use to the farmer, based upon the determination of those conditions which conduce to fertility rather than to the chemical constitution of the soil. Of the remaining papers read before this section, the following were of more than technical interest:—

"Climatic Influences on Contagious Diseases of Live Stock," by P. R. Gordon; "How to Grow Fruit," by Albert H. Benson; "Floods and Forests," by Philip MacMahon; "Semi-Tropical Horticulture," by Leslie G. Corrie; "Forage Plants and Grasses of Australia," by Fred. Turner; "The Agricultural Chemistry of the Sugar Cane," by Joseph Fletcher.

Engineering and Architecture.

Mr. James Fincham, President of this Section, delivered his presidential address on "Architecture and Engineering."

Prof. W. C. Kernot contributed a paper on wind pressure. The paper was a continuation of one read at the Adelaide meeting. It dealt with the relation between velocity and pressure, and detailed series of experiments leading to the formula $P = \cdot 0033V^2$, which approximates very closely to the rule given by Dines, and disagrees with the rules given by Smeaton and Crosby. The pressure of wind upon roofs was also dealt with, and experiments were quoted to show that the ordinary method of computing the pressure is fairly accurate when the roof is supported on thin columns, so that the wind can pass freely below, but is altogether wrong when the roof is supported on walls. In this latter case the pressure is greatly reduced, and when the walls terminate in parapets is often rendered negative, the roof having a distinct tendency to lift.

Other papers communicated to this section were:—"Experiments on the Waterproofing of Bricks and Sandstones with Oils," and "Experiments on the Porosity of Plasters and Cements," by Prof. A. Liversidge, M.A., F.R.S.; "On Teredo-Resisting River Structures," by Thomas Parker; "Earthquakes in Relation to Building Construction," by Thos. Turnbull.

Sanitary Science and Hygiene.

The President of the Section of Sanitary Science and Hygiene, Dr. J. W. Springthorpe, read an address on "The Teaching of Science in Matters of Health."

Among the papers read were:—"The Promise of 'Serum Therapeutics' in regard to Tuberculosis," by Dr. J. Sidney Hunt; "Contagiousness of Tuberculosis," by F. H. Vivian Voss; "The Prevalence and Intercommunicability of Human and Animal Tuberculosis," by S. S. Cameron; "Leprosy," by Dr. C. E. Dumbleton, and also by A. Francis; and "Etiological Views of the Maintenance of Leprosy," by Dr. J. A. Thompson.

Mental Science and Education.

Prof. F. Anderson, the President of this Section, delivered his address on "Education in Politics."

Dr. Henry Belcher contributed a paper on the use and abuse of examinations. The advantages of the examination system were shortly stated as follows:—It enables the teacher to stimulate the intelligence and test the progress of the pupil, and to fill up flaws and gaps due to imperfect apprehension, carelessness, or defective memory; it is a power almost indispensable to the teacher's efficiency, and is thus a potent factor in general education; it had an alterative and prophylactic effect upon private adventure schools, raising their tone both intellectually and morally. The author doubted whether it was wise to entrust the examination of pupils to persons other than their teachers. The disadvantages of the examination system were that the best part of a teacher's work escapes analysis; methods of higher teaching rise in quality and character, while methods of examination lie behind; by the selection of set books, and the publication of manuals thereon, an intolerable yoke and shackle is placed upon elementary scholarship; examinations appeal to the lower side of human nature—what will pay becomes the pupil's ruling thought. Certain subjects of great importance are neglected because they do not largely count for prizes and honours; and research is altogether neglected.

Among the remaining papers read were:—"Science as a Subject in Girls' Schools," by Miss F. E. Hunt; "The Curriculum of Secondary Education," by D. H. Hollidge; "The Technical Element in a State System of Education," by Antony St. Ledger; "A Contribution towards the Study of the Relation of Ethics and Science," by the Rev. J. S. Pollock; "The Importance of Mental Science as a Guide in Primary Education," by James Rule.

The business of the Association concluded with a meeting of the General Council, at which the following recommendations, among others, were adopted:—

(1) That the committee for the investigation of the thermodynamics of the voltaic cell be reappointed without grant.

(2) That the report of the Seismological Committee be printed, and that the committee be reappointed and allowed a grant of £10 towards the cost of the erection of the instruments presented by Dr. Von Rebeur-Paschwitz at Timaru.

(3) That the following be a committee—namely, Messrs. F. M. Bailey, R. L. Jack, A. Gibb Maitland, A. Meston, C. W. De Vis, and H. Tryon—to investigate the geology, land flora, and natural resources generally of the islands and islets of the Great Barrier Reef.

(4) That the New Zealand Government be asked to set apart Stephen's Island, Cook Strait, as a reserve for the Tuatara Lizard.

(5) That the committee for the investigation of glacial deposits in Australasia be Messrs. Hutton, R. L. Jack, R. Tate, R. M. Johnston, F. W. E. David (secretary), G. Sweet, J. Shirley, W. Houchins, E. G. Hogg, E. J. Dunn, A. Montgomery, and E. F. Pittman.

(6) That a committee—consisting of Messrs. H. C. Stanley, A. B. Brady, Thomas Parker, Prof. Warren, Prof. Kernot, Henry Moncrieff, and James Fincham—be appointed to inquire into the habits of the teredo, and the best means of preserving timber or structures subject to the action of tidal waters.

(7) That the committee on psycho-physical research be appointed without a grant.

The next meeting of the Association will be held at Sydney in 1897, under the presidency of Prof. Liversidge, and the following meeting will take place at Melbourne.

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*ELECTRIFICATION OF AIR, AND THERMAL CONDUCTIVITY OF ROCK AT DIFFERENT TEMPERATURES.**

(I.) "ON THE ELECTRIFICATION OF AIR.

§ 1. CONTINUOUS observation of natural atmospheric electricity has given ample proof that cloudless air at moderate heights above the earth's surface, in all weathers, is electrified with very far from homogeneous distribution of electric density. Observing, at many times from May till September, 1859, with my portable electrometer on a flat open sea-beach of Brodick Bay in the Island of Arran, in ordinary fair weather at all hours of the day, I found the difference of potentials, between the earth and an insulated burning match at a height of 9 feet above it (2 feet from the uninsulated metal case of the instrument, held over the head of the observer), to vary from 200 to 400 Daniell's elements, or as we may now say volts, and often during light breezes from the east and north-east, it went up to 3000 or 4000 volts. In that place, and in fair weather, I never found the potential other than positive (never negative, never even down to zero), if for brevity we call the earth's potential at the place zero. In perfectly clear weather under a sky sometimes cloudless, more generally somewhat clouded, I often observed the potential at the 9 feet height to vary from about 300 volts gradually to three or four times that amount, and gradually back again to nearly the same lower value in the course of about two minutes.† I inferred that these gradual variations must have been produced by electrified masses of air moving past the place of observation. I did not remark then, but I now see, that the electricity in these moving masses of air must, in all probability have been chiefly positive to cause the variations which I observed, as I shall explain to you a little later.

* Two communications to the Philosophical Society of Glasgow meeting, in the Natural Philosophy Lecture-room of the University of Glasgow, March 27, "On the Electrification of Air": "On the Thermal Conductivity of Rock at different temperatures."

† "Electrostatics and Magnetism" (Sir William Thomson), xvi. §§ 281, 282.

§ 2. Soon after that time a recording atmospheric electrometer * which I devised, to show by a photographic curve the continuous variation of electric potential at a fixed point, was established at the Kew Meteorological Observatory, and has been kept in regular action from the commencement of the year 1861 till the present time. It showed incessant variations quite of the same character, though not often as large, as those which I had observed on the sea-beach of Arran.

Through the kindness of the Astronomer Royal, I am able to place before you this evening the photographic curves for the year 1893, produced by a similar recording electrometer which has been in action for many years at the Royal Observatory, Greenwich. They show, as you see, not infrequently, during several hours of the day or night, negative potential and rapid transitions from large positive to large negative. Those were certainly times of broken weather, with at least showers of rain, or snow, or hail. But throughout a very large proportion of the whole time the curve quite answers to the description of what I observed on the Arran sea-beach thirty-six years ago, except that the variations which it shows are not often of so large amount in proportion to the mean or to the minimums.

§ 3. Thinking over the subject now, we see that the gradual variations, minute after minute through so wide a range as the 3 or 4 to 1, which I frequently observed, and not infrequently rising to twenty times the ordinary minimum, must have been due to *positively* electrified masses of air, within a few hundred feet of the place of observation, wafted along with the gentle winds of 5 or 10 or 15 feet per second which were blowing at the time. If any comparably large quantities of negatively electrified air had been similarly carried past, it is quite certain that the minimum observed potential, instead of being in every case positive, would have been frequently large negative.

§ 4. Two fundamental questions in respect to the atmospheric electricity of fair weather force themselves on our attention:—(1) What is the cause of the prevalent positive potential in the air near the earth, the earth's potential being called zero? (2) How comes the lower air to be electrified to different electric densities whether positive or negative in different parts? Observations and laboratory experiments made within the last six or eight years, and particularly two remarkable discoveries made by Lenard, which I am going to describe to you, have contributed largely to answering the second of these questions.

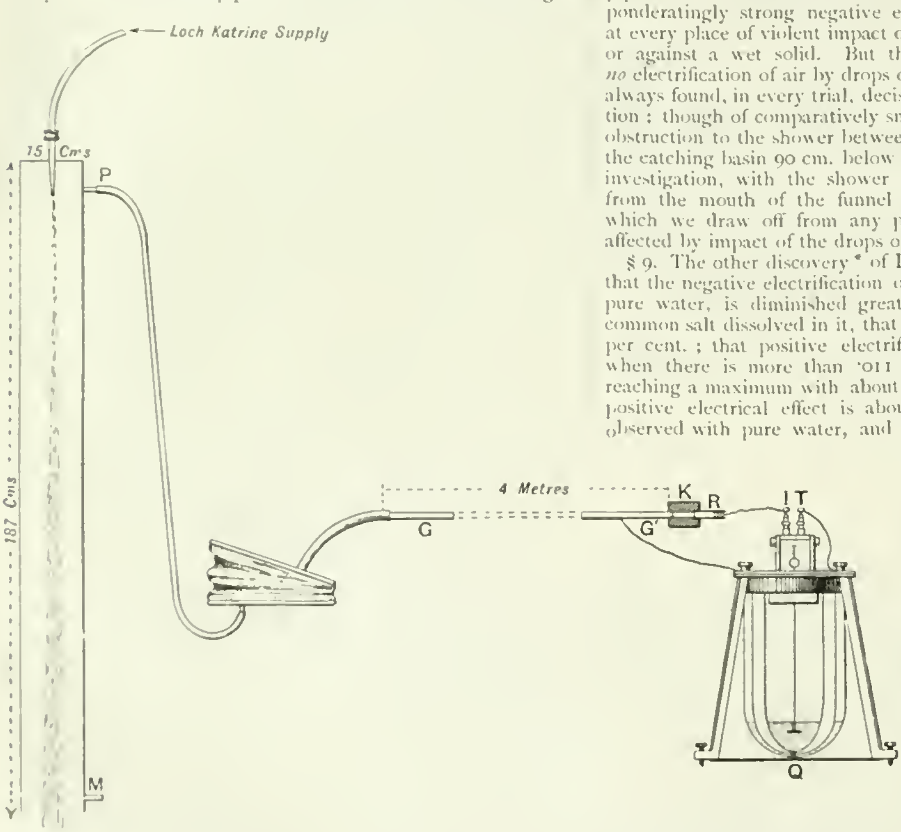
§ 5. In an article "On the Electrification of Air by a Water-jet," by Magnus Maclean and Makita Goto,† experiments were described showing air to be negatively electrified by a jet of water shot vertically down through it from a fine nozzle into a basin of water about 60 centimetres below it. It seemed natural to suppose that the observed electrification was produced by the rush of the fine drops through the air; but Lenard conclusively proved, by elaborate and searching experiments, that it was in reality due chiefly, if not wholly, to the violent commotions of the drops impinging on the water surface of the receiving basin, and he found that the negative electrification of the air was greater when they were allowed to fall on a hard slab of any material thoroughly wetted by water, than when they fell on a yielding surface of water several centimetres deep. He had been engaged in studying the great negative potential which had been found in air in the neighbourhood of waterfalls, and which had generally been attributed to the inductive action of the ordinary fine weather electric force, giving negative electricity to each drop of water-spray before it breaks away from conducting communication with the earth. Before he knew Maclean and Goto's paper, he had found strong reason for believing that that theory was not correct, and that the true explanation of the electrification of the air must be found in some physical action not hitherto discovered. A less thorough inquirer might have been satisfied with the simple explanation of the electricity of waterfalls naturally suggested by Maclean and Goto's result, and might have rested in the belief that it was due to an electrifying effect produced by the rush of the broken water through the air; but Lenard made an independent experimental investigation in the Physical Laboratories of Heidelberg and Bonn, by which he learned that the seat of the negative electrification of the air electrified is the lacerated water at the foot of the fall, or at any rocks against which the water impinges, and not the multitudinous interfaces between air and water falling freely in drops through it.

§ 6. It still seems worthy of searching inquiry to find

* "Electrostatics and Magnetism" xvi. §§ 271, 292.

† *Philosophical Magazine*, 1890, second half-year.

electrification of air by water falling in drops through it, even though we now know that, if there is any such electrification, it is not the main cause of the great negative electrification of air which has been found in the neighbourhood of waterfalls. For this purpose an experiment has been very recently made by Mr. Maclean, Mr. Galt, and myself, in the course of an investigation regarding electrification and diselectrification of air with which we have been occupied for more than a year. The apparatus which we used is before you. It consists of a quadrant electrometer connected with an insulated electric filter * applied to test the electrification of air drawn from different parts of a tinned iron funnel, 187 centimetres long and 15 centimetres diameter, fixed in a vertical position with its lower end open and its upper end closed, except a glass nozzle, of 1·6 mm. aperture, admitting a jet of Glasgow supply water (from Loch Katrine) shot vertically down along its axis. The electric filter (R in the drawing), a simplified and improved form of that described in the *Proceedings* of the Royal Society for March 21, consists of twelve circles of fine wire gauze rammed as close as possible together in the middle of a piece of block tin pipe of 1 cm. bore and 2 cm. length.



One end of it is stuck into one end of a perforation through a block of paraffin, K, which supports it. The other end (G') of this perforation is connected by block tin pipe (which in the apparatus actually employed was 4 metres long, but might have been shorter), and india-rubber tubing through bellows to one or other of two short outlet pipes (M and P) projecting from the large funnel.

§ 7. We first applied the india-rubber pipe to draw air from the funnel at the *upper* outlet, P, and made many experiments to test the electricity given by it to the receiving filter, R, under various conditions as to the water-jet; the bellows being worked as uniformly as the operator could. When the water fell fairly through the funnel with no drops striking it, and through 90 cm. of free air below its mouth, a small negative electrification of R was in every case observed (which we thought might possibly be attributable to electrification of the air where the water was caught in a basin about 90 cm. below the mouth of the funnel). But when the funnel was slanted so that the whole shower of drops from the jet, or even a small part of it, struck the inside of the funnel, the negative electrification of R was largely increased. So it was also when the shower, after falling freely down the middle of the funnel, impinged on a metal plate in metallic communication with the funnel, held close under its mouth, or 10 or 20 cm. below it. For example, in a series of experiments made last Monday (March 25), we found ·28 of a volt in 15 minutes with no obstruction to the shower; and 4·18 volts in five minutes, with a metal plate held three or four centimetres below the mouth of the funnel; the air being drawn from the upper outlet (P). Immediately after, with P closed, and air drawn from the lower outlet (M), but all other circumstances the same, we found ·20 of a volt in five minutes with no obstruction; and 6·78 volts in five minutes with the metal plate held below the mouth as before.

§ 8. These results, and others which we have found, with many variations of detail, confirm, by direct test of air drawn away from the neighbourhood of the waterfall through a narrow pipe to a distant electrometer, Lenard's conclusion that a preponderatingly strong negative electrification is given to the air at every place of violent impact of a drop against a water-surface, or against a wet solid. But they do not prove that there is *no* electrification of air by drops of water falling through it. We always found, in every trial, decisive proof of negative electrification; though of comparatively small amount when there was no obstruction to the shower between the mouth of the funnel and the catching basin 90 cm. below it. We intend to continue the investigation, with the shower falling freely far enough down from the mouth of the funnel to make quite sure that the air which we draw off from any part of the funnel is not sensibly affected by impact of the drops on anything below.

§ 9. The other discovery * of Lenard, of which I told you, is that the negative electrification of air, in his experiments with pure water, is diminished greatly by very small quantities of common salt dissolved in it, that it is brought to nothing by ·011 per cent.; that positive electrification is produced in the air when there is more than ·011 per cent. of salt in the water, reaching a maximum with about 5 per cent. of salt, when the positive electrical effect is about equal to the negative effect observed with pure water, and falling to 14 per cent. of this amount when there is 25 per cent. of salt in the solution. Hence sea-water, containing as it does about 3 per cent. of common salt, may be expected to give almost as strong positive electrification to air as pure water would give of negative in similar circumstances as to commotion. Lenard infers that breaking waves of the sea must give positive electricity to the air over them; he finds, in fact, a recorded observation by Exner, on the coast of Ceylon, showing the normal positive electric potential of the air to be notably increased by a storm at sea. I believe Lenard's discovery fully explains also some very interesting observations of atmospheric electricity of my own, which I described in a letter to Dr. Joule, which he published in the *Proceedings* of the Literary and Philosophical Society of Manchester for October 18, 1859.† "The atmospheric effect ranged from 30° to about 420° [of a heterostatic torsion electrometer of 'the divided-ring' species] during the four days which I had to test it; that is to say, the electrometric force per foot of air, measured horizontally from the side of the house, was from 9 to above 126 zinc-copper water cells. The weather was almost perfectly settled, either calm, or with slight east wind, and in general an easterly haze in the air. The electrometer twice within half an hour went above 420°, there being at the time a fresh temporary breeze from the east. What I had previously observed regarding the effect of east wind was amply confirmed.

* Kelvin, Maclean, Galt, "On the Diselectrification of Air." *Proc.* Roy. Soc. March 14, 1895.

* "Ueber die Electricitat der Wasserfälle." Table xvii. p. 628. *Annalen der Physik und Chemie*, 1892, vol. xlvi.

† Republished in "Electrostatics and Magnetism." "Atmospheric Electricity," xvi. § 262.

Invariably the electrometer showed very high positive in fine weather, before and during east wind. It generally rose very much shortly before a slight puff of wind from that quarter, and continued high till the breeze would begin to abate. I never once observed the electrometer going up unusually high during fair weather without east wind following immediately. One evening in August I did not perceive the east wind at all, when warned by the electrometer to expect it; but I took the precaution of bringing my boat up to a safe part of the beach, and immediately found by waves coming in that the wind must be blowing a short distance out at sea, although it did not get so far as the shore. . . . On two different mornings the ratio of the house to a station about sixty yards distant on the road beside the sea was ·97 and ·96 respectively. On the afternoon of the 11th instant, during a fresh temporary breeze of east wind, blowing up a little spray as far as the road station, most of which would fall short of the house, the ratio was 1·08 in favour of the house electrometer—both standing at the time very high—the house about 350°. I have little doubt but that this was owing to the negative electricity carried by the spray from the sea, which would diminish relatively the indications of the road electrometer."

§ 10. The negative electricity spoken of in this last sentence, "as carried by the spray from the sea," was certainly due to the inductive effect of the ordinary electrostatic force in the air close above the water, by which every drop or splash breaking away from the surface must become negatively electrified; but this only partially explains the difference which I observed between the road station and the house station. We now know, by the second of Lenard's two discoveries, to which I have alluded, that every drop of the salt water spray, falling on the ground or rocks wetted by it, must have given positive electricity to the adjoining air. The air, thus positively electrified, was carried towards and over the house by the on-shore east wind which was blowing. Thus, while the road electrometer under the spray showed less electrostatic force than would have been found in the air over it and above the spray, the house electrometer showed greater electrostatic force because of the positively electrified air blown over the house from the wet ground struck by the spray.

§ 11. The strong positive electricity, which, as described in my letter to Joule, I always found in Arran with east wind, seemed at first to be an attribute of wind from that quarter. But I soon found that in other localities east wind did not give any very notable augmentation, nor perhaps any augmentation at all, of the ordinary fair weather positive electric force, and for a long time I have had the impression that what I observed in this respect, on the sea-beach of Brodick Bay in Arran, was really due to the twelve nautical miles of sea between it and the Ayrshire coast east-north-east of it; and now it seems to me more probable than ever that this is the explanation when we know from Lenard that the countless breaking waves, such as even a gentle east wind produces over the sea between Ardrossan and Brodick, must every one of them give some positive electricity to the air wherever a spherule of spray falls upon unbroken water. It becomes now a more and more interesting subject for observation (which I hope may be taken up by naturalists having the opportunity) to find whether or not the ordinary fine weather positive electric force at the sea coast in various localities is increased by gentle or by strong winds from the sea, whether north, south, east or west of the land.

§ 12. From Lenard's investigation we now know that every drop of rain falling on the ground or on the sea,* and every drop of fresh water spray of a breaking wave, falling on a fresh water lake, sends negative electricity from the water surface to the air; and we know that every drop of salt water, falling on the sea from breaking waves, sends positive electricity into the air from the water surface. Lenard remarks that more than two-thirds of the earth's surface is sea, and suggests that breaking sea-waves may give contributions of positive electricity to the air which may possibly preponderate over the negative electricity given to it from other sources, and may thus be the determining cause of the normal fair weather positive of natural atmospheric electricity. It seems to me highly probable that this preponderance is real for atmospheric electricity at sea. In average weather, all the year round, sailors in very small vessels are more wet by sea-spray than by rain, and I think it is almost certain that more positive electricity is given to the air by breaking waves than negative electricity by rain. It seems also probable that the positive electricity from the waves is much more carried up by strong winds to considerable heights above the sea, than the negative electricity given to the air by rain falling on the sea; the greater part of which may be quickly lost into the sea, and but a small part carried up to great heights. But it seems to me almost certain that the exceedingly rapid recovery of the normal fair weather positive, after the smaller positive or the negative atmospheric electricity of broken weather, which was first found by Beccaria in Italy 120 years ago, and which has been amply verified in Scotland and England,* could not be accounted for by positively electrified air coming from the sea. Even at Beccaria's Observatory, at Garzegna di Mondovi in Piedmont, or at Kew, or Greenwich, or Glasgow, we should often have to wait a very long time for reinstatement of the normal positive after broken weather, if it could only come in virtue of positively electrified air blowing over the place from the sea; and several days, at least, would have to pass before this result could possibly be obtained in the centre of Europe.

§ 13. It has indeed always seemed to me probable that the rain itself is the real restorer of the normal fair weather positive. Rain or snow, condensing out of the air high-up in the clouds, must itself, I believe, become negatively electrified as it grows, and must leave positive electricity in the air from which it falls. Thus rain falling from negatively electrified air would leave it less negatively electrified, or non-electrified or positively electrified: rain falling from non-electrified air would leave it positively electrified; and rain falling from positively electrified air would leave it with more of positive electricity than it had before it lost water from its composition. Several times within the last thirty years I have made imperfect and unsuccessful attempts to verify this hypothesis by laboratory experiments, and it still remains unproved. But I am much interested just now to find some degree of observational confirmation of it in Elster and Geitel's large and careful investigation of the electricity produced in an insulated basin by rain or snow falling into it, which they described in a communication published in the *Sitzungsberichte* of the Vienna Academy of Sciences, of May 1890. They find generally a large electrical effect, whether positive or negative, by rain or snow falling into the basin for even so short a time as a quarter of a minute, with however, on the whole, a preponderance of negative electrification.

§ 14. But my subject this evening is not merely natural atmospheric electricity, although this is certainly by far the most interesting to mankind of all hitherto known effects of the electrification of air. I shall conclude by telling you very briefly, and without detail, something of new experimental results regarding electrification and diselectrification of air, found within the last few months in our laboratory here by Mr. Maclean, Mr. Galt, and myself. We hope before the end of the present session of the Royal Society to be able to communicate a sufficiently full account of our work.

§ 15. Air blown from an uninsulated tube, so as to rise in bubbles through pure water in an uninsulated vessel, and carried through an insulated pipe to the electric receiving filter, of which I have already told you, gives negative electricity to the filter. With a small quantity of salt dissolved in the water, or sea water substituted for fresh water, it gives positive electricity to the air. There can be no doubt but these results are due to the same physical cause as Lenard's negative and positive electrification of air by the impact of drops of fresh water or of salt water on a surface of water or wet solid.

§ 16. A small quantity of fresh water or salt water shaken up vehemently with air in a corked bottle electrifies the air, fresh water negatively, salt water positively. A "Winchester quart" bottle (of which the cubic contents is about two litres and a half), with one-fourth of a litre of fresh or salt water poured into it, and closed by an india-rubber cork, serves very well for the experiment. After shaking it vehemently till the whole water is filled with fine bubbles of air, we leave it till all the bubbles have risen and the liquid is at rest, then take out the cork, put in a metal or india-rubber pipe, and by double-acting bellows, draw off the air and send it through the electric filter. We find the electric effect, negative or positive according as the water is fresh or salt, shown very decidedly by the quadrant electrometer: and this, even if we have kept the bottle corked for two or three minutes after the liquid has come to rest before we take out the cork and draw off the air.

§ 17. An insulated spirit lamp or hydrogen lamp being con-

* "Ueber die Electricität der Wasserfälle." *Annalen der Physik und Chemie*, 1892, vol. xlvi. p. 631.

* "Electrostatics and Magnetism," xvi. § 287.

nected with the positive or with the negative terminal of a little Voss electric machine, its fumes (products of combustion mixed with air) sent through a block-tin pipe, four metres long, and one centimetre bore, ending with a short insulating tunnel of paraffin and the electric filter, gives strong positive or strong negative electricity to the filter.

§ 18. Using the little biscuit-canister and electrified needle, as described in "our communication" * to the Royal Society "On the Diselectrification of Air," but altered to have two insulated needles with varied distances of from half a centimetre to two or three centimetres between them, we find that when the two needles are kept at equal differences of potential positive and negative, from the enclosing metal canister, little or no electrification is shown by the electric filter; and when the differences of potential from the surrounding metal are unequal, electrification, of the same sign as that of the needle whose difference of potential is the greater, is found on the filter.

When a ball and needle-point are used, the effect found depends chiefly on the difference of potentials between the needle-point and the surrounding canister, and is comparatively little affected by opposite electrification of the ball. When two balls are used, and sparks in abundance pass between them, but little electricity is deposited by the sparks in the air, even when one of the balls is kept at the same potential as the surrounding metal. [The communication was illustrated by a repetition of some of the experiments shown on the occasion of a Friday evening lecture † on Atmospheric Electricity at the Royal Institution on May 18, 1860, in which one half of the air of the lecture-room was electrified positively, and the other half negatively, by two insulated spirit lamps mounted on the positive and negative conductors of an electric machine.]

(2) "ON THE THERMAL CONDUCTIVITY OF ROCK AT DIFFERENT TEMPERATURES."

Experiments by Lord Kelvin and Mr. Erskine Murray were described, and the apparatus used in them was shown, by which it was found that the thermal conductivity of specimens of slate, sandstone, and granite is less at higher temperatures than at lower for each of these rocks. The last tested was Aberdeen granite, for which experiments of fairly satisfactory accuracy showed the mean conductivity for the range from 146° C. to 215 C. to be 86 per cent. of the mean conductivity in the range from 81 C. to 146 C. They hope to send a communication to the Royal Society describing their work before the end of the present session.

KELVIN.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Mr. D. R. Pike, of the Charterhouse, has been elected to an open Exhibition in Natural Science at Jesus College, and Mr. L. C. W. Brigstocke, of Haverfordwest Grammar School, has been elected to a Welsh Foundation Scholarship in Natural Science at the same College.

Open Scholarships and Exhibitions in Natural Science have been announced for competition at Merton College, New College, Magdalen College, and Corpus Christi College. Particulars may be obtained on application to the Dean in any of these Colleges.

CAMBRIDGE.—The Walsingham Medal for an original monograph on a botanical, geological, zoological, or physiological subject will be awarded in the Michaelmas Term. Essays are to be sent to Prof. Newton by October 10, 1895. Candidates must be B.A.'s not of standing to take the M.A. degree.

The subject for the Adams Prize of 1897 is connected with Bessel's Functions. It is set forth in the *University Reporter* for May 14. The prize is of the value of about £197. It is open to all graduates of the University. Essays are to be sent to the Vice-Chancellor by December 16, 1896.

THE Association of Technical Institutions has endeavoured to induce the Science and Art Department to discontinue the examinations now held in practical inorganic and organic chemistry, and to award attendance grants for instruction in those subjects, the amount of such grants to be dependent upon the report of the Department's inspectors on the efficiency of the equipment and teaching. The Association has received a reply to the effect that it is not possible for the Department to comply with their request. A new syllabus for practical inorganic chemistry will appear, however, in the forthcoming edition of the Science and Art Directory, and there seems little doubt that the instruction will be so arranged in it as to make it possible to coordinate more closely the laboratory and lecture work in that subject, and afford the same latitude to teachers as is given by the new Regulations for Organised Science Schools.

* *Proceedings of the Royal Society*, March 14, 1895.

† "Electrostatics and Magnetism," xvi. [illegible]

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xvii. No. 2 (Baltimore, April 1895).—A method for calculating simultaneously all the roots of an equation, is a paper by Dr. E. McClintock, which was read before the American Mathematical Society on August 14 and October 27, 1894. It opens with the application to an example employed by Spitzer and by Jelinek. The calculations of these mathematicians can only be done for a pair of roots at a time, and that with considerable difficulty. The method employed by our author is fairly facile. Very little has hitherto been done in the direction of this memoir, which is one of great value in the subject of algebraic equations. The writer discusses eleven examples at length, the highest degreed equation being one of the sixth degree in x.—Sur le logarithme de la fonction gamma, by Hermite, is a note upon Raabe's integral, in continuation of an article in the *Math. Annalen* (41, p. 581).—Sur la pression dans les milieux diélectriques ou magnétiques, by Prof. P. Duhem, corrects an error in his "Leçons sur l'Electricité et le magnétisme," and is a valuable working out of the theory of the pressures, initiated by Clerk Maxwell, and further improved by von Helmholtz, Kirchhoff, and other writers. The number closes with an article on ternary substitution-groups of finite order which leave a triangle unchanged, by H. Maschke. This paper is complementary to C. Jordan's "Sur les équations, differentielles linéaires à integrale algébrique," and "Sur la détermination des groupes d'ordre fini contenues dans le groupe linéaire."

Zeitschrift für wissenschaftliche Zoologie, Bd. lix. Heft 1.—Prof. A. R. von Heider gives a detailed description of a new Actinian (*Zoanthus chierchiae*) obtained during the cruise of the *Vettor Pisani*. Prof. A. Korotneff describes the embryonic development of *Salpa democratica*. According to him the follicle-cells do not play the important part in the development of *Salpa* which Salensky attributed to them, nor do they form a temporary scaffolding for the blastomeres, as stated by Brooks. The embryo is built up of blastomeres in the normal manner, and embryonic layers are present with the same significance as in other groups. The cloaca is formed by the union of endodermal diverticula, and the pericardium develops as an outgrowth of the pharynx.—Prof. W. Schimkewitsch writes upon the structure and development of a species of *Dinophilus* living in the White Sea, near the Solovetzki laboratory. The twofold affinities of this interesting type, on the one hand with the Annelids, and on the other with the Rotifers, are succinctly stated.—Prof. Vejdovsky writes upon the sexual apparatus of *Lumbriculus variegatus*.—Dr. Montgomery deals fully with the anatomy of a new type of Nemertine (*Stichostemma Eilhardi*) discovered in fresh-water aquaria in the Berlin Zoological Institute.—Dr. McKim describes the nephridial funnel apparatus of *Hirudo*.

SOCIETIES AND ACADEMIES.

EDINBURGH.

Royal Society, March 18.—The Rev. Prof. Flint, Vice-President, in the chair.—Prof. Crum-Brown communicated a paper, by Mr. R. Fairbairn and himself, on the action of sodium mercaptide on dibromomalonic ether.—Prof. J. C. Ewart communicated a paper, by Mr. F. J. Cole and himself on the dorsal branches of the cranial and spinal nerves in elasmobranchs.—Dr. Traquair read a paper on phosphorescent sandstones.—Prof. Tait read a note on the electromagnetic wave-surface.

April 1.—Sir Douglas Maclagan, President in the chair.—A paper, by the Duke of Argyll, on the glaciation of two glens, was read. The glens are Glenaray and Glenshira. The usual explanation of the phenomena of glaciation as observed in the West Highlands is that the glaciation was caused by an enormous ice-cap covering the whole country. His Grace does not consider that the phenomena can be so explained. Rocks are found which are striated and smoothed on one side, while the other side remains rough. Isolated blocks, without striation,

are found in positions where they could not have been placed except by the agency of floating ice-floes. He considers that the marks of glaciation were caused by ice-floes, driven by strong north-east and south-west currents, in a sea whose surface reached a level on the land of from 1500 to 2000 feet over the present level. The two glens run nearly parallel in a north-easterly direction, and are separated by a range of hills and moors not much more than two miles broad. The rocks of both belong to the same geological formation, and yet the glens are entirely dissimilar in appearance. Glenshira has smooth, regular slopes, with a smooth level bottom; Glenaray is a typical highland glen traversed by a rapid river with a rocky bed and three waterfalls, and exhibits strong glaciation. His Grace does not consider that an ice-sheet, operating over the whole country, could account for these differences. Neither does he consider that local glaciers could have produced the effect, for such a glacier must have been formed on the slopes of Ben Loy and have flowed down Glenshira. On the other hand, Glenaray terminates in a low pass 480 feet above sea-level, while Glenshira is closed in by ridges 2000 feet in height. The former was therefore open to the action of floes, while the higher peaks would shelter the latter.

April 17.—Sir Douglas Maclagan, President, in the chair. Prof. Flinders Petrie gave a lecture "On a New Race in Egypt," describing the result of his work in Egypt during the last season.

PARIS.

Academy of Sciences, May 6.—M. Marey in the chair.—The zoological work of James Dana, by M. Blanchard. The main outlines of James Dana's work are sketched from a zoologist's point of view. Reference is particularly made to his work on the geographical distribution of zoophytes, on coral reefs and islands, on animal distribution with reference to depth and temperature in the sea, and on Crustaceans.—The mineralogical and geological work of James Dana, by M. Daubrée. A very full account is given of the chief points in Dana's geological work, special reference being made to his publication of a "System of Mineralogy," and his "Manual of Geology."—The work of M. Carl Vogt, by M. Émile Blanchard.—Researches on the cerite earths, by M. P. Schützenberger. The author establishes the result that in cerite, cerium oxide is accompanied by small quantities of another earth of a metal with somewhat lower atomic weight, which is capable of being oxidised like cerium oxide, and of which the sulphate is isomorphous with that of cerium, and gives insoluble double sulphates with alkaline sulphates. The calcined higher oxide is of reddish-brown colour, even without presence of didymium.—Action of fluorine on argon, by M. Henri Moissan (see Notes, p. 61). Systematic application of the potato to the feeding of cattle, by M. Aimé Girard. The results are reported of experiments on the feeding of cattle and sheep, both quantity and quality of meat obtained being considered. The best results were obtained with given proportions of cooked potatoes and hay, a very superior article being obtained yielding high profits.—Report on the table of triangular numbers of M. Arnaudeau.—On the orbit of the 1771 comet, by M. Bigourdan. A re-examination of the original manuscript of Saint-Jacques has allowed the discovery of an error made by Burckhardt in reducing observations of this comet. The result of a preliminary recalculation of the observations allows the definite rejection of a hyberbolic orbit, and renders it very probable that the orbit is an ellipse of eccentricity 0·998.—Every algebraical condition imposed on the movement of a body is realisable by means of an articulated system, by M. G. Kœnigs.—On the use of a fourth dimension, by M. de la Rive.—On fluted spectra, by Prof. Arthur Schuster. A discussion of the different interpretation of phenomena by the author and M. Poincaré. In conclusion, the author is unable to doubt the justice of M. Gouy's view, that the regularity of the vibrations, shown by the observations of Fizeau and Foucault, does not exist in the luminous movement, but is produced by the apparatus used.—Unequal absorption of dextrorotatory and lævorotatory circularly polarised light in certain active substances, by M. A. Cotton. This unequal absorption is indicated by the conversion of a plane polarised ray into an elliptically polarised ray by passage through substances such as the coloured metallic tartrates. The method of measuring the effect is indicated and results promised in a further communication.—On the freezing of solutions at constant temperature, by M. Sarrau. Solidification is produced under pressure so that no lowering of the freezing point occurs, the connection between the compensating pressure and molecular weight is considered.—Closed isothermal cycles, reversible and maintained in equilibrium by gravity, by M. A. Ponsot.—Observations on the project of a balloon expedition to the Arctic regions put forth by M. S. A. Andrée, by M. Gaston Tissandier.—Researches on mercurous sulphate, nitrate, and acetate, by M. Raoul Varet. The heats of formation from their elements taken in their actual states are: for $Hg_2 SO_4$ sol. + 175 Cal.; for $Hg_2 (NO_3)_2 2H_2O$ sol. + 69·4 Cal.; and for $Hg_2 (C_2H_3O_2)_2$ sol. + 202·1 Cal.—On the presence of chitin in the cellular membrane of mushrooms, by M. Eugene Gilson. Chitin has been found in all the fungi examined, taking the place and fulfilling the functions of cellulose in phanerogams and cryptogams. The experimental evidence concerns Agaricus campestris, Amanita muscaria, Cantharellus cebarius, Hypholoma fasciculare, Polyporus officinalis, Polyporus fumosus, Russula, Boletus, Tricholona, Bovista, and Claviceps purpurea.—Comparative study of the "appareils odorifiques" in the different groups of Heteropterous Hemiptera, by M. J. Künckel d'Herculais.—Overlap of the Jurassic beds in the massif of the Vendée, by M. Fred. Wallerant.—Influence of de-oxygenated blood, and of some poisons, on the contractility of the lymphatic vessels, by MM. L. Camus and E. Gley.—On the scarlatinous streptococcus, by M. Ad. d'Espine.—The manuring of vines and quality of the wines, by M. A. Müntz. The supposed deleterious action of manure on the quality of wine produced from the dressed vineries has no substantial foundation in fact.

BERLIN.

Physiological Society, April 5.—Prof. H. Munk, President, in the chair.—Prof. J. Munk had investigated the excretion of mineral waste during Prof. Zuntz' experiments on the effects of excessive exercise on metabolism. (See NATURE, vol. li. p. 503.) He found that the urinary output of sulphur was increased in correspondence with the increased proteid metabolism, the excess taking the form of sulphuric acid, not of ethereal-sulphates. Phosphorus and potassium were also similarly increased, and since neither of them are normal constituents of proteid, their greater excretion denoted some destruction of other tissues. This view was confirmed by the increased excretion of lime, which further points to a possibly greater metabolism of bone-tissue during the exercise.—Dr. Treitel had carried out observations on the perception of the vibrations of tuning-forks by the skin, and had found that the sensibility varied in different parts of its surface, and did not correspond with that for the perception of mere touch or localisation.—Dr. Schultz demonstrated the contraction of single bundles of unstriated muscle-fibres on a preparation made from the muscular coat of a frog's stomach. The fibres could be seen to slowly contract on electric stimulation, relaxing equally slowly after the stimulus had ceased.

Meteorological Society, April 2.—Prof. Hellmann, President, in the chair.—Dr. Less spoke on the various types of winter weather, dealing in detail with the five types established by Teisserenc de Bort as depending on the distribution of barometric maxima and minima over the Atlantic Ocean and Europe. He added to these a sixth type of mild and squally weather which most usually follows after other types of warm winter weather. He pointed out that the winter just past could for the most part not be included under any of the above six types.

Physical Society, April 26.—Prof. Schwalbe, President, in the chair.—Dr. Pringsheim gave an account of his experiments on the electric conductivity of heated gases. In a Chamotte-tube closed by brass caps the various gases, such as air, hydrogen, and carbon dioxide, were heated to a temperature of 700 to 900° C. The electrodes consisted of circular discs of platinum capable of being placed at varying distances from each other. A current of 1·6 to 10 volts was passed through the gases, and all the results obtained by Becquerel in 1853 were confirmed. As the electrodes were separated from each other the deflection of the galvanometer became less, and with constant distance between the electrodes the current became less the longer it flowed. This fact led to the suspicion, verified by experiment, that polarisation was here playing a part. On breaking the primary current, the polarisation of the electrodes was quite perceptible for a full half-hour. The speaker concluded from the above that conduction in heated gases is an electrolytic phenomenon, and intends to carry on his researches, using more carefully purified gases and a trustworthy pyrometer.—Dr. du Bois reported on a paper presented by Prof. van Aubel, dealing with Hall's phenomenon as investigated on thin layers of bismuth

deposited electrolytically. It appeared that when the deposit was made from nitrate of bismuth the phenomenon was as marked as it is with cast plates of the metal, whereas when deposited from the tartrate the phenomenon was either extremely feeble or non-existent. The asymmetry of the phenomenon on reversal of the magnetic field was explained by the author as due to the influence exerted by the magnetic field on the electric conductivity of the metal. He further regarded the difference in behaviour of the metallic film as precipitated, on the one hand, from the nitrate, and on the other, from the tartrate or citrate, as due to the fact that in the case of the latter salts the bismuth is mixed with carbon, whereas in the case of the nitrate the metal is deposited in a pure state.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, MAY 16.

ROYAL SOCIETY, at 4.30.—On Measurements of Small Strains in the Testing of Materials and Structures: Prof. Ewing, F.R.S.—The Electrical Measurement of Starlight. Observations made at the Observatory of Daramona House, Co. Westmeath, in April 1895. Preliminary Report: Prof. G. M. Minchin.—The Complete System of the Periods of a Hollow Vortex Ring: H. C. Pocklington.—India's Contribution to Geodesy: General Walker, F.R.S.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—Kjeldahl's Process for the Determination of Nitrogen; Dr. Bernard Dyer.—The Action of Nitrous Acid on 1 : 4 : 2 Dibromaniline: Prof. Meldola, F.R.S., and E. R. Andrews.—Derivatives of Succinyl and Phthalyl Dithiocarbimides: Prof. Dixon and Dr. Doran.

ROYAL INSTITUTION, at 3.—The Liquefaction of Gases: Prof. J. Dewar, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.

FRIDAY, MAY 17.

QUEKETT MICROSCOPICAL CLUB, at 8.

EPIDEMIOLOGICAL SOCIETY.—Paper by Dr. Washbourn.

SATURDAY, MAY 18.

LONDON GEOLOGICAL FIELD CLASS (Cannon Street Station), at 2.17.—Excursion to view Escarpment Valleys from the Weald to Chalk.

GEOLOGISTS' ASSOCIATION (Cannon Street Station), at 1.35.—Excursion to Betchworth and Headley.

ESSEX FIELD CLUB (Chingford, 2 p.m., and High Beach, 4 p.m.).—Inspection of Forest, and Paper by Mr. E. N. Buxton, on Plan for forming a Protected Area for certain Birds in Old Waltham Forest District.

MONDAY, MAY 20.

SOCIETY OF ARTS, at 8.—Japanese Art Industries: Dr. Ernest Hart.

ROYAL GEOGRAPHICAL SOCIETY, at 8.45.—Meeting to Commemorate the Fiftieth Anniversary of the Sailing of the Arctic Expedition under Sir John Franklin.

VICTORIA INSTITUTE, at 4.30.—Prof. E. Hull, F.R.S.

MEDICAL SOCIETY, at 8.30.

TUESDAY, MAY 21.

ROYAL INSTITUTION, at 3.—Thirty Years' Progress in Biological Science (II.): Prof. E. Ray Lankester, F.R.S.

SOCIETY OF ARTS, at 8.—Commercial Education in Belgium: Prof. William Layton.

ZOOLOGICAL SOCIETY, at 8.30.—On the Ornithological Collections made by Dr. Donaldson Smith during his Recent Expedition in Somaliland and Gallaland: Dr. R. Bowdler Sharpe.—A Synopsis of the Genera and Species of Apodal Batrachians, with Descriptions of a New Genus and Species (Bdellophis vittatus): G. A. Boulenger, F.R.S.—List and Distribution of the Land-Mollusca of the Andaman and Nicobar Group of Islands in the Bay of Bengal, with Descriptions of some New Species: Lieut.-Colonel H. H. Godwin-Austen, F.R.S.—On a New Species of Hedgehog from Somaliland: Dr. J. Anderson, F.R.S.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Last Ballot for Members for the Session.

ROYAL STATISTICAL SOCIETY (Royal United Service Institution), at 5.—Municipal Finance: E. Orford Smith.

ROYAL PHOTOGRAPHIC SOCIETY, at 8.—Apparatus for Process Photography: Wm. Gamble.

ROYAL VICTORIA HALL, at 8.—The History of a Myth: Prof. Sollas, F.R.S.

PATHOLOGICAL SOCIETY, at 8.30.

WEDNESDAY, MAY 22.

SOCIETY OF ARTS, at 8.—The Dressing and Metallurgical Treatment of Nickel Ores: A. G. Charleton.

GEOLOGICAL SOCIETY, at 8.—On a Human Skull and Limb-Bones found in the Palæolithic Terrace-Gravel at Galley Hill (Kent): E. T. Newton, F.R.S.—Geological Notes of a Journey round the Coast of Norway and into Northern Russia: G. S. Boulger.—On Rhætic Foraminifera from Wedmore (West Somerset): Frederick Chapman.

THURSDAY, MAY 23.

ROYAL INSTITUTION, at 3.—The Instruments and Methods of Spectroscopic Astronomy: Dr. W. Huggins, F.R.S.

SOCIETY OF ARTS, at 4.30.—The Northern Balochis: their Customs and Folk-lore: Oswald V. Yates.

INSTITUTION OF ELECTRICAL ENGINEERS (Society of Arts), at 8.—On the Recent Development of the Single-Acting High-Speed Engine, for Central Station Work: Mark H. Robinson.

FRIDAY, MAY 24.

ROYAL INSTITUTION, at 9.—The Absolute Measurement of Electrical Resistance: J. Viriamu Jones, F.R.S.

LINNEAN SOCIETY, at 3.—Annual Meeting.

PHYSICAL SOCIETY, at 5.—On Mixtures of Ethane and Nitrous Oxide: Dr. Kuenen.—The Measurement of Cyclically Varying Temperature: H. F. W. Burstall.

SATURDAY, MAY 25.

GEOLOGISTS' ASSOCIATION (Paddington Station), at 10.2 a.m.—Excursion to Goring. Directors: J. H. Blake and W. Whitaker, F.R.S.

LONDON GEOLOGICAL FIELD CLASS (Waterloo Station), at 3.5.—Excursion to the Bagshot Sand Hills at Frimley.

ROYAL BOTANIC SOCIETY, at 3.45.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Æsthetic Principles: H. R. Marshall (Macmillan).—Crystallography: Prof. N. Story-Maskelyne (Oxford, Clarendon Press).—A Primer of Mayan Hieroglyphics: Dr. D. G. Brinton (Boston, Ginn).—The Migration of British Birds: C. Dixon (Chapman).—Electricity in our Homes and Workshops: S. F. Walker, 3rd edition (Whittaker).—The Practical Telephone Handbook, J. Poole, 2nd edition (Whittaker).—The Land Birds in and around St. Andrews: G. Bruce (Dundee, Leng).—Wild Nature won by Kindness: Mrs. Brightwen, 6th edition (Unwin).—The Elements of Botany: F. Darwin (Cambridge University Press).—John Dalton and the Rise of Modern Chemistry: Sir H. E. Roscoe (Cassell).—Royal Natural History, Vol. 3 (Warne).—Geschichte der Explosivstoffe. I. Geschichte der Sprengstoffchemie der Sprengtechnik und des Torpedowesens: S. J. von Romocki (Berlin, Oppenheim).—The Scientific Transactions of the Royal Dublin Society, Vol 5 (series 2): The Brain of the Microcephalic Idiot: Prof. D. J. Cunningham (Williams).—Royal University of Ireland. Examination Papers, 1894 (Dublin, Ponsonby).—A Manual for the Study of Insects: J. H. and A. B. Comstock (Ithaca, Comstock).—Sitzungsberichte der K. B. Gesellschaft der Wissenschaften, Mathematisch-Naturwissenschaftliche Classe 1894 (Prag).

PAMPHLETS.—Summary Report of the Geological Survey Department for the Year 1894 (Ottawa, Dawson).—Elasticity a Mode of Motion: R. Stevenson (San Francisco).—Kindergarten Mathematics (series A) Algebra, Part 1: M. H. Senior (Oldham, Bardsley).—Petroleum, its Development and Uses: R. N. Boyd (Whittaker).—Notes on the Geology of the Island of Cuba: R. T. Hill (Cambridge, Mass.).—Jahresbericht der K. B. Gesellschaft der Wissenschaften, 1894 (Prag).

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THURSDAY, MAY 23, 1895.

WERNER VON SIEMENS.

The Scientific and Technical Papers of Werner von Siemens. Translated from the second German edition. Two volumes. (London: John Murray, 1892 and 1895.)

THESE two large volumes form a complete history of the work of Werner Siemens, and give a very vivid impression of his unceasing activity. In addition to building up one of the largest commercial houses on the continent, and by his inventions and discoveries materially assisting in almost every step which, during the last fifty years, has been made in the application of electricity to the service of man, he has found time to conduct long researches on subjects unconnected with his technical work, and, particularly in his later years, has written several important papers on meteorology. It is chiefly, however, in connection with electro-technology that the name of Siemens is famous, for it is this subject that Werner Siemens in Germany, and Sir William Siemens in England, have made particularly their own.

The first of the volumes under notice contains the "scientific" papers, while the second contains the technical ones; the papers in either volume being arranged in chronological order. The distinction drawn between the scientific and technical papers is more apparent than real, for in most of the papers included under the first of these heads it is very evident that the investigations were suggested by some difficulty met with in practice, or were undertaken with a view to some practical application. Hence it is questionable whether it would not have been better to keep all the papers together, arranging them in chronological order, so as to render the relation between the experimental or theoretical investigation and its practical application more obvious.

The first paper in chronological order is a note on "an application by Second-Lieutenant Werner Siemens for a patent for a process of dissolving gold by means of the galvanic current, and for gilding by the wet method." Although no complete account is given of the method employed, this note is of interest for two reasons. In the first place, the experiments which led to the discovery of this method of electro-gilding were made in a cell at the citadel of Magdeburg, in which place, on account of his participation in a duel, young Siemens was at the time a prisoner; the chemicals and apparatus employed being procured and smuggled into the fortress by a friendly chemist of the town. In the second place, it was the sale of the patent rights in this invention in England which supplied the brothers Werner and William with the necessary funds to carry on their experiments, and so helped to lay the foundation of the important firms of Siemens and Halske in Germany, and Siemens Bros. in England.

Although still in the army, Werner Siemens continued his scientific experiments, the next discovery of importance having reference to the insulation of electric wires with gutta-percha. When the newly-discovered substance, gutta-percha, was first put upon the English market, William Siemens sent a specimen to his brother, who, being at that time engaged in an attempt to discover a practicable method of insulating underground telegraph wires, immediately proceeded to try if this substance was suitable for the purpose, and found that even a thin layer when freed from moisture possessed sufficient insulating power. In addition, the property which gutta-percha possesses of becoming plastic and sticking together when heated, appeared to remove the difficulty of making sound joints between the separate pieces of the covering. At first a hot gutta-percha strip was pressed round the wire by means of grooved rollers, and cables insulated in this way were used on a short underground telegraph line between Berlin and Gross-Beeren, as well as for the submarine mines, the first of their kind, which Siemens laid down for the defence of Kiel harbour. It was found, however, that the method of covering was defective, since the material rolled round the wire often did not stick well together. In order to overcome this difficulty, Siemens, in conjunction with his future partner, Halske, invented a machine by means of which gutta-percha could be continuously pressed round the wire without any seam. The plastic gutta-percha is in this machine forced into a metal box having a number of holes drilled through two opposite sides: the holes on the lower side being of such a size as to just allow the passage of the uncovered wire, while the holes on the upper side are the size of the finished insulated wire. The wires pass through the lower narrow holes into the space filled with hot gutta-percha, and come out through the upper holes covered with a uniform and seamless coating.

In consequence of the perfection with which wires could be insulated by this new method, Siemens was employed in designing and laying the Prussian State telegraphs, and in this connection devised a method for testing the perfection of the insulation during the manufacture of the cable, and also a system of tests for localising the position of any "faults" which might occur after the cable was buried in the ground. While superintending the laying of the Red Sea cable, these systematic tests were further elaborated by Siemens, and the success which attended the laying of this cable, as well as the numerous others laid by his firm, may be traced in a great measure to the severe and continuous testing to which the cables were subjected during the process of manufacture and the subsequent laying.

In practically all the earlier telegraph lines of the Prussian telegraphs, underground conductors were employed, since Siemens considered they were better than overhead conductors, being less liable to malicious or accidental injury. In addition, they are unaffected by the atmospheric electricity, which in a dry climate often renders the overhead lines unworkable. Although these underground lines were in after years a source of constant trouble, on account of the frequent break-downs, attributed by Siemens to careless and defective repairing, yet their use led him to two very interesting discoveries. In the first place, he found that an underground cable acted like a large Leyden jar, the copper conductor forming the inside, and the moist earth the outside coating. On this account, it was found necessary to design special apparatus to work satisfactorily through these underground lines, and the practice obtained in designing such instruments must have stood him in good stead when he

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came to deal with submarine cables, in which the same capacity effect is met with. The second point was the observation that very strong earth currents—that is, electric currents through the crust of the earth—were produced whenever the aurora borealis was visible.

There is one paper which, although it is included in the first volume, certainly describes a rather amusing practical application of electricity. Werner Siemens, with a party of friends, had ascended the Cheops pyramid, and after reaching the top they noticed that the wind, which had been continually increasing in strength, was raising the sand of the desert with a continuous whirling motion. "When it had arrived at the highest step we noticed a whistling noise, which I ascribed to the increasing violence of the wind. The Arabs, who were squatted around us on the nearest steps, sprang up suddenly with the cry 'Chamsin,' and held up their forefinger in the air. There was now a peculiar whistling noise to be heard, similar to that of singing water. We thought at first that the Arabs were uttering this sound, but I soon satisfied myself that it also took place when I stood upon the highest point of the pyramid and held up my own forefinger in the air. There was also a slight, hardly perceptible, prickling observable on the skin of the finger which was opposed to the wind. I could only explain this fact, observed by all of us, as an electrical phenomenon, and such it proved to be. When I held up a full bottle of wine, the top of which was covered with tinfoil, I heard the same singing sound as when the finger was held up. At the same time little sparks sprang continually from the label to my hand, and when I touched the head of the bottle with my other hand, I received a strong electric shock. It is clear that the liquid inside the bottle, brought into metallic connection with the metallic covering of the head of the bottle through the damp cork, formed the inner coating of a Leyden jar, whilst the label and hand formed the outer coating. When I had completed the outer coating of my bottle by wrapping it in damp paper, the charge was so strong that I could make use of it as a very powerful weapon of defence. After the Arabs had watched our proceedings for a time with wonder, they came to the conclusion that we were engaged in sorcery, and requested us to leave the pyramid. As their remarks, when interpreted to us, were without effect, they wanted to use the power of the strongest to remove us from the top by violence. I withdrew to the highest point, and fully charged my strengthened flask, when the Arab leader caught hold of my hand and tried to drag me away from the position I had attained; at this critical moment I approached the top of my flask to within striking distance of the tip of his nose, which might be about 10 m.m. The action of the discharge exceeded my utmost expectation. The son of the desert, whose nerves had never before received such a shock, fell on the ground as though struck by lightning, rushed away with a loud howl, and vanished with a great spring from our vicinity, followed by the whole of his comrades. We had now a full opportunity of carrying out our experiments."

Before 1860, when Siemens published his paper on a reproducible unit of resistance, there was no generally accepted unit, so that it was impossible to compare the results obtained by any one observer with those obtained by any other. The need of such a unit is very well illustrated in one of the early papers in these volumes, where the unit of resistance used in an investigation is said to be the resistance of an iron telegraph wire 2 m.m. thick and 100 Russian versts long! At the present day, with our well-defined systems of electrical units, it is almost impossible to imagine the difficulty and confusion which must have existed when resistances, to take one example, were stated in such terms as that mentioned above. It is true that Jacobi had previously proposed as unit the resistance of a certain copper wire in his possession, and had issued copies of this unit. These copies, however, varied so much one from another as to be quite useless for the more refined and accurate measurements which the previously mentioned tests for localising the faults in underground conductors rendered necessary. Weber also had proposed his "absolute" unit of resistance, but at this time no trustworthy experiments had been made so as to embody this "absolute" unit in a material resistance. Siemens was thus led to the adoption of another arbitrary unit of resistance, and for this purpose chose the resistance at 0° C. of a column of mercury 100 c.m. long and having a cross section of one square millimetre. He employed mercury, since it can be comparatively easily prepared in a practically pure state, and being a liquid its molecular condition, and hence its resistance, does not alter with time, as it was quite possible that of a solid metallic wire might do. This unit, known as the Siemens unit, came into very general use, particularly on the continent. Nevertheless, the Paris Congress in 1881 decided to use as the international unit of resistance the nearest approach possible to Weber's "absolute" unit, in order to bring the resistance unit into agreement with the other electrical units. On this subject Siemens says:—

"It was certainly somewhat hard for me, that my resistance unit, arrived at with so much trouble and labour, which had, speaking generally, made the first comparable electrical measurements possible, then was employed for more than a decennium throughout the world and adopted as the legal international standard resistance for telegraphy should have suddenly to be set aside with my own co-operation." (Siemens was the German representative at the Paris Congress.) "But the great advantage of a theoretically established system of standards consistently carried out necessitated this sacrifice offered up to science and the public interest."

One cannot help sympathising with him in this matter, for it is always hard to disown one's own offspring, particularly after they have had a comparatively long and brilliant career.

Most of the earlier papers in both volumes deal either directly or indirectly with telegraphy. In the remaining portions of either volume, however, a very prominent part is played by papers and inventions in connection with the conversion of mechanical energy into electrical energy. In connection with a form of magneto-electric machine, *i.e.* one in which the magnetic field is produced by permanent steel magnets, for use in telegraphy, Siemens invented a form of armature, which has since been known as the Siemens armature. This armature is shuttle-shaped and has an iron core, the cross

section being something like an H, and has the wire wound longitudinally in the two grooves. Wilde, who may be said to have taken the first step in the direction of the evolution of the modern dynamo, combined two machines with Siemens' armatures, one a small magneto, the other a large machine with electro-magnets in place of the permanent steel magnets. The armatures of these two machines were rotated, and the current from the magneto was led round the electro-magnets of the other machine. In this way, the magnetic field in which the armature of the large machine rotated, was very much stronger than it was possible to obtain with permanent magnets.

"The technical knowledge of the production of electric currents by means of mechanical power had extended thus far," says Siemens, "when I succeeded, in the autumn of 1866, in obviating entirely the need of steel magnets. The well-known fact that the electric current driving an electro-magnetic machine (motor) is considerably weakened by the induced currents produced in the windings of the electro-magnets, made it appear probable to me that by driving a properly constructed electro-magnetic machine backwards, the slight magnetism remaining in the electro-magnets must be considerably increased since the induced currents are then produced in the same direction as those due to the existing magnetism. Experience confirmed my conjecture. I called this new kind of current-producing machine dynamo-electric, as by it mechanical force is directly changed into electric currents, whilst the magnetism only appears as an intermediate product, not as the real source of the current produced."

Siemens communicated a paper on this new dynamo-electric machine to the Royal Academy of Sciences of Berlin, on January 17, 1867. A few weeks later, William Siemens, at his brother's suggestion, communicated a paper to the Royal Society on this subject. This paper was read at a meeting at which Prof. Wheatstone, who, without knowing of Werner Siemens' discovery, had been working at this question, read a paper embodying the same idea. Some time afterwards it became generally known that a provisional patent, which had been kept secret, and which also covered this invention, had been issued to the Brothers Varley in December 1866.

It appears, therefore, that several people hit upon what may be called the dynamo principle almost simultaneously. From the fact, however, that Siemens was the first to publish the discovery, according to the usually accepted principle introduced by Arago, there seems no doubt that his claim for priority is justified.

This claim for priority with reference to the invention of the dynamo is made again and again in several addresses, &c., in the second volume. As most of these papers are mere repetitions, one of another, it is very doubtful whether any good purpose is served by printing more than one, since the reader becomes very tired of being taken over the same ground several times.

At the end of the second volume there are a number of patent claims, &c., for meters to measure electrical energy. The demand for such a meter, which should combine accuracy with a moderate cost, arose directly the supply of electric current for lighting and power purposes became at all general. Such a demand in connection with any electrical subject was always for Werner Siemens almost a mandate, and he at once devoted a good deal of time and attention to supplying this want.

The chief interest of most of the papers is, no doubt, historic; the two last of all, however, have a special interest at the present moment in this country. They form an appendix to the second volume, and have reference to the foundation by Werner von Siemens of the Physico-Technical Institution at Charlottenburg. The reasons given by Siemens for the foundation of such an institution in Germany apply to the case of our own country at the present day, for we are still without such an institution, though, through the munificence of Dr. Ludwig Mond, the region of usefulness of the Royal Institution is to be extended in this direction. Siemens, during his long and successful career, had noticed that although the general standard of scientific education was probably higher in Germany than in any other country, the result was to produce not so much scientific workers and discoverers as teachers.

"Scientific investigation," he says, "itself is nowhere a life vocation in the State organisation, it is only a permitted private business of the learned besides their vocation, teaching business. . . . It must, however, be pointed out as a waste of national strength, that highly gifted inquirers, talents such as only seldom come to light, are heavily burdened with professional (? professorial) labours, which others would perhaps perform even better, and are thereby in great measure withdrawn from science itself, to which they would bear incalculable service if they could give themselves up entirely to it. But it is a still greater pity that so many talented and highly-cultured young students find no opportunity to carry out scientific work. The unfortunate consequence in most cases is that scientific labours which would animate and fructify whole domains of life, remain undone, and that, in the struggle for existence, talents do not develop or fall to the ground unrecognised, which under more favourable circumstances would have been able to perform great things to the honour and to the material advantage of the country. It is to be feared that the advantage . . . of better scientific instruction and of more widely-spread scientific culture, will soon be lost . . . if it is not supported by State organisations. These organisations would have to fulfil a double purpose, to advance scientific inquiry generally and to aid industry by means of the solution of scientific technical problems and questions which are essential to its development. . . . In order to make clear the great importance which such an institution, well supplied and liberally endowed, would have on the development of industry, a short retrospect of the history of this development is quite sufficient. We see this everywhere associated with persons and institutions, where it was possible by specially favourable conditions that scientific researches went hand-in-hand with their technical applications. The scientific light, which in consequence led technical combinations and methods, gave such institutions such a preponderance over others that the cost of experiments was not only covered by the higher commercial results, but also whole branches of industry were radically transformed by them, and new ones of great importance created. . . . This combination is most easily realisable in chemical manufacture. . . . More unfavourable is, however, the position of the trades depending on mechanical bases. Exact physical experiments demand much more costly instruments and specially-prepared rooms. . . . If the State, therefore, confines itself as heretofore only to looking after instruction, the mechanical crafts necessarily lag behind the chemical in their development."

Thus spoke Werner Siemens, a man who, by his long and eventful life, was specially qualified to speak with authority on this subject, and the results which have, during the few years of its existence, already been achieved at Charlottenburg are proving him a true prophet.

In conclusion, we may say that these volumes will be found most interesting, not only on account of the insight they give regarding the development of the electrical industry, but also on account of the interesting personality which pervades the whole. W. Watson.

ATMOSPHERIC PRESSURE OF THE NORTH ATLANTIC OCEAN.

Répartition de la Pression Atmosphérique sur l'Océan Atlantique Septentrional, d'après les Observations de 1870 *a* 1889, *avec la Direction Moyenne du Vent sur les Littoraux.* Par le Capitaine G. Rung. (Copenhagen: 1894.)

THIS Atlas, showing the monthly and annual atmospheric pressure and prevailing winds over the North Atlantic and connected seas, is a fine example of cartography and typography. The monographs for this and the other oceans have generally dealt only with February, May, August, and November; but this work presents us with the results for each of the twelve months, and for the year, on a mean of the twenty years from 1870 to 1889.

The really heavy part of the work carried out by Captain Rung has been the calculation of the monthly means from the nine years' daily weather charts of the Danish and German meteorologists from December 1880 to November 1889, including the similar charts of the Meteorological Council for the year ending August 1883. This has been done for eighty points over the ocean between lat. 10° and 77° 30′ N. and between long. 25° E. and 80° W.

It being desirable that the discussion should cover a longer period than nine years, the twenty years ending with 1889 were adopted, these years being selected with the view of utilising the fifteen years' means (1870–84) for this part of the globe which have been published in Buchan's "*Challenger* Report on Atmospheric Circulation," thus greatly facilitating the inquiry. The means for the subsequent five years were independently worked out, and thereafter combined with Buchan's to make up the twenty years' means. The next step was to bring, by the usual method of differentiation, the nine years means of the ocean stations to approximate means for the twenty years. Table iv. gives the means thus calculated for ninety-two coast or land stations surrounding the ocean, and Table v. for the eighty ocean stations. The mean directions of the wind have been calculated for the stations in Denmark and its colonies; but for all other stations the data have been taken *simpliciter* from the "*Challenger* Report." It might materially have aided the inquiry in the north-western part of the ocean if means for pressure and wind direction had been calculated and given for the Labrador stations at Hoffenthal, Zoar, Nain, Okak, Hebron, and Rama, the observations at which have been published from 1882 to 1889.

The monthly and annual means for the eighty ocean stations, and the charting of the results on the thirteen maps, constitute the novel part of Captain Rung's work, and must be regarded as a substantial addition to our knowledge of the meteorology of the North Atlantic. This remark holds good emphatically as regards the northern half of this ocean, and for the five months from May to September. Thus, for these months, we have now a more accurate knowledge of the distribution of atmospheric pressure and of the prevailing winds north of latitude 60 than could have been obtained from any work previously published on the subject.

But such well-merited praise cannot be extended to the working out of the results for the five winter months from November to March. An examination of the Danish and German daily weather-maps of the Atlantic of the nine years for these months shows that over the whole ocean to the north of a line drawn from St. John's, Newfoundland, to Valentia, observations from a ship at sea is an event of extremely rare occurrence. The consequence is that the monthly means for this important region, from which fresh information is so desirable, have been obtained wholly from the observations made at the land stations of this part of the ocean. Hence the results given in the Atlas cannot be regarded as a contribution to the meteorology of the ocean. In this Atlas, what strikes one at first sight as new fact is the distribution of atmospheric pressure during the winter months from the south-west of Greenland round by Iceland to north of Norway, particularly the three or four distinct areas of pressure a little lower than prevails generally over this region. But a close examination of the daily weather-maps themselves suggests the idea that these three or four low-pressure systems may be no more than the outcome of an interpretation, made in constructing these daily maps, of the amount of pressure over the ocean drawn from the pressure and winds observed at the land stations, the interpretation being made in the complete absence of observations at sea. Thus the observations made at the Greenland stations since 1840 amply show that the winds on its coast are very greatly deflected from their true direction, as that would be determined by the distribution of pressure, by the high ground and valleys near the coast. It is in this connection that a discussion of the Labrador observations would have come in so handy.

Captain Rung has raised a side issue to his report in a discussion of the distribution of atmospheric pressure in the interior of Southern Scandinavia, where the Atlas shows a singular local excess of pressure in the winter months, which excess is also plainly shown by his monthly means of the Norwegian, Swedish, and Danish stations. In looking closely at this matter, it is necessary to leave out of view the means for Dovre, Tonset, and Röros, which approach to, or exceed, 2000 feet above the sea, their positions not being suitable in discussing small sea-level differences of pressure such as are here dealt with. We have calculated afresh the January means for all other stations not exceeding 620 feet in height, for the same twenty years, and obtain a set of figures differing widely from those published in the Atlas, which give no countenance to the idea of a local excess of pressure in winter over this region. To test the matter in another

way, several means for the same stations for ten years each from the observations of the last quarter of a century have been calculated, with the result that none of these series show an excess, the only variation being such as appears in the isobars of this region for December, January, and February in the maps of the "*Challenger* Report." Finally, on comparing the means for the twenty years given in the Atlas with those we have newly calculated, the strange result comes out that to the north of a line drawn from near Hernösand in Sweden, to a point fifty miles to the north of the Skaw, the pressure means of the Atlas are all in excess of the other means from 0·030 inch downwards, whereas to the south of this line, the pressure means of the newly calculated stations are all in excess of those of the Atlas from 0·030 inch downwards. For now many years, this error has appeared in nearly all maps published on the continent showing the distribution of atmospheric pressure over its surface; and it received greater currency by being adopted in 1887 in the *Meteorological Atlas*, forming part of *Berghaus' Physical Atlas*. It is probable that the error would never have appeared, if there had been established in Southern Scandinavia a true high level Meteorological Observatory, that is, an observatory situated on a peak such as we have in the Ben Nevis Observatory and the other high level observatories on the continent.

OUR BOOKSHELF.

Text-book of Anatomy and Physiology for Nurses. Compiled by D. C. Kimber. (London: Macmillan, 1895.)

THIS is a book of 268 pages on anatomy and physiology, written by a member of the nursing profession. The author states that the text is compiled from many well-known books, and that nearly all the illustrations are figures taken from standard works. On first taking up the book, we were surprised at the amount of detailed anatomy it is considered necessary to impart to nurses in the American training schools, and we are told that the scheme of the book has been practically worked out in class-teaching. So far as we can judge, the class-teaching is conducted in a radically wrong way. In the first place, there are no directions for practical work anywhere in the book. Anatomy and physiology cannot be taught to any one without observation; and with women entering so practical and serious a profession as nursing, actual observation and simple experiments could be insisted upon and more easily carried out than with a class of school-girls. If the work is to be considered as a text-book only, it is far too difficult to be put at once into the hands of a nurse; yet the author makes no statement about previous knowledge. The descriptions given of structure and functions must surely be in many cases very difficult, if not impossible, for beginners to understand, for such descriptions often consist of a few sentences slightly modified, apparently taken from full accounts found in well-known books. Such detached sentences alone, although correct enough in themselves, can lead to no proper understanding of the subject. The book is burdened with much detailed anatomy, such as of the bones, muscles, development of blood-vessels, which although possibly of use to nurses, would have better given place to a simple, clear, and connected description of the general structure and functions of the body. The arrangement observed in the book is not good, and some subjects are treated of in a wrong connection. For instance, the disposition and action of the muscles of the eyeball are considered in the chapter on muscles in general, as is also the action of the muscles of respiration, and these descriptions are consequently inadequate. There are instances of anticipation of topics, strange sentences thrown in, which must be unintelligible until matters treated of later have been grasped. In the chapter on the heart, the author describes almost at once the arrangement of the muscular fibres of the chambers, before even a general description of the organ is given, or the words auricle and ventricle defined; in fact, the whole description of the heart should be much clearer, and the account of its action fuller and more accurate. It would be easy enough to point out some loose and erring statements, and one or two misprints; we are told, for instance, that water is produced "when two molecules of oxygen unite with one of hydrogen." It is far the best for nurses to learn the anatomy and physiology they require from anatomists and physiologists, and nursing from nurses. The book, however, contains a full and excellent glossary.

Calcareous Cements: their Nature and Uses. By G. R. Redgrave. (London: C. Griffin and Co., Limited, 1895.)

MANY valuable contributions to the wide literature of cements have appeared from time to time in the engineering and chemical journals devoted to the industries. Several of these are of foreign origin.

The author of this work is to be congratulated on having collected, in a handy volume of 222 pages, all the most interesting and important facts dealing with the history, manufacture, testing, &c., of "Calcareous Cements."

The volume is divided into sixteen chapters and eight appendices. The first three chapters are devoted to a historical review of the subject, and then follow in systematic order chapters dealing with the various stages in the manufacture of Roman and Portland cements.

Chapter viii. contains a short but accurate account of the researches of Fremy, Le Chatelier, and Landrin on the setting of cement. The author has given to the subject of cement-testing its fullest importance; the various methods and appliances for determining the strength of cements are fully described, and the use of Unwin's formula is clearly stated. The last chapter deals with different specifications for cement. In connection with this subject, the author deplores the want of a uniform and generally accepted system of cement-testing in this country; and, in the hope no doubt of stimulating consumers and manufacturers to an agreement, he gives, in Appendix E, a full translation of the German standard tests.

It is not encouraging to find that an industry which originated in England with the work of Aspdin and Smeaton is slowly but surely passing over to the continent. The annual production of cement in Germany equals that in England; but that is not all, *starting with raw materials of an exceedingly unfavourable character*, Germany produces a finer and more reliable cement than that manufactured in England, and at no greater cost. French cement is also, as a rule, superior to the English article.

A figure of Scheibler's, or any other form of calcimeter, in the chapter on chemical analysis, due to Mr. Spackman, would help to make the work more complete in itself; and Schumann's convenient apparatus for determining the specific gravity of cement is not mentioned; the cumbrous Keates' bottle is alone described and figured.

The illustrations, thirty in number, are good, and the book is supplied with a very complete index.

E. A. W.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The Origin of the Cultivated Cineraria.

It appears to me that Mr. Bateson very imperfectly appreciates the nature of the problem of which he has hazarded what I venture to think an ill-considered solution.

In my last letter I pointed out briefly the grave objective difficulties which he had to face in substantiating his case. As Mr. Bateson is, by reputation, a serious naturalist, I think it was his duty to take up the challenge which I virtually threw down to him, and deal with the points which I brought under his consideration. This he has not chosen to do, but falls back again upon his "historical evidence" and his dialectic.

Now I must confess that I am myself as much bored as I suppose most people must be with the "modern Cineraria." And I grudge the time demanded for the discussion of a point which I brought forward as a merely incidental illustration. It may, however, be useful in saying all that I intend to say in reply to Mr. Bateson, to make a few general remarks on the whole subject.

It is apparently the fashion nowadays for the younger biologists to undertake the reconstruction of the Darwinian theory. The field is undoubtedly open, and posterity may safely be trusted to appreciate the value of their labours. But I cannot but observe that as between them and the author of the theory, there is this difference. Mr. Darwin, as he has told us, spent the best part of his life in studying patiently and sifting critically a vast mass of observation and fact. Ultimately he permitted himself to draw certain conclusions. The result is that if you take any statement which Mr. Darwin has put forward, you may feel assured that behind it is a formidable body of carefully considered evidence not likely to be upset. With the modern writers on evolution, the position is exactly the opposite. They launch their theories gaily on the world, and on demanding their substratum of facts, one is told that that is a matter for future collection. I myself am old-fashioned enough to think that, of the two methods, that of Mr. Darwin is the sounder, the more scientific, and in the long run the more convincing.

I have pointed out again and again the vast wealth of material for the scientific study of variation which is presented every day to the eyes of any one engaged in horticultural practice. The difficulty is that few persons possess either the scientific capacity, the patience, or the leisure for its profitable utilisation. We want, in fact, for the purpose a second Darwin, or at least a Herbert.

In his "Variation of Animals and Plants under Domestication," Mr. Darwin made a use which was remarkably effective of the observations made by "practical men" in horticultural literature. They served his purpose in establishing, as had never been done before, the amount and character of the variation which was possible under artificial conditions, and therefore, by analogy, under natural. But this class of evidence appears to me unsatisfactory for the investigation of the further problem which is at the moment of supreme interest, the nature and laws of variation itself. I think that Mr. Darwin squeezed out of it all that it would profitably yield. And for this reason: the evidence is not scientific—that is to say, it was never drawn up by persons having in view the requirements of scientific exactitude. Those who gave it have been pressed into court in a cause in which they never contemplated engaging. This has the merit of ensuring that their evidence is unbiassed, but it does not allow of its being pushed further than what it is capable of proving.

The defects of horticultural evidence may be illustrated in a variety of ways. One or two will suffice. In the first place, is the weakness of its nomenclature. Horticulturists are not, for the most part, skilled botanists. When they give a plant name, it is impossible to be sure that it is what a technical botanist would accept. It is as if one were reading the writings of a chemist, and when he mentioned potassium, the doubt occurred as to whether it was not lithium which was intended. I do not mean to imply any censure on the horticulturists: they use names current at the moment which are good enough for practical purposes, though they will not stand a critical test. But in after years no technical botanist would dream of accepting them as unimpeachable.

Again, it has often been found that where remarkable hybrids have been recorded, it has been ascertained later that no cross has in point of fact been effected at all. Yet the original announcement will be quoted, and often has been as an undoubted evidence of the fact.

I arrive, then, at the conviction that if any profitable use is to be made of horticultural experience in the study of variation, the so-called historical evidence will have to be discarded. Every step of the investigation must be made under the actual eye of a competent observer, and nothing taken at second-hand.

I will now return to the Cineraria. The feral form had been long lost to cultivation, but some years ago it was reintroduced to Kew from the Canaries. Mr. Rolfe, a member of my scientific staff, illustrated it in the *Gardeners' Chronicle* in 1888, and pointed out the striking changes which it had exhibited under cultivation. These have subsequently interested me because I have been endeavouring to collect facts as to the rate of variation.

Now Mr. Bateson, solely on what he calls historical evidence, still asserts, and in the face of the difficulties which I have pointed out that such a theory presents, that the modern Cineraria is of hybrid origin. Very well; let us assume that as a provisional hypothesis. How is it to be tested? It is easy to see from an analogous case. The horse and the zebra have been crossed; are we justified in asserting that the last winner of the Derby is of zebra descent? The criteria are two, and I think two only: (1) an uncontested pedigree: (2) palpable marks of parental characters.

Now, with regard to (1), practically in plants it cannot be obtained. We can only fall back upon "historical evidence." I have attempted to show above, in a general way, how little scientific value can ordinarily be attributed to this. One cannot be sure that the asserted parents were what they are stated to be. But my object was not to undermine the weight of what Mr. Bateson has brought forward. I accept it and reject it as wholly irrelevant. As my friend Prof. Rolleston was fond of saying, it would be valueless evidence even to convict a poacher.

The fact that certain shrubby Cinerarias with hoary leaves and one with yellow flowers were crossed (if they really were) early in the century, proves nothing as to the existing Cineraria, any more than the cross between the zebra and the horse does as to the parentage of any existing horse.

These shrubby Cinerarias were, as Mr. Bateson states, propagated by cuttings (they are not too easy to strike); and like many other interesting plants, they disappeared from all but botanic gardens towards the middle of the present century.

As I am quite unable, then, to attach any weight to the so-called historical evidence, because I fail to see that it establishes any filiation between the plants with which it deals, strikingly different as they are, and the plant with which I am dealing, there is nothing left but to try (2), and see what evidence of its parentage the plant itself affords.

Now, it is well known that organisms of hybrid origin preserve, in some degree, their parental characters, and this has even been shown to be true of their histological elements. Modern taxonomic botany has met with considerable success in the analysis of plants of hybrid origin into their constituents. The Floras have in consequence been cleared of a multitude of dubious plants, the real nature of which can now be accounted for. And the validity of the method has been established by the results of a corresponding synthesis. We had, then, no hesitation at Kew in applying the test to the Cineraria. Although it had often been examined before, with the assistance of some members of my staff I made a fresh examination. I took copious specimens of *Cineraria cruenta*, and of an average cultivated form, and carefully compared them point by point. Except in the multiplication of the florets in the heads, especially of the ray-florets, we could distinguish no tangible morphological difference. In fact, having accidentally mixed up leaves belonging to the two parcels, I found myself unable with any certainty to refer them back again. This is pretty conclusive evidence of the actual morphological identity of the vegetative organs of the two plants.

The next thing was to compare the cultivated Cineraria with its reputed shrubby "historical" parents. These present well marked and somewhat peculiar characteristics not readily described in non-technical language. But the cultivated Cineraria does not present the smallest trace of any one of them. As far, then, as the matter admits of investigation at all by any known methods, I regard the conclusion which is generally accepted here as a sound one. At any rate, it rests on a careful

consideration of the objective facts which Mr. Bateson wholly shirks.

I now come to the other point. I put colour change entirely aside for reasons which seem valid to me, and which I may take another opportunity of explaining. Apart from these the cultivated Cineraria exhibits no variation from the feral form which may not be described as dimensional. While the foliage has remained approximately constant, the loose corymbose habit has been contracted into a tight corymb, and the heads of florets have been enormously enlarged. While the feral form stands about five feet high, the cultivated one is about eighteen inches. I am disposed to restrict the term "sporting" to a definite morphological change such as is exhibited in the flowers of the garden Chrysanthemum, and recently in the occurrence of an "ivy-leaved" form of the Chinese Primrose. But except a race of so-called double Cinerarias, which did not take the public fancy, the history of the garden Cineraria does not present, as far as I know, any trace of a real morphological change. If I might venture to use a mathematical analogy, I should say that the form of the Cineraria-function has remained unaltered.

Now the object of these dimensional changes has been to make the plant worked upon handy and convenient for decorative purposes. Those points which were unessential for this purpose have been unconsciously neglected, and their stability has not been affected. But I do not doubt that if it had been otherwise the Cineraria might have been brought by this time to any configuration which the cultivators fancied.

As far as I can make out, the transformation of the Cineraria has taken about sixty years to effect. Mr. Bateson will not complain if I quote a few words from one of his own authorities of about that date:—"One species especially merits cultivation, viz. *C. cruenta*. This may be regarded as the parent of many of those beautiful varieties which are so successfully cultivated by Messrs. Henderson." Now my memory of the cultivated Cineraria goes back some thirty years. I can remember when it was a rather lanky plant, about half the height of the feral form, with a somewhat lax inflorescence and far smaller flower-heads than are now to be seen. The present fashionable Cinerarias, with a very condensed inflorescence and very large flower-heads, only date back some ten or twelve years.

I see, therefore, no reason for abandoning my assertion that the evolution of the modern Cineraria has been slow and gradual, and not *per saltum*, and this is in accord with general horticultural experience. As soon as a new plant is introduced, every one wants to get a form with bigger flowers or floral structures than anybody else. There is only one secure path to this result, and that is by taking advantage of seminal variation and selecting the minutest trace of change in the desired direction. By patiently and continuously repeating the operation, almost any desired result can be obtained. The horticultural gambler may hope to reach it by a "sport," but he will not. *Anthurium scherzerianum* is a good illustration. Introduced in 1862, it was little more than a curiosity: now its enormous and brilliant spathes are a conspicuous object at every flower-show. This has simply been accomplished by progressive selection working on seminal variation.

Mr. Bateson has now the coolness to say that "the hybrid origin of cultivated Cinerarias is of subordinate interest." All I can say is that in that case it is a pity that he wasted three columns of NATURE with a discussion of the subject. I should have thought myself that it was a matter of very considerable importance indeed to be able to form an approximate idea of the amount of change in a given time in an unmixed species, and so obtain some measure of the possible rate of evolution, at least in regard to dimensional characters.

For my part, I think that in the study of evolution we have had enough and to spare of facile theorising. I infinitely prefer the sober method of Prof. Weldon, even if it should run counter to my own prepossessions, to the barren dialectic of Mr. Bateson.

W. T. THISELTON-DYER.

Royal Gardens, Kew, May 13.

Some Bibliographical Discoveries in Terrestrial Magnetism.

I HAVE recently made some interesting discoveries pertaining to the history of Halley's famous chart of the Lines of Equal Magnetic Variation (Declination), to which renewed attention is just now being called by Prof. Hellmann's admirable facsimile reproduction of the earliest geomagnetic charts.[1]

The first reproduction in facsimile of Halley's chart was undertaken by G. B. Airy, and published in "Greenwich Observations" for 1869. Airy was led to do this by reason of the fact that he could find no geomagnetician of his time who had ever seen Halley's chart. After diligent inquiry among academies and libraries at home and abroad, it was found that the British Museum possessed a copy, and, it was believed, the *only* copy extant. Since then, Prof. Hellmann has succeeded in tracing two other copies, one at Hamburg (Stadt Bibliothek) and one at Paris (Bibliothéque Nationale), and has also, since the publication of his book (as he has just informed me), come into possession of a copy himself.

I have personally examined the Hamburg and Paris copies, and, during a brief stay in London in March, also the copy in the British Museum used by Airy. I have found, moreover, in the British Museum, three other Halley charts and two Dutch reprints. By a careful and critical study of these various copies, some new light is thrown upon the publication of Halley's chart. To make this apparent, some wearisome details with regard to the various copies will be necessary. I will begin with the British Museum copies.

Catalogue No. 974 (5).—"A new and correct Sea-chart of the Whole World, showing the Variations of the Compass as they were found in the Year 1700, by Edmund Halley." Date (according to the Catalogue), 1701.

The above is the English title of the chart referred to at times by the Latin title, "Tabula Nautica," &c. This copy appears to be the one used by Airy in his facsimile reproduction of the Halley chart published in "Greenwich Observations" for 1869, which in turn has been used for Prof. Hellmann's reproduction. There is no date on the chart, nor the name of the publishing firm. The date 1701, assigned hitherto, is probably due to Halley's defence of his chart, contained in *Phil. Trans.* vol. xxix. (Unabridged), 1714. Halley says, p. 165, "to examine the chart I published in the year 1701, for shewing at one View the Variations of the Magnetical Compass, in all those Seas with which the English Navigators are acquainted." But we find that the above number is dedicated "To his Royal Highness, Prince George of Denmark, Lord High Admiral of England, Generalissimo of all Her Majestie's Forces." As Prince George, consort of Queen Anne, did not bear this title until April 17, 1702,[2] it is evident that the above number is either not the original Halley chart published in 1701, or it is a reprint with a later dedication. If it is to be regarded as an original Halley chart (not a reprint), then a date between 1702 and 1708 must be given it, as Prince George died October 28, 1708. It was published probably not far from 1702, and is in excellent condition.

No. 973 (15). Same title as previous number. Date given in the Catalogue, 1720(?) I found upon examination that this is identical with No. 974 (5). The Catalogue date is doubtless erroneous. This copy is cut into sections and remounted.

No. S. 112 (6). This is a large folio atlas containing a reprint of No. 974 (5), bearing now the name of the publishing firm, R. Mount and T. Page, and having in addition an extra strip, from 90° to 160° E. of London, pasted on the left-hand side, so that the chart now embraces 430° of longitude instead of 360 as before. The Hamburg and Paris copies are exact duplicates of this, the only difference being that they have pasted below a strip bearing the explanation of the chart by Halley. Prof. Hellmann, in the work cited, has given us the Hamburg text. The Paris text differs in the orthography of a few words, and in the spacing of some of the lines. It appears to be the older text, as below it we find the name of the firm as R. and W. Mount and T. Page, while the name of the firm on the Hamburg text is Thomas Page and William Mount, and the former I have ascertained to have been the earlier firm. This English text I have failed to find attached to the British Museum copies.[3]

[1] Neudrucke von Schriften und Karten Über Meteorologie und Erdmagnetismus. Herausgegeben von Prof. Dr. G. Hellmann, No. 4. . . . E. Halley, W. Whiston, J. C. Wilcke, A. von Humboldt, C. Hansteen: Die ältesten Karten der Isogonen, Isoklinen, Isodynamen; 1701, 1721, 1768, 1804, 1825, 1826. 4to. 26 pp. 7 plates. (Berlin: A. Ascher and Co., 1895.)

[2] Rapin de Thoyras's History of England, London, 1751, vol. iii. 1689-1707, p. 544.

[3] The atlas contains, besides "An Account of the Methods used to describe Lines on Dr. Halley's Chart of the Terraqueous Globe," &c., by W. Mountaine and J. Dodson, London, 1758, and copies of the Halley chart revised for epochs 1744 and 1756. It bears the title on the hack: "Tabulæ Nauticul Variationes Magneticas Denotentes. E. Halley." It appears to be a compilation of charts, probably by the authors (Mountaine and Dodson) of the revision.

Nos. 974 (6) and 974 (1) are Dutch editions by R. and I. Ottens, of Amsterdam, of the Halley chart as modified and found under No. S. 112 (6). The base of the chart has been changed, but not the lines of equal variation. The dedication to Prince George has been omitted. The dates assigned by the Catalogue are respectively 1735 (?) and 1740. The chief interest in these Dutch reprints lies in the fact that they have a French text pasted on the left-hand side, and a Dutch text on the right-hand side, over Halley's name.

No. 974 (4). "A new and correct Chart showing the Variations of the Compass in the Western and Southern Oceans, as observed in ye Year 1700 by his Maties Command by Edm. Halley." Date given by the Catalogue, 1720, marked doubtful. This chart extends from 50° N. to 59° S., and from $21\frac{1}{2}$° E. to 100° W. of London. It is enclosed by a border; the base of the chart is entirely different from that of 974 (5); yet the equal variation lines, as far as given, are identical with those for the same region on 974 (5). In but one respect is there a difference in the lines, viz. in no case are they drawn over the land, and in a few cases, also, they are slightly extended. It contains in addition the course of the *Paramour Pink*, the ship in which Halley made his observations, 1697–1700, with the chief aid of which he drew the equal variation lines for the Atlantic Ocean. But the matter of chief importance is that this chart is dedicated to *King William III.* This fixes its date. William III. died March 8, 1702. It is highly probable, then, that this is the chart published in 1701, referred to by Halley in the above quotation, and, in consequence, *the original Halley chart.* It is, moreover, reasonable to suppose that Halley would dedicate his first chart to King William III., who had furnished the means for the making of the observations, to which the chart was due. This chart has escaped the attention of all geomagneticians and bibliographers, and the British Museum copy may be the only one in existence.[1]

Another matter of historical interest, apparently unknown to all modern authors in terrestrial magnetism, was ascertained. I find it asserted that the Frenchman, L. I. Duperrey, was the first (1836) to construct the "Magnetic Meridians" for the whole earth, *i.e.* those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle. It seems, however, that this honour should be accorded to an Englishman, Thomas Yeates, who, in 1817, published a chart of the Lines of Equal Magnetic Variation, accompanied by a "New and Accurate Delineation of the Magnetic Meridians." A second edition of this chart was published in 1824. Copies of both editions were found in the British Museum.

Washington, April 20. L. A. BAUER.

The Unit of Heat.

DR. JOLY'S strictures on the units of heat at present in use will meet with a ready endorsement from those who have worked on calorimetry. The large calorie is too large for convenience in most cases, and the small calorie is too small, while the confusion created by different writers using different units with the same name is scarcely reduced by their writing one with a capital and the other with a small c. A unit of convenient magnitude would be one equivalent to about 100 small calories, and 100 calories has, indeed, been adopted as a unit by more than one writer on thermochemistry. There is, however, what may be termed a natural quantity which is nearly equivalent to such a unit, namely, the heat of fusion on one gram of water at 0° C., which is nearly eighty calories. This appears to be just as suitable from other points of view as the heat of vaporisation of one gram of water at constant temperature and 760 m.m. pressure; and if this latter can be recommended on the ground that in defining it we replace the thermometer by the barometer, the former will possess the superior claim of (for all practical purposes) not depending even on the barometer.

If I remember rightly, this unit has already been adopted in one work on thermochemistry.

No doubt the heat of fusion of water requires redetermination; but it should be determinable with quite as much accuracy as the heat of vaporisation.

Neither of these proposed units, however, possess what should be the chief characteristic of a physical unit, namely, a simple relation to other units; and before adopting either of them, it would be well to consider whether some convenient unit related to, say, the electrical units, could not be adopted. A Committee of the British Association would be a body most suited to investigate this matter.

For practical purposes, a quantity which is even of greater importance than the magnitude of the unit adopted, is the relative value of the heat capacity of water at different temperatures. In spite of the large amount of work which has been expended on this subject, great uncertainty still prevails respecting it. The heat capacity of water, and the heat of fusion of ice, are subjects which I have been for some years intending to turn my attention to, and the work is now practically in hand.

Harpenden, May 4. SPENCER PICKERING.

MY objection to the latent heat of water unit is that this is an inaccessible unit on account of the difficulties attending measurements with the Bunsen calorimeter.

Some years ago I began experiments on a gravimetric ice calorimeter. I have not had leisure to go on with them, but the results obtained were very encouraging. The substance was cooled below 0° while hanging suspended from one arm of a chemical balance. This was effected in a double-walled chamber of copper. A tube, stopped by a plug, connected this chamber with a reservoir of water and clear broken ice. The water was previously boiled to expel air. On raising the plug the water at 0° flows rapidly into the calorimeter, and a shell of clear ice forms upon the substance. The effect on the balance is noted, and by observing the change of buoyancy upon the melting of the ice, and knowing the density of ice at 0°, the mass of the latter can be estimated. The weight measurement will extend to about 0·5 of a calorie. In the steam calorimeter the weight measurement extends to 0·1 calorie, or even less.

There is, of course, much to be said for a thermo-dynamic unit. The question is certainly deserving of having the opinions and views of scientific men fully expressed upon it—as Mr. Pickering suggests. A glance at any of the recent accurate thermal work done in England will show what confusion there exists as to what is the calorie, and as to how all the pet calories of various physicists are related. To render many old measurements of value, this last question should be decided. It reminds one of the state of thermometry in De Saussure's time.

Trinity College, Dublin. J. JOLY.

Reputed Traces of Negrito Pygmies in India.

MAY I be permitted to suggest to readers of M. Quatrefages' work on the Pygmies, the English edition of which has recently been reviewed in NATURE, to pause before accepting his conclusions as to traces of Negritos being found in peninsular India.

The evidence he relies on partly consists of a description by M. Rousselet of a half-starved wanderer from Sirjuga, whom he assigns to the race Bander Lokn (or, as it is printed in the English edition, Bandra Lokh) and the tribe Djangal. Any Anglo-Indian with the slightest knowledge of the language, not to say of ethnology, would be amused at such nicknames being applied as definite racial terms. The first simply means monkey-people (equivalent to savage), as applied by dwellers in the plains to the wilder inhabitants generally; and the second, if it can be said to mean anything in the form presented, is simply "jangli," or a dweller in jungle.

The portrait of this "Djangal," from a rapid pen and ink sketch, is a caricature of a somewhat exceptional and by no means typical individual, and affords no trustworthy material for an ethnological discussion.

The "fever-stricken inaccessible" region Sirjuga,[1] from whence this specimen was a fugitive, according to M. Rousselet, is well known to me, and when travelling there I spent some days in the company of the late General Dalton; and not only then, but in connection with the production of his great work on the Ethnology of Bengal, to which I had the privilege of contributing, I had many conversations with him regarding the tribes of that region. I was, moreover, well acquainted with the true Negritos of the Andamans, of whom I had then already seen many; and I do not hesitate to say that I never met with the slightest trace of a Negrito element among the numerous tribes I became acquainted with during many years travelling in the hilly tracts of Western Bengal, the Central Provinces and the Northern Provinces of Madras. Individuals belonging to different tribes

[1] Upon [illegible] Prof. Hellmann with a brief description of this chart, he [illegible] found that Le Monnier, in his [illegible] Lois du Magnétisme, Paris, 1776 and 1778, has reproduced it. Prof. Hellmann's copy of the Halley chart is a [illegible] No. S 112 (6), with the exception that it embraces but [illegible] of longitude. It also has no text.

[1] The district of Sirjuga in Chota Nagpur is not near Amerkantak, nor is it included in the Vindhyan Range, as is stated by M. Quatrefages.

with curly, not really woolly, hair are occasionally to be seen; but I venture to think that such occasional freaks are casual, and wholly without significance; although they were regarded as evidence of a Negroid element in the population by the late Sir George Campbell.

As, in consequence of the statements and theories of M. Quatrefages, the idea is already spreading that traces of pygmy Negrito races are to be found in these parts of India, I contemplate on a suitable occasion, ere long, publishing some notes, made at the time, on the tribes I met with in my travels in the wild regions referred to. I shall therefore say no more at present, save that the evidence culled by M. Quatrefages out of General Dalton's lithographed groups—one of a girl with her hair *cropped* short, and another of two somewhat curly-headed Sonthals—in support of his theory, is not merely feeble, but is liable to mislead.

Sir Wm. Flower has referred to the use by M. Quatrefages of the term Mincopie for the Andamanese. As he points out, there is in reality no such term. How it originated, though long unknown, has been suggested by Mr. Man. Its derivation foiled even the acute research of Sir Henry Yule. Its first use was by Lieut. Colebrooke in the year 1795, but it has not been recognised in any Indian dialect, and does not seem to have ever been in use among Anglo-Indians, any more than is the name Zebu, which is used in some European languages for the humped cattle of India. Such names, and there are a few others, not being current in the country itself, have to be forgotten by those who visit India. I well remember being not understood when I used the term Zebu on my first arrival in Calcutta some thirty years ago. V. BALL.

Dublin, May 13.

Epping Forest: an Explanation.

SOME years ago you were good enough to publish a paper of mine on the conservation of the Forest from the naturalists' point of view (vol. xxvii. p. 447). That paper was written soon after the Forest was taken over by the Corporation of London, when some unpleasant signs of artificial treatment had become manifest, and more especially with reference to certain railway schemes which, in the interest of naturalists, we of the Essex Field Club felt it our duty to oppose. It is a matter of ancient history that our opposition was successful. My object in entering the lists again is to assure your readers, as representing the scientific public, that the controversy which is now going on concerning the management of the Forest has nothing whatever to do with the agitation about the railway scheme of 1883. This statement may appear superfluous, but I am compelled to trespass upon your space because certain unscrupulous critics are in the habit of misleading the public by quoting from that paper published twelve years ago, without giving date or context, and without a single word of explanation as to its object. Moreover, the critics in question have endeavoured, by a method which in other controversial spheres would be called by a very strong name, to make it appear that some of the views put forward in 1883 are opposed to the attitude which, it is well known, I now hold in the present controversies. So far as naturalists are concerned, they may rest assured that nothing that is now being done is in the way of injury to the Forest; far from this, there are signs of marked improvement The policy of the Conservators is to restore the Forest to a natural condition by thinning out overcrowded pollards which are now beginning to injure one another, and to kill off the varied undergrowth which is such a relief to the gloomy barrenness of an unnaturally dense growth of trees. I may point out that the overcrowding is due to two opposite causes, viz. to entire neglect in some parts, and to too much attention in others. The latter cases refer to those parts in which in past times the rights of lopping were severely exercised. Here of course, now that the Conservators have extinguished these rights, the pollards are throwing up straight and lanky branches of a most unsightly character. In those very limited parts which were not formerly pollarded, and which consist of groves of spear trees, no attempt at systematic thinning had been made before the present Conservancy, and here also there is an overcrowding necessitating woodcraft. Within the last few years all that has been done has been done with care, skill, and forethought. I rejoice to be able to bear testimony on this point, and to reassure those who may have been misled from a want of personal knowledge of the nature and history of the district, into giving credence to the intemperate correspondence in the newspapers. R. MELDOLA.

May 21.

PROFESSOR LOTHAR MEYER.

Gestern Abend 11 Uhr entschlief plötzlich sanft und schmerzlos im 65. Lebensjahre mein lieber Mann

DR. LOTHAR MEYER

ord. Professor der Chemie an der Universitat Tübingen.

JOHANNA MEYER geb. Volkmann
mit ihren Kindern.

Tübingen, den 12. April 1895.

WE were thankful his "falling on sleep" was "sudden, gentle, and without pain"; but we grieved he should have left us so soon.

Julius Lothar Meyer was born at Varel in Oldenburg, on August 19, 1830. After completing his school course in the Gymnasium, he studied in the University of Zürich from 1851 to 1853, then at Würzburg from 1853 to 1854; from Würzburg he went to Heidelberg, where he remained till the autumn of 1856, and from thence he migrated to Königsberg, where he remained until Easter 1858. Meyer's original intention was to devote himself to medicine, and he graduated as Doctor in Medicine at Würzburg on February 24, 1854. At Heidelberg he came under the influence of Bunsen, and his work became more and more chemical. At Königsberg his studies were devoted mainly to mathematical physics, under the guidance of F. Neumann. In 1858 he took the degree of Ph.D. at Breslau; and on February 21, 1859, he received leave to teach chemistry and physics. From 1859 to 1866 Meyer was in charge of the chemical laboratory of the Physiological Institute at Breslau. In 1866 he was called to the Royal Prussian *Forstakademie* at Eberswalde, where he remained until 1868, when he went to the *Polytecknikum* at Carlsruhe. In 1876 Prof. Fittig was called from Tübingen to the University of Strassburg, and Lothar Meyer was appointed to fill the vacancy at Tübingen.

He had nearly completed twenty years' work at Tübingen when the summons came. Cerebral apoplexy stopped his labours, on April 11 of this year; and, *plötzlich, sanft, und schmerzlos*, he passed.

It was while teaching chemistry and physics at Breslau that Meyer published the first edition of the work on which his reputation as a philosophical chemist chiefly rests. "Die Modernen Theorien der Chemie" appeared in 1864. A second edition was published in 1872; and since that time have appeared a third, fourth, and fifth edition. At the time of his death Meyer was engaged in the preparation of a sixth edition, which he intended to publish in three, more or less independent, parts. An English translation of the fifth edition, by Messrs. Bedson and Williams, appeared in 1888. In 1883 Profs. Meyer and Seubert recalculated the atomic weights of the elements from the original data, and laid all chemists under a debt of gratitude by publishing their results, under the title "Die Atomgewichte der Elemente aus den Originalzahlen neu berechnet."

Lothar Meyer was one of the earliest investigators of the relations between the properties and the atomic weights of the elements. In the first edition of his "Modernen Theorien" published in 1864 he traced relations between the atomic weights and the chemical values of the elements; and in December 1869 appeared a memoir by him entitled "Die Natur der chemischen Elemente als Funktion ihrer Atomgewichte," wherein he arranged the elements in order of atomic weights, in a single table, and indicated the periodic character of the dependence of properties on atomic weights.

The clear enunciation, and the application in detail, of the most far-reaching generalisation that has been made in chemistry since the work of Dalton, must, undoubtedly, be credited to that great chemist Mendeléeff

but, nevertheless, a perusal of the controversy between Mendeléeff and Meyer shows, I think, that Meyer arrived at the fundamental conception of the periodic law independently of Mendeléeff. Those who are interested in such controversies will find papers by Mendeléeff and Meyer in *Berichte* xiii. pp. 259, 1796, 2043 [1880].

In his discourse to the German Chemical Society on May 29, 1893, "Ueber den Vortrag der unorganischen Chemie nach dem naturlichen Systeme der Elemente," Meyer quotes the words which Laurent had used fifty years before concerning organic chemistry, and applies them to the teaching of inorganic chemistry at the presen time:—*que l'arbitraire y règne sans partage.* If these words can be applied to the teaching of inorganic and general chemistry to-day, how much more fully and literally were they applicable at the time when the first edition of Meyer's "Die Modernen Theorien" appeared thirty years ago! That book has probably done more than any other publication within the twenty years after 1864 to advance the study of comparative chemistry; its influence on the conception of chemistry as an accurate and orderly body of facts and principles has been very great, and has been wholly good. The labour bestowed on the preparation of the first edition of the "Modern Theories" must have been immense. The author speaks in his preface of rewriting the MS. three times. It is true that thirty years ago physical chemistry was practically non-existent, that the facts of organic chemistry could be mastered and held by a man with an ordinary memory, and that one might be a chemist without first being a mathematical physicist. But it is also true that the facts of inorganic chemistry had not been coordinated by the luminous conception of the periodic law, that there was a lack of clearness in the notions of most chemists about the structure of organic compounds—for Kekulé had not yet made his famous ride on the top of the Clapham omnibus—and that the many isolated facts regarding the influence of temperature, time, and the masses of the reacting bodies, on chemical changes had not been gathered together and illuminated by the law of mass action and the conceptions arising from the applications of this law. It was then that "Die Modernen Theorien" appeared; and at once a flood of light was thrown on the whole domain of chemical science. Old problems were made clear, and new problems were suggested. Chemistry entered on its modern phase.

As the study of comparative chemistry progressed—a study which was introduced by the enunciation of the periodic law—it became necessary to know with accuracy the analytical bases whereon rested the values accepted for the atomic weights of the elements. Hence Lothar Meyer was induced to devote a large amount of labour to the somewhat thankless task of recalculating these values; the result of this work, carried out with the help of his colleague Prof. Seubert, appeared in 1883. This work received additional value from the fact that it appeared almost at the same time as Clarke's "Recalculation of the Atomic Weights." Every worker in this department has the data of all previous workers brought to his hand, and presented in the most manageable form.

Besides these two treatises bearing on general chemistry, Lothar Meyer was an investigator in the sphere of experimental chemistry. He has published memoirs on subjects in almost every branch of the science, on the atomic weight of beryllium, on determinations of vapour densities, on the combustion of carbon monoxide, on the preparation of hydriodic acid, on the transpiration of gases, on various organic compounds, and on other matters.

A great chemist has passed away from us; his work remains, and that work will ever be held in remembrance.

M. M. PATTISON MUIR.

NOTES.

THE Institute of France has decided to solicit subscriptio for the erection of a statue to Lavoisier at Paris. It is intende to make the appeal an international one, so that all admirers of Lavoisier may do honour to the memory of one of the creators of modern science. Subscriptions may be sent to the Treasurer of the Committee for the Lavoisier Memorial, 55 quai des Grands-Augustins, Paris.

THE centenary of the Institute of France is to be celebrated next October. The *Times* states that on the 24th of that month the foreign representatives invited to the celebration will be received, and the Minister of Education will hold a reception. On the following day M. Faure will attend a ceremony at the Sorbonne, and a banquet will be held. There will also be a dramatic entertainment and a reception at the Elysée. Chantilly, the future property of the Institute, will be visited on the 27th, by permission of the Duc d'Aumale.

LIVERPOOL, determined that the visit of the British Association in 1896 shall be a success, has taken time by the forelock. At an influential meeting held in the Town Hall last week, it was announced that an executive working committee had been appointed thoroughly representative of the inhabitants of Liverpool and the neighbourhood. The Chairman is the Right Hon. the Lord Mayor of Liverpool, the Vice-Chairmen are Sir W. B. Forwood and Mr. E. K. Muspratt; the Hon. Treasurer, Reginald Bushell, and the Hon. Secretaries, Prof. W. A. Herdman, F.R.S., Mr. J. C. Thompson, and Mr. W. E. Willink. The meeting was very enthusiastic, and the keynote running through the various speeches was to the effect that the welcome extended to the members of the British Association should not in any direction be allowed to compare unfavourably with that at the meeting at Manchester in 1887, which in the matter of subscriptions at present holds the record. From the short statement made by the Hon. Treasurer, this hope seems likely to be realised. Without making any public appeal for funds, but simply putting the matter before a few of his more influential friends, the Hon. Treasurer was able to make the gratifying statement that no less than £1350 had been subscribed. The Committee preferred a subscription list to a guarantee fund, and in this they are no doubt well advised. A donor, however, is not entitled to any privileges as a member of the British Association, by reason of his subscription, but to every subscriber of £10 a member's ticket or two associate tickets will be given, and one associate ticket to subscribers of £5. With this early start, Liverpool ought to have no difficulty in raising the £5000 which Sir W. Forwood regards as the minimum sum required for a successful and record meeting.

THIS year's conversazione of the Society of Arts will be held in the South Kensington Museum on Wednesday evening, June 19.

DR. THORNE THORNE, C.B., F.R.S., has been appointed a member of the General Medical Council for five years, in place of Sir John Simon, resigned.

MR. GEORGE MURRAY has been appointed Keeper of Botany in the British Museum, in succession to Mr. Carruthers, who retires on superannuation.

THE death is announced of Dr. H. F. C. Cleghorn, well known for his work in connection with the organisation and development of the Forest Department of India. He was for some years president of the Royal Scottish Arboricultural Society, and examiner in forestry to the Highland Society. He also took a leading part in the founding of the forestry lectureship in the University of Edinburgh.

The munificent gift made by Mr. Seth Low, ex-Mayor of Brooklyn, and now President of Columbia College, to that college, at a meeting of the trustees a few days ago, places him in the front rank of the world's benefactors. One million dollars for a library building, twelve scholarships for Columbia College for Brooklyn boys, and twelve to Barnard College for Brooklyn girls, eight university scholarships and a university fellowship, make a list of gifts rarely equalled. At the same meeting, Mr. C. Schermerhorn presented 300,000 dollars for a new building. Then the Townsend library, a complete compilation of all the printed matter relating to the American Civil War, in eighty-nine volumes of 600 pages each, larger than an ordinary ledger, which was begun six months before the war, and is the result of thirty-three years of unceasing labour by Thomas S. Townsend, was formally presented to the college, together with an encyclopædia of reference to it, and 4000 dollars to complete the encyclopædia.

The trustees at the same meeting, following the recommendation made by the National Academy of Sciences at their recent meeting, awarded the Barnard medal to Lord Rayleigh for the discovery of argon. This gold medal, which has a value of 200 dollars, is awarded every five years to the investigator who makes within the preceding five years the most valuable discovery in physics or astronomy, in accordance with the will of President F. A. P. Barnard, who died in 1889, and was the immediate predecessor of Mr. Seth Low.

The Brooklyn Institute has just sustained a great loss in the retirement of General John B. Woodward, who has been president for eighteen years, covering the entire epoch of the great development and expansion of the Institute. He will be succeeded by Mr. A. Augustus Healy.

Sir William Dawson has sent us a printed statement, in which he traverses the arguments against the organic nature of *Eozoon Canadense*, brought forward by Dr. J. W. Gregory and Prof. Johnston-Lavis, in a recent paper entitled "Eozoonal Structure of the Ejected Blocks of Monte Somma," noted in our issue of January 10 (p. 251). He states a number of facts which indicate "that the specimens of Eozoon found in the Laurentian limestone of Canada in no respect resemble in their associations and mode of occurrence the banded forms from Mount Somma described in the paper in question."

A strong earthquake disturbance of about five seconds' duration occurred at Florence at nine o'clock on the evening of Saturday last, and was felt at Bologna four minutes earlier. Two hours later another shock was felt. Many of the houses in Florence were injured by the movements, but the damage appears to have been greater in the surrounding villages—Grassina, Lapaggi, and San Martino, where the church was destroyed. At Orezzo the earthquake is said to have lasted ten seconds, and there were two distinct shocks at Siena. The movement was strongly marked at Parma, and to a less degree at Pisa and Placentia. Reuter's correspondent at Spoleto reports that severe shocks were also felt there on Monday evening.

A general meeting of the Federated Institution of Mining Engineers will be held in London on Thursday, May 30, and on Friday, May 31. The presidential address will be given by Mr. W. N. Atkinson on the Thursday. The papers to be read on the same day are:—Notes on bauxite in County Antrim, &c., and its uses, by Mr. George G. Blackwell; sampling, by Mr. T. Clarkson; blasting explosives, by Prof. Vivian B. Lewes; and the gold-milling process at Pestarena, by Mr. A. G. Charleton. At the meeting on May 31 the following papers will be read, or taken as read:—Remarks on the banket formation of Johannesburg, Transvaal, by Mr. A. R. Sawyer; the composition of the extinctive atmospheres produced by various flames and by respiration, by Prof. Frank Clowes; the composition of the limiting explosive mixtures of various gases with air, by Prof. Frank Clowes; the mineral oils of Lower Elsass, by Dr. L. van Werveke; copper-mining in India, by Mr. Robert Oates; the recent magnetic survey of the United Kingdom, by Prof. A. W. Rücker; the MacArthur-Forrest process, by Mr. John McConnell.

In consequence of the renewed attacks upon the Conservators of Epping Forest, another large and influential meeting of the Essex Field Club was held on Saturday last, under the conductorship of Mr. Edward North Buxton, Prof. Boulger, Prof. Meldola, and the hon. secretaries. More than 100 members and visitors were present, among them being many residents in the district and lovers of the Forest, as well as such well-known experts as Prof. W. R. Fisher, of Cooper's Hill, and Mr. Angus D. Webster. The districts visited were those about which complaints had been made by a certain class of newspaper correspondents, viz. Bury Wood, the so-called Clay Ride, and Monk Wood. Beyond a few personal discussions between the conductors and experts and one or two of those who had been criticising the action of the Conservators, no public ventilation of views was permitted, as the conductors were of opinion that a mere inspection of the places named would enable the members and their friends to form their own conclusions. The party assembled at the King's Oak at High Beach for tea, after which an ordinary meeting of the Club was held, the President, Mr. David Howard, taking the chair. Mr. E. N. Buxton explained a scheme which he had been carrying out for the purpose of affording protection to the birds of the Forest district. By enlisting the sympathies and securing the co-operation of the surrounding landowners, he had succeeded in obtaining a promise that a total area of some 20,000 acres, including the 6000 acres of Forest, should constitute a sanctuary within which no rare or interesting birds should be destroyed. The President indicated that such an organisation as the Essex Field Club was well calculated to enforce by example and precept the desirability of protecting both animals and plants. Mr. F. C. Gould, in reply to those correspondents who had stated that the birds were becoming rarer in Epping Forest, said that this was quite contrary to the facts. Birds were never so plentiful in the Forest as they had been during the past few years, and Mr. Gould gave a list of species which had been observed by his son in the course of one day. After tea the party proceeded to the more northern part of the Forest, and inspected Epping Thicks. Although no formal division on the question of the management of the Forest was taken, the majority could not help expressing their admiration at the skill and judgment with which this year's thinnings had been effected. Many of those present also expressed some anxiety that the Conservators might be influenced by the newspaper correspondence, and allow the Forest to degenerate by acceding to the request recently made by a deputation to the Committee that no further thinning should be allowed for a period of five years.

A spell of very cold weather for the time of year was experienced last week over the entire area of the British Isles, owing to a depression which, at the time of our last issue, lay over Denmark, and caused strong gales from north and north-west over the North Sea. The temperature fell about 30° over the inland parts of England, while snow and hail were reported from many places. On several nights the sheltered thermometer fell to within a few degrees of the freezing-point, and actually reached it in the east and west of Scotland, on the morning of the 17th instant; while the highest day readings have in many parts failed to reach 50°, a temperature which is fully 10° below the average. During the first part of the present week a depression which had spread westwards from Germany, caused a continuation of cold, gloomy weather over our islands.

Some years ago the desirability of publishing the observations made by the late J. Allan Broun at Trevandrum, in Southern India, for over twelve years, was brought before the Royal Society of London by the Royal Society of Edinburgh, and the records were deposited at the Meteorological Office for safe keeping. The Meteorological Council subsequently drew the attention of the Royal Society to the subject, and that body induced the Indian authorities to render this valuable material accessible to scientific men, the result being that the Meteorological Department of India has just published the barometrical and thermometrical observations in vol. vii. of their *Memoirs*. The publication contains the hourly observations and means from January 1853 to December 1864, with the exception of Sundays, on which no observations were taken. The whole of the original entries have been carefully examined for clerical errors, under the superintendence of Mr. J. Eliot, the Government Meteorological Reporter, and we gather from the preface that a discussion of the results will eventually be carried out.

A most eloquent appeal for the wider diffusion of a knowledge of sanitary matters has been recently made by Dr. Carlo Ruata, Professor at the University of Perugia, in his introductory address to a course of lectures on the duties of sanitation. Efficient sanitation, urges Dr. Ruata, may justly be demanded as a right by the individual from the State; but, at the same time, each individual must be adequately impressed with his duties and responsibilities to other members of society in the proper conduct of sanitary matters. It is pointed out how much may be, and has been, done by judicious legislation and enlightened public opinion in recent years; but Dr. Ruata would insist upon more vigorous measures, and upon a knowledge of the principles of hygiene being rendered compulsory in systems of education. Ignorance and lack of all sense of responsibility is only too frequently to blame for the generation and spread of disease, and Dr. Ruata's appeal, that proper hygienic conduct should be insisted upon as the serious duty which one member of society owes to another is fully justified. Dr. Ruata is confident that with improved hygienic conditions society will benefit not only physically but morally; but whether it will bring about the utopian state sketched by the lecturer in his sanguine peroration, remains yet to be seen.

A report, by Mr. P. G. Craigie, on the agricultural experiment stations and agricultural colleges in the United States, just published as a Parliamentary Paper, should be seen by every one interested in agricultural education and research. It appears that, at the present day, upwards of three-score collegiate institutions are engaged in the United States wholly or partly in agricultural teaching, and, according to the statistics collected and published for 1892, they enjoy an aggregate revenue of £689,000, practically one half of which was granted by the Federal Government, while £223,000 is added by the several States, minor aid being rendered by £40,000 which came from fees, and by the benevolence of local committees or private individuals, while the remainder was raised by the sale of farm produce or miscellaneous receipts. The number of separate experiment stations is fifty-four, of which forty-eight receive subventions from the Federal Government out of national funds, the uniform grant being roughly £3000 to each station. According to the returns published of the revenue of these stations in 1892, upwards of a million dollars, or roughly £200,000, is available as annual revenue, the Federal Government finding £140,000, and the grants of the States reaching rather more than £30,000.

Mr. Craigie's report bears out his conclusion that "great and practical energy is being directed to the discovery of the best means of extending the field of agricultural and horticultural knowledge. It should not be overlooked that side by side with the growth of local stations a very extensive development of the scientific staff engaged on the special inquiries of the Federal Department at Washington has taken place in the last ten years. The American Government seems willing to face any cost to the community that promises the better to equip the farmer with a knowledge of his business. The authorities seem assured that in indicating methods of profitable production, and still more by the careful perfecting of the produce of the vast lands of the Republic, in whatever directions of extensive or of intensive culture the economic circumstances of the moment may prescribe, they are providing a solid means of advancing the well-being of the nation as a whole."

A fresh addition to periodical literature is the *Journal of the South-Eastern Agricultural College*, Wye, Kent, which is to be published three times a year, and is intended to be a brief record of the history of the college from term to term, and to announce the results of investigations and experiments conducted by the college or members of its staff, together with other observations that may seem of interest to the agriculture of the counties of Kent and Surrey. The first number contains a description, with a plan, of the farm attached to the college, together with an account of the dairy school, of the water supply of the college, and of the field experiments which are being instituted. Mr. F. V. Theobald's notes on poultry parasites would appear to open out an instructive field of inquiry. Mr. J. Percival gives an abstract of a paper, already published, relating to eelworms in hop plants, their ravages resulting in the condition of the plants known as "nettle-headed." The nematode *Heterodera Schachtii* attacks so many kinds of plants, that its presence in hops was quite to be looked for. No reference seems to be made to the value which hop-growers set upon rape as a "trap-plant" for enticing the eelworms away from the infested crop. If future numbers are as attractive as this one, the publication is likely to prove acceptable to those in whose interest it is issued.

In a recent number of the *Bulletin Geol. Soc. America*, Messrs. G. K. Gilbert and F. P. Gulliver give an interesting account of the remarkable "tepee buttes" that occur abundantly in the neighbourhood of Pueblo, Colorado. Using the term "butte" to denote steep-sided hills with narrow summits, which may be of very various origin, the authors mention the various types of buttes (volcanic necks, geyser deposits, &c.), and discuss this particular form. They are low hills, less than twenty feet in height, that owe their origin to the resistance to denudation of peculiar vertical masses of limestone occurring in the shales of the Pierre series (Upper Cretaceous). The limestone is composed of shells, chiefly of *Lucina* and *Inoceramus*, united by a matrix of shell-fragments, foraminifera and clay. This structure of the limestone, in comparison with that of the calcareous concretions that occur normally throughout the shale, negatives its concretionary origin, nor does it resemble the spring-deposited masses of limestone known elsewhere. It is concluded that particular local conditions determined the establishment of colonies of Mollusca that continued for generations at these spots, though what these conditions may have been it is not easy to explain. Attention is called to the description, by Dr. Bell, of similar limestone masses in Devonian shales in Canada.

The motion of a pianoforte wire when struck has been investigated by Herr W. Kaufmann, whose paper on the subject in *Wiedemann's Annalen* is accompanied by a set of very interesting photographic records, obtained by a modification of the method invented by Kaps and Krigar-Menzel. By vibrating the wire in front of a luminous slit, and throwing the image of it upon sensitive paper rotating upon a cylinder, a white line is traced upon a black ground. This line, which is due to the interruption of the luminous slit by the opaque wire, exhibits all the motions of the particular point in the wire which is crossed by the slit. In

order to bring the plane of the slit into exact coincidence with the wire, an image of the slit, produced by a lens with the aid of the electric arc, was thrown upon the wire itself. Since the hammer struck the wire at the point photographed, the motion of the wire was traced from the very first, the commencement of the vibration being the most interesting stage. Hard and soft hammers were tried, the latter corresponding to those actually used in the piano. It was found that the duration of contact is longer with feeble than with hard striking. As the force increases, the duration of contact rapidly approaches a limiting value equal to that of a hard hammer of equal weight. But the practically most important resultant was the proof that when a wire is struck at a point between one-seventh and one-ninth of its length, the fundamental tone has a maximum, and the harmonics—especially the third—are very feeble. Hence a wire thus struck gives its strongest and richest tone. This fact is acted upon by piano-builders, but is not explained by supposing that the nodes of the higher harmonics are struck, thus preventing their being heard. They are heard, but are outweighed by the more harmonious ones.

AN interesting paper on the magnetisation of iron in very weak fields, by W. Schmidt, appears in the current number of *Wiedemann's Annalen*. The author uses the magnetometer method slightly modified, a compensating coil being placed on the opposite side of the magnetometer to the magnetising coil. The effect of the iron under investigation on the magnetometer needle is compensated by passing a known current through an independent coil of large radius, so that the method is a "zero" one. A Duprez-d'Arsonval galvanometer was used to measure the current, its constant being determined by means of standard Clark cells. The samples of iron and steel under investigation had the form of ellipsoids, the semi-minor axis being 3 m.m. and the semi-major axis 200 m.m. for one set of experiments, and 150 m.m. for the other. The curves obtained for iron and steel show that for fields up to 0·06 C.G.S. units the susceptibility is constant, thus confirming Lord Rayleigh's results. As the magnetising field increases between 0·06 and 0·4 units, the curve giving the relation between the magnetising force and the susceptibility is a straight line. The author sums up the results of his experiments as follows:—Steel follows weak magnetising forces more quickly than iron. The susceptibility of soft steel is for small magnetising forces greater than that of iron. Thus for fields less than 0·06 C.G.S. unit the susceptibility of soft steel is to that of iron in the ratio of 4 to 3. For magnetising fields of about 1 unit the susceptibility of the two is about the same, while for greater field strengths the susceptibility of iron is greater than that of steel. The limits within which the susceptibility remains constant vary considerably for different samples, but the author considers that 0·06 C.G.S. unit may be taken as the upper limit with sufficient accuracy for most purposes.

THE May number of the *Irish Naturalist* well sustains the reputation for utility and general interest which has been obtained by this periodical. Mr. R. M. Barrington gives an interesting sketch of the career and writings of the late Mr. A. G. More, a naturalist of unusual versatility, who has contributed greatly to our knowledge of Irish Natural History. Mr. Robert Warren writes on the Breeding Birds of Loughs Conn, Carra, and Mask. Dr. R. Hanitsch gives a brief but valuable account of the Fresh-water Sponges of Ireland. The distribution of these forms presents certain features of peculiar interest. The eastern part of the island possesses only common European forms, whereas three out of the four species found along the west coast prove to be American. It is pointed out that the formation of gemmules gives to the Spongillidæ more favourable chances of dispersal than are enjoyed by most other animals. Mr. Clement Reid has examined a sample of marl from which skeletons of the Irish elk had been obtained, and finds that it consists largely of *Chara* and *Potamogeton*. He offers an ingenious suggestion to explain the occurrence of skeletons of *Cervu megoceros* in deposits of *Chara*-marl. Those familiar with pools containing *Chara* will be well aware of the appearance of shallowness, and of a solid floor, which is so deceptive. The plants grow to a depth of several feet, but appear to form a carpet of turf just below the surface of the pools; any animal treading on this turf would immediately plunge head-foremost into the water, and in the case of the elk the antlers would almost inevitably become entangled among the stems of *Chara* and other still tougher pondweeds. This entanglement theory accounts very well for the fact that the remains of stags are far more abundant than those of hinds.

A PHOTOGRAPH of the late Prof. J. D. Dana, taken about six weeks before his death, is reproduced in the current number of the *American Journal of Science*, together with a full biographical notice, and a list of his works.

THE "Year-Book of Scientific and Learned Societies" in Great Britain and Ireland, the twelfth annual issue of which has just been published by Messrs. C. Griffin and Co., is undoubtedly a very useful handbook of reference. A general idea of the progress of science during the past year can be obtained from the lists of papers subjoined to the designations of the various societies.

IN the Michigan Mining School theoretical knowledge seems to be well combined with practical training. We notice in the Calendar, just received, that the elements of astronomy is one of the subjects in which all students are examined for entrance into the School. The course of instruction is arranged so that a good foundation is given in the principles of science, and experience and practice are obtained in every subject taught.

THE new editions received during the past week include the first volume of the British Museum "Catalogue of Fishes," containing the Centrarchidæ, Percidæ, and Serranidæ (part), by Mr. G. A. Boulenger, F.R.S. When the first edition of the work was published, in 1859, the Museum collection of fishes comprised 29,275 specimens. The additions since that date have brought the collection up to twice its dimensions at the time when the original catalogue was compiled. The need for revision will, therefore, be fully understood. In the volume which begins the publication of the new edition of the catalogue, Mr. Boulenger confers a benefit upon ichthyologists by omitting seventy-six imperfectly or incorrectly characterised species. The result of this is that, though many new species are included, the present volume contains only fifty-eight more recognised species than the original one. Mr. Boulenger's list gives 375 species, of which 261 are now represented in the British Museum collection, by 2353 specimens.

NEW editions of two technical manuals have reached us from Messrs. Whittaker and Co. One of these is "Electricity in our Houses and Workshops," an admirable handbook by Mr. S. F. Walker, in which the every-day working of common forms of electrical apparatus is simply described. "The Practical Telephone Handbook," by Mr. Joseph Poole, which is now issued in an enlarged form, should prove of increased value to all interested in the methods of telephone working. A new and enlarged edition (the fourth) of Balfour Stewart's "Lessons in Elementary Physics" has been published by Messrs. Macmillan and Co. In this volume we have a treatise in which the whole domain of physics is covered, and which is so arranged that the connections between the various branches of the subject are clearly brought before the student. Though a quarter of a century old,

if the book is brought into line with modern physics from time to time, it will command success for many years to come. "Wild Nature won by Kindness" (Fisher Unwin), has attained the eminence of a sixth edition. Mrs. Brightwen's pleasantly written papers on natural history subjects are evidently appreciated by a large public. Three books by Prof. S. Cooke, of the College of Science, Poona, published by Messrs. George Bell and Sons, have also been received. They are "First Principles of Astronomy" (fifth edition), "First Principles of Chemistry" (sixth edition), and "Test Tables for Qualitative Analysis" (third edition.)

THE synthesis of caffeine is the subject of a short communication to the Berlin Academy by Emil Fischer and Lorenz Ach (*Sitzb. König. Preuss. Akad. Wiss. Berlin*, xiv. p. 261). By the condensation of dimethyl urea and malonic acid the substance $CO:(NMe.CO)_2:CH_2$, is obtained. The nitroso-derivative, $CO:(NMe.CO)_2:CH.NO$, is reduced to dimethyluramil, $CO:(NMe.CO)_2:CH.NH_2$, whence dimethylpseudouric acid, $CO:(NMe.CO)_2:CH.NH.CO.NH_2$, is produced. By abstraction of the elements of water with oxalic acid, dimethyluric acid is formed. This substance is converted into theophylline, an isomeride of theobromine, of the formula

$$CO\begin{array}{l}\diagup NMe.CH:C.NH\diagdown \\ \diagdown NMe\ .\ C:N\diagup\end{array}CO.$$

Thence caffeine,

$$CO\begin{array}{l}\diagup NMe.CH:C.NMe\diagdown \\ \diagdown NMe\ .\ C:N\diagup\end{array}CO,$$

is obtained by the ordinary methyl iodide reaction. As this is the first synthesis of caffeine, details of the methods used will be looked forward to with considerable interest.

THE observation by Martin Freund and Ernst Göbel, that thebaine is a derivative of phenanthrene (*Ber.* 28, 7, 941) brings this alkaloid into line with morphine and codeine as instances of the few natural phenanthrene derivatives yet known. Thebaine is related to dihydrophenanthrene in the same manner as morphine and codeine are connected with tetrahydrophenanthrene.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii*, ♀) from Natal, presented by Mr. Alfred James; a Common Jackal (*Canis aureus*), two Bengal Foxes (*Canis bengalensis*), a Jungle Cat (*Felis chaus*) from India, presented by Dr. John Anderson, F.R.S.; two Short-tailed Capromys (*Capromys brachyurus*) from Jamaica, presented by Mr. Frank Cundall; a Dorsal Squirrel (*Sciurus hypopyrrhus*) from Central America, presented by Mrs. Brett; five Squirrel-like Phalangers (*Belideus sciureus*) from Australia, presented by the Right Hon. Earl Cadogan, K.G., a Cambayan Turtle Dove (*Turtur senegalensis*) from West Africa, presented by Mr. C. L. Sutherland; a Salle's Amazon (*Chrysotis sallæi*) from St. Domingo, presented by Mr. W. Windsor Spriggs; a Spotted Salamander (*Salamandra maculosa*), European, presented by Mr. E. Layton Bennett; two Great Wallaroos (*Macropus robustus*, ♂ ♀), two Agile Wallabies (*Halmaturus agilis*) from Australia, a Blue and Yellow Macaw (*Ara ararauna*) from South America, deposited; two Canada Geese (*Bernicla canadensis*) from North America, two Yellowish Weaver Birds (*Sitagra luteola*) from Tropical Regions, thirteen Green Lizards (*Lacerta viridis*) from Jersey, purchased; a Malaccan Parrakeet (*Palæornis malaccensis*) from Malacca, a Nicobar Pigeon (*Calænas nicobarica*) from the Indian Archipelago, received in exchange; a Bennett's Wallaby (*Halmaturus bennetti*, ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

STARS WITH REMARKABLE SPECTRA.—At those stages of celestial evolution in which the temperature is low, it is probable that the average condensing body will not be very bright, so that the study of their spectra at these phases of their development presents some difficulties. Thus, the great majority of the stars with bright-line spectra, and stars showing intense carbon absorption, are of low magnitudes, and because comparatively few were identified in the earlier surveys of stellar spectra, they were looked upon as "peculiar." Nevertheless, a study of these spectra in relation to those of the brighter stars indicated that they probably represented stages in the history of all condensing bodies, so that their discovery in greater numbers was only to be expected. At Harvard College especially has the photographic investigation of these faint spectra been carried on, and the discovery of eleven more objects with peculiar spectra has been recently announced by Mrs. Fleming; two of these are simply stated to be "peculiar," three are nebulæ with bright lines, two have the F line of hydrogen bright, two appear to be bright-line stars of the Wolf-Rayet type, and two show carbon absorption. In addition, the spectra of eleven stars of the α Herculis type were found to show the F line of hydrogen bright, and this unfailing sign of variability in this group of stars is fully substantiated by an examination of chart plates of the same regions. (*Astrophysical Journal*, May.) In his observations of the visual spectra of faint stars, Rev. T. E. Espin has discovered numerous objects which he describes as "remarkable." (*Ast. Nach.* 3286.) Most of these, however, appear to be of the α Herculis type; but some are spectra in which carbon absorption is predominant (Group VI.). The most interesting observations by Mr. Espin are those of variable stars, and they tend to show that, in the stars of Group VI., the variability is accompanied by spectroscopic variations which render it difficult to distinguish them from stars of Group II., in which the carbon flutings are bright.

THE PARIS OBSERVATORY.—The annual report for 1894 indicates a high state of activity in this institution, in many directions besides those with which occasional published papers have made us familiar. Great progress has been made with the star catalogue initiated by Admiral Mouchez in 1882, comprising 350,000 observations made between 1837 and 1881: two additional volumes will be published during the present year, and the last two in 1899; a supplementary couple of volumes, dealing with observations made since 1881, will also be issued very shortly. The number of meridian observations during last year amounted to over 18,000, while observations of sun, moon, and planets total 455. Comets, minor planets, and double stars have also received a vast amount of attention. For the great photographic chart, 278 negatives were taken during the year, and the positions of nearly 33,000 stars on various plates have been measured under the direction of Mdlle. Klumpek.

The reduction of these measures was seriously commenced in November, and up to the end of the year the measures of 11 plates, showing 1760 stars, were completely reduced. Meteorological observations on the usual plan were continued regularly.

The magnificent work on lunar photography with the equatorial coudé, as well as the spectroscopic researches of M. Deslandres, have already been referred to in our columns.

THE ACTION OF LIGHT ON ANIMAL LIFE.

ALTHOUGH a number of investigations have been made on the action of light on bacteria, very few experiments have been carried out to ascertain how direct insolation affects animals inoculated with particular disease microbes. Does exposure to sunshine increase or diminish an animal's susceptibility to disease? De Renzi was, we believe, the first to study this question experimentally, and he endeavoured to answer it as regards tuberculosis by inoculating guinea-pigs with tuberculous material. Some of the animals he kept in glass boxes exposed to the direct rays of the sun for five or six hours daily, whilst others were placed in the sunshine, but instead of glass, wooden boxes were used. De Renzi found that, whilst the guinea-pigs in glass boxes—to which, therefore, the maximum amount of sunshine had access—died after 24, 39, 52, and 89 days, those in the opaque wooden boxes died after 20, 25, 26, and 41 days. Thus it would appear that sunshine materially assisted these animals in combating with tuberculous disease, for those individuals deprived of sunshine succumbed far more rapidly.

More recently, Dr. Masella has carried out a series of similar experiments with guinea-pigs inoculated, however, with cholera

and typhoid bacilli respectively. Various points were investigated as to whether insolation *previous* to inoculation increased the animal's susceptibility to these diseases, also what was the effect of insolation on the animal after infection, and whether the same results were obtained when the temperature of the surrounding air during insolation was not permitted to rise. The toxic properties of the cholera and typhoid broth cultures employed were carefully tested, and it was ascertained that the lethal dose in the case of cholera, procuring death in twenty-four hours, was secured by employing cultures in the proportion of 0·20 per cent of the weight of the animal operated upon, whilst to obtain similar results with typhoid cultures, 0·40 per cent. of the weight of the animal was the proportion in which they had to be used.

In the case of both cholera and typhoid it was found that previous exposure to sunshine increased the animals' susceptibility to these diseases, for not only did they die more rapidly when subsequently inoculated with these cultures than the guinea-pigs similarly treated, exposed, however, only to diffused light, but they succumbed to smaller doses, and doses which did not prove fatal to the guinea-pigs which had been previously protected from sunshine. When the exposure to sunshine took place *after* infection fatal results were greatly accelerated, for instead of dying in from 15 to 24 hours they succumbed in from 3 to 5 hours. These experiments were, however, open to the objection that the accelerated lethal action through subsequent insolation might be due to the higher temperature which necessarily prevailed in boxes exposed to sunshine over those to which diffused light only was admitted. To dispose of this difficulty, boxes were constructed with double cases through which a current of water was kept circulating; in the "sunshine" boxes, as before, only glass was used, whilst in the "diffused light" boxes the outer case was made of zinc. In spite, however, of these precautions as regards temperature the results confirmed those previously obtained, the insolated animals still exhibiting the same increased susceptibility to infection from these diseases over the non-isolated animals.

Dr. Masella does not attempt to give any explanation of the remarkable results he has obtained, but we would suggest that the action of sunshine should be tried on anti-toxines. It would be of great interest to ascertain how the potency of these protective fluids outside the body was affected by exposure to sunshine, and also what result, if any, isolation had on their generation within the animal system.

We know that the toxic properties of, for example, tetanus cultures may be entirely destroyed in from 15 to 18 hours in direct sunshine at a temperature of from 35° to 43° C., and Roux and Yersin state that five hours' direct insolation greatly modifies the toxic properties of diphtheria cultures; again, Calmette has found that after two weeks' insolation the poison of the *Naja tripudians* is completely destroyed, whilst a similar exposure has a damaging effect on the poison of the rattlesnake. So far as we are aware, the action of sunshine on the immunising properties of serum has not been investigated, and its study should prove of immense interest and importance.

The results obtained by De Renzi with tuberculous infection have a practical confirmation in the acknowledged benefit which patients suffering from tuberculosis derive from residence in places such as Davos, where the maximum amount of sunshine may be secured. On the other hand, Dr. Masella's experiments leave us with an uncomfortable uncertainty as to the wisdom of basking in the sunshine. He would have us believe that his investigations explain the greater prevalence and virulence of typhoid and cholera (which he states as an accepted fact) in hot countries where the sun shines with greater power and more continuously. After all, our smoke-laden atmosphere and dreary yellow fogs may be turned to account seemingly, and the London water companies may congratulate themselves that these two water-borne diseases, *par excellence*, may be made to yield not only to efficient purifying processes at their hands, but that such an unexpected ally, according to Dr. Masella, is to be found in the limited amount of sunshine which Londoners can enjoy!

G. C. FRANKLAND.

THE CONSTRUCTION OF STANDARD THERMOMETERS.

A SERIES of important articles on the preparation and testing of standard thermometers have been communicated to the *Zeitschrift für Instrumentenkunde* by Drs. Pernet, Jaeger, and Gumlich, of the Physikalisch-Technische Reichsanstalt. The selection of the best glass, the calibration of the thermometers, the determination of the coefficients of external and internal pressure, and the verification of the principal points are fully dealt with. One source of error in thermometers as usually constructed, lies in the fact of the bulbs being blown from the tubes. The vaporisation of certain constituents of the glass during this operation leads to a difference of chemical constitution between the stem and the bulb. This may be obviated by making the bulbs out of thin walled tubes of the same kind of glass, and welding them on to the stems. As regards the depression of the freezing point, it was found by Wiebe and Schott, of Jena, that glasses containing either sodium or potassium, but not both, showed this after-effect to the least extent. In order to render the reading of temperatures accurate to within 0°·002, the length of a degree should not be less than 6 mm., and since the length of the stem cannot conveniently exceed 60 cm., the range of measurable temperature is practically limited to 100°. Stem thermometers without enamel backs or enclosing tubes were the only ones found suitable for first-class standards. When certain fixed points outside the scale were to be brought in, this was accomplished by widening out the tube above them. An equal linear division of the scale was adopted, this having great advantages over the more or less untrustworthy division by equal volumes. For calibration, threads of mercury of different lengths were cut off from the main portion and measured with micrometer microscopes, viewing them both through the face and the back of the stem. But the threads were not cut off by local heating, since that is apt to produce a permanent change of capacity. The small and almost microscopic bubble which remains in every thermometer was made use of. It was brought to the entrance of the bulb when the desired portion of the thread had been driven into the stem, and then a slight jerk sufficed to cut off the required length. To facilitate this operation, the bulb was narrowed to a neck at the entrance to the stem. As regards pressure, two factors had to be considered. The external atmospheric pressure, and the pressure of the liquid in which it is immersed, tend to compress the glass vessel and to produce an apparent elevation of temperature. The capillary pressure of the mercury, and its hydrostatic pressure, on the other hand, tend to widen the bulb and produce an apparent cooling. The first of these elements was investigated by exposing the thermometer to various high and low pressures in a glycerine bath, and the second by observing the readings when the thermometer stood horizontally and vertically respectively, at its highest measurable temperature. The capillary pressure was found to be too capricious to be accurately measured, but it is a negligible quantity. The coefficient of apparent expansion of mercury in the new Jena glass thermometer 16^{III} was found to be 0·0001571 between 0° and 100°.

THE INFLUENCE OF MAGNETIC FIELDS UPON ELECTRICAL RESISTANCE.

IT is well known that the resistance (R) of a wire of bismuth, as measured with a constant current, increases under the influence of a magnetic field, and that this increase depends on the strength of the field and its direction with reference to the current in the wire. If the current traversing the bismuth is oscillatory, the resistance has a value O outside the magnetic field, or in a field in which the lines of force are parallel to the wire which is less than R. If, however, the wire is perpendicular to the lines of force of a field greater than 6000 C.G.S. units, the resistance O is greater than R; the difference O - R increases from this point pretty rapidly as the strength of the field increases. These changes are not due to alterations in the self-inductor, since they are independent of the form of the bismuth spiral. This curious phenomenon has lately been examined by M. I. Sadovsky (*Journal de la Société Physico-Chemique de Russe*, xxvi. 1894, and *Journal de Physique*, April 1895), who sums up the results of his experiments as follows: (1) The difference in the resistance of bismuth observed with constant or alternating currents is measurable outside a magnetic field with 300 alternations per second, and can be detected in magnetic fields with only three or four alternations per second; (2) this difference depends on the number of oscillations per second, and without the magnetic field increases with the increase in the frequency of the alternations; (3) the resistance which bismuth, in a strong magnetic field, offers to an increasing current is greater, and that to a decreasing current less than the resistance for steady currents. The difference between the resistances to an increasing and decreasing

current increases with the rate of change in the strength of the current $\left(\frac{dC}{dt}\right)$, and this difference is more marked with strong currents than with weak. Thus M. Sadovsky has discovered the remarkable fact that for variable electric currents the resistance of bismuth changes with any change in $\frac{1}{C}$ or $\frac{dC}{dt}$ where C is the current. The author mentions that the effects observed cannot be due to self-induction, or they would occur when the bismuth is not in a magnetic field. In a note on the above paper in the *Journal de Physique*, M. Sagnac considers what would happen if the same series of experiments were repeated with an iron wire. A straight cylindrical iron wire becomes, when traversed by a current C, circularly magnetised; the energy due to this magnetisation being, according to Kirchhoff, $\pi\kappa l C^2$, where κ is the susceptibility and l the length of the wire. This energy may possibly increase the coefficient of self-induction by $2\pi\kappa l$. From Klemenčič's data the order of the change in the apparent resistance can be calculated. For weak magnetic fields in which κ has a large value, the difference between the value of the apparent resistance for steady currents and for increasing currents may amount to several hundredths of the value of the resistance for steady currents.

TONBRIDGE SCHOOL LABORATORIES.

I HAVE often been asked to give some account of the laboratories at Tonbridge School: and as they represent some ten years of pleasant labour on my own part, and a considerable expenditure, joined with much sympathy and help from the Governors of the School (the Company of Skinners), I feel it a privilege to do so.

It is difficult to render the subject interesting to those who are not concerned in teaching, although as an instance of an ancient foundation lending itself to the most modern of claims, it may appeal to a wider circle. I must ask to be excused from entering upon any treatment of the well-worn subject, scientific education. I am not quite sure that it is any business of mine. In course of time, no doubt, a condition of stable balance will be reached, as regards the relative weight and value of the various school subjects. Those who are in the thick of the fight cannot always tell which side is winning.

So far we have little at Tonbridge beyond the training-ground itself, consisting of laboratories and workshops, which may be mentioned in sequence as follows:—

Wood Workshops.
Metal Workshops.
Mechanical Laboratory.
Physical Laboratories.
Chemical Laboratories.
Engine-rooms with electric light plant.
Biological Laboratory and Museum.

A description of these in detail is given herewith.

Wood Workshops.—These shops are well lighted and airy, occupying a ground space of 48 feet by 30 feet. Work-benches to the number of sixteen, with appropriate fittings, allow about sixty boys to work at the same time. A skilled carpenter is always in attendance for teaching his craft, and a course of graduated tasks are exacted before a pupil is allowed to construct the shelves, boxes, coal-boxes, tables, and other articles which form the staple produce of school shops.

Metal Workshops.—The wood workshops lead on to the metal shops, in use as well as in fact. They are under the care of a practical instrument-maker, and the physical laboratory owes much to his skill. It may be mentioned here that no physical laboratory can be considered complete unless it is in connection with suitable workshops wherein instruments may be constructed and repaired. These shops are devised to accommodate about twenty boys working together. They are fitted with all the necessary appliances, including planing and drilling machines and six lathes (from 4 in. centre up to 7 in.). The ground space devoted to metal work is 40 feet by 20 feet. After a course of woodwork, boys are taught to make their own tools, forging and tempering them themselves, to use the file properly, to turn, and afterwards to construct such instruments as they may fancy, it being always required that a working drawing should be made beforehand. The favourite occupation is the construction of electric bells, small dynamos, microscopes, and levels.

Mechanical Laboratory.—This room, which measures 40 feet by 21 feet, is fitted for those important lessons in accuracy of observation to which I give the name of Elementary Physical Measurements, *i.e.* the measurements of length, mass, and time, and for Practical Mechanics, *i.e.* the simpler measurements of forces and the conditions of equilibrium, the measurement of gravitation, and observations of the general properties of matter and the behaviour of matter under stress. All the work-tables are movable, and the walls are fitted with brackets and boards for the support of models and apparatus.

Physical Laboratory.—This laboratory opens out from the Mechanical Laboratory, and like it is well-lit and lofty. It is 42 feet long and 30 feet broad. The centre of the room is fitted with five solid benches attached to the floor and provided with gas. These benches are arranged to enable elementary classes to work together at the same experiment. With this object, drawers in the benches are stocked with a large quantity of apparatus which enables a class of twenty-four boys to work together through a long series of experiments in practical physics. Each experiment has to be represented by at least twelve sets of apparatus for this purpose, and some years have been occupied in organising this branch of work. The work-benches along the walls of the room lend themselves to the more advanced work in practical physics. It is needless to say that here the apparatus is not twelve-fold. Beyond the physical laboratory is the science master's private room, which has a tendency to shape itself as an advanced physical laboratory.

Chemical Laboratory.—This is a fine room, with both skylight and side windows. It is 45 feet long, 30 feet broad, and 30 feet high. Eight benches are fixed, two abreast, across the room, allowing the greatest possible freedom of movement. The benches are arranged to admit forty-eight students working together. They are fitted with shelves for reagents, fixed across the bench, and not lengthways, whereby reaching over one's work is avoided, and also a more complete view and control of the whole room is possible for the master. Each student is provided with a most efficient draught-box, serving also as a support for the vessels he is using. This arrangement keeps the laboratory thoroughly free from fumes, in spite of all well-meant efforts to the contrary on the part of pupils. The shelves and draught-boxes are removable from the benches, so that a clear space can be obtained when required for setting up apparatus on an extensive scale. The wall space is occupied by shelves for reagents, and by lead troughs for washing-up purposes. By this arrangement of confining the water-supply to the walls of the room, most of the ordinary splashing and untidiness of laboratories is avoided. The transverse arrangement of the benches reduces to a minimum the walking about occasioned by this plan. The cupboards and drawers of these benches recede, so that it is possible to sit close up to one's work. A balance-room, 30 by 15 feet, leads out from the laboratory, and beyond this is a large theatre or lecture-room capable of seating about 150 boys. The balance-room is provided with chemical balances and books of reference. The lecture-room has a suitably furnished lecture-table, blackboards, screen for lantern, and cases of minerals and chemical specimens.

Engine and Electric Light Rooms.—The electric light, being used for the main portion of the school, puts the Science Department in possession of valuable plant. A gas-engine of 12 indicated horse-power, and a reserve steam-engine of 6 indicated horse-power, fitted with a Crosby indicator, together with dynamos and accumulators, give plenty of opportunity for gaining a practical knowledge of electric engineering. In addition to this, the current obtained is most useful in providing means for practical work and testing in the physical laboratory. The electric light is also used with the mirror galvanometer, to the great advantage of cleanliness and convenience.

Biological Laboratory and Museum.—It is appropriate that the description of this laboratory should come last. It is one of the most recent additions to the school, and it should undoubtedly be the last laboratory for the schoolboy to enter. Biology, unless it is approached through a training in physics and chemistry, is not to be considered as a suitable subject for preparatory education. The roots of biological sciences must always be in physical and chemical ground.

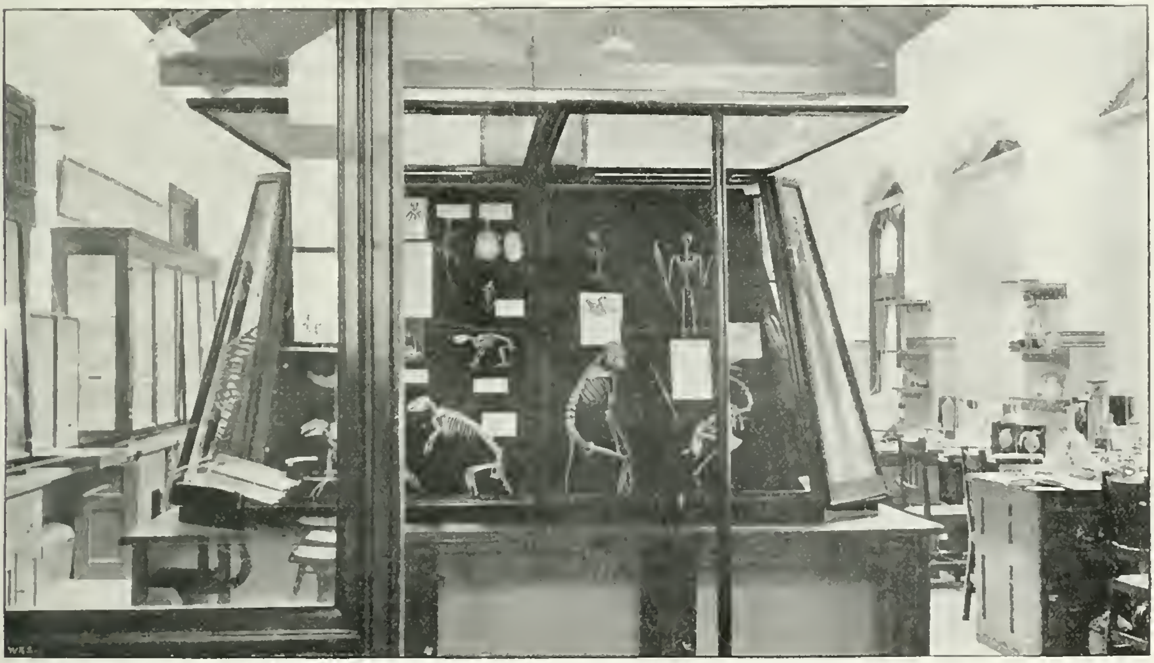
The room devoted to this work is carefully planned to ensure the most perfect light. The work-benches face windows which come down to the level of the benches, and in the roof is fixed a good skylight. The work-benches are formed of plate glass, gently sloping at the back into a white glazed gutter running into large white-ware troughs or sinks. Water-supply is at the hand of each worker, and the benches can be kept continually flushed and clean. Standing away from the work bench is the small writing-table and cupboard, &c., of each student. The arrange-

ments are peculiar, but—I may be allowed to say—most successful. The greater part of the room, which is 40 feet long by 21 feet wide, is occupied by cases which contain preparations and specimens under the headings of (*a*) Form and Locomotion, (*b*) Alimentation, (*c*) Circulation and Respiration, (*d*) Nervous System and Sense Organs, and lastly, objects displaying the main lines of classification. In fact, a collection modelled, both as to cases and modes of display, on the same lines as the admirable Introductory Collection of Sir William Flower at the Natural History Museum. It is, of course, on a miniature scale, and it is not yet complete. Beyond the main laboratory is a smaller room temporarily occupied for another purpose.

It now remains for me to add some attempt at a description of the general appearance of these laboratories. In the main, one may say, there is an air of dignity about the lofty and well-proportioned rooms, with their substantial and costly fittings. The woodwork is pitch-pine topped everywhere by thick teak. In the biological museums the cases are of mahogany, and perfectly constructed. Most of the teak tops of benches and tables are thinly coated with paraffin as a preservative. It is still important that rooms devoted to scientific work in schools should be exceptionally neat and bright in appearance. Indeed, it may even be said that appearances are at present more important than anything else as regards the value attached to the subject. Manners must grow to match the clothes. We have to bear in mind that we labour in the cause not of science alone, but of science as an instrument of school training. The laboratories are all *en suite*, whereby control is more easy, and a feeling of organic unity gained. Moreover, the workshops are within touch of the laboratories, as is also the large drawing-school.

Finally, I may mention that all the water and waste system has been laid down in direct contravention of all the best traditions of plumbing, with the happy result that we never need the services of a plumber for repairing.

ALFRED EARL.



A Case of Specimens in Biological Laboratory, illustrating Form and Locomotion.

THE DEVELOPMENT OF THE EXPERIMENTAL STUDY OF HEAT ENGINES.[1]

IT was Mr. Forrest's intention that the annual lecture bearing his name should illustrate the dependence of the engineer in his practical professional work on the mathematical and physical sciences. It therefore naturally takes the form of a review of some branch of engineering with special reference to the scientific principles which have been factors in its advancement.

Twenty years ago, Lord Armstrong stated that of all the coal raised in this country about one-third was used for household purposes, one-third for generating steam, and one-third for iron-making and manufacturing processes. He remarked that in the two first divisions the waste of fuel was shameful, and that without carrying economy to extreme limits, all the effects now realised from the use of coal could be obtained by an expenditure of half the quantity. The improvement of the steam engine is mainly due to an incessant attempt to diminish the waste of fuel.

Tests of Steam Engines in Cornwall.

Steam engineers have been face to face with the problem of economy for more than a century. Coal was excessively dear in Cornwall, and as the mines were deepened and more power was required, the cost of working increased ruinously. By reducing fuel cost, Watt saved the mining industry from extinction, and he adopted the plan of taking in payment for his engines a sum reckoned equivalent to one-third of the fuel saved. By agreement with the miners, tests were made, and the standard duty of a Newcomen engine was fixed at 7,037,000 foot lbs. per bushel. Regular duty determinations were made for all Watt's engines. Generally they gave a duty of 20,000,000. When Watt's connection with Cornwall ceased in 1800; the duty determinations were neglected, and the engines deteriorated.

Then Captain Joel Lean, who had reorganised the machinery at one of the mines, and effected great economies, started again the system of duty trials. He and his sons carried on the work for many years, and published reports of the results of the trials. Of these reports Dr. Pole says: "The publication produced an extraordinary effect in improving the duty of the engines. Engineers were stimulated to emulation amongst themselves. The practice of reporting is thought to have been attended with more benefit to the county than any other single event excepting only the invention of the steam engine itself."

I shall show later that the creation of a new and more scientific system of testing by Hirn and his colleagues in Alsace, in 1855, was the starting-point of a similar process of improvement. Quite lately there has been a revival of careful and independent engine testing and of the publication of the results, and records have been established which would have been thought impossible ten years ago.

The peculiar character of the load against which the Cornish

[1] Abstract of the "James Forrest" Lecture, delivered at the Institution of Civil Engineers, May 2, by Prof. W. C. Unwin, F.R.S.

engine worked, the lifting of a heavy mass of pump-rods, contributed to force the use of expansive working. To work without shock, a large initial and gradually diminishing effort was necessary. The extraordinary economy obtained was due probably in part to the special action of the steam, the Cornish engine being virtually a compound engine, and the admission surface being protected from cooling to the condenser: partly to the great effectiveness of a steam-jacket in an engine which worked slowly and with pauses at the end of the stroke, partly to the small clearance and separate admission and exhaust valves. The lesson engineers should have learned from Cornish experience was that in restricted conditions high ratios of expansion were economical. In this case, as in others, later engineers generalised too crudely, and concluded that expansive working was always economical. A new scientific investigation was required to correct the error.

Experiments on Boilers.

To generate steam power economically the boiler must be efficient, and the engine must be efficient, and the conditions of efficiency of boiler and engine are completely independent. Hence the early method of Watt, in which attention was paid only to fuel used and water pumped has been replaced by a method of independent boiler and engine testing. The boiler uses coal and generates steam; the engine uses steam and generates power. The economy of the boiler is reckoned on the fuel; that of the engine on the steam.

Different coals, at any rate the better kinds of coal, do not differ much in absolute calorific value. Used in boiler furnaces, they differ more, partly from differences of mechanical aggregation, but chiefly because the more bituminous coals require a larger air supply for tolerably smokeless combustion than those which consist chiefly of fixed carbon. The greater excess of air involves greater chimney waste. It is to test the commercial calorific value that Prof. Schroter has established in Munich a heat laboratory where fuels can be tested on a large scale and under ordinary practical conditions of combustion. The arrangements permit the determination of the exact conditions most suitable for each fuel.

An enormous number of boiler trials have been carried out, but most of them are mere individual tests of very little scientific value. Engineers have been too much under the impression that the evaporation depended chiefly on the type or proportions of the boiler, or the arrangement of the heating surface. But there are no obscure or complicated actions concerned in generating steam. Boilers of all types give nearly the same results, provided only proper conditions of combustion are secured. They may differ in cost, in durability, in convenience, but in efficiency they differ less than I think is commonly assumed. The following table shows that boilers of extremely different types, with very different proportions of heating surface and very different rates of combustion, and even with different coals, have all reached evaporations of from 11 to 13 lbs. of water from and at 212° per pound of coal:

Boiler Trials.

Type.	Ratio of grate to heating surface.	Coal per sq. ft. of grate per hour.	Evaporation from and at 212° per lb. of coal.	Coal.
Cornish		7·2	11·0	Welsh
Lancashire	1 : 30	22·9	11·2	Lancashire
Galloway	1 : 24	8·5	11·6	Anthracite
[illegible]	1 : 60	12·8	11·8	Welsh
[illegible]	1 : 46	10·8	11·0	Anthracite
[illegible]	1 : 38	8·9	11·8	..
[illegible]	1 : 34	22·4	12·0	Welsh
..	1 : 50	25·5	12·5	Lancashire
[illegible]	1 : 70	7·7	13·4	Welsh
..	1 : 61	18·6	12·5	..

Mulhouse Trial of 1859.

The earliest boiler trials carried out in a completely satisfactory way were those made by the Société Industrielle of Mulhouse in 1859. The Society offered a prize to the maker of any boiler which would evaporate 1800 lbs. per hour, at 75 lbs. per square inch pressure, and which would evaporate 9·1 lbs. of water, from and at 212°, per pound of Alsatian coal of not very good quality. With the coal used in these trials, 130 cubic feet of air per pound of coal are chemically necessary for complete combustion. It was found that the reduction of the air supply almost to this limit, and to a point at which there was definitely incomplete combustion, reduced the chimney waste and increased the efficiency of the boiler. In two special trials, each of a week's duration, the evaporation was 9 lbs. with 331 cubic feet of air, per pound, and 9·53 or 6 per cent. more with 247 cubic feet.

The determination of the air supply to a boiler is not altogether an easy operation. An anemometer was used in Alsace, and in suitable conditions it will give approximately accurate results. In recent trials chemical analyses of samples of the furnace gases have been made, and the amount of air supplied calculated from the percentage of CO_2. This method is accurate in principle, but the samples analysed are a very minute fraction of the total chimney discharge, and the samples may not be average samples. In some trials in which this method has been used, there are discrepancies in the ratio of the chimney loss and the undetermined loss, chiefly due to radiation, difficult to understand. Neither anemometer nor chemical analysis is suited to serve as a means of regulating the air supply in the ordinary working of a boiler. What is wanted is an instrument as easily read as a pressure gauge, and giving continuous indications.

The Dasymeter.

The dasymeter, invented by Messrs. Siegert and Durr, of Munich, is a fine balance in an enclosed case through which a current of the furnace gases is drawn. At one end of the balance is a glass globe of large displacement, at the other a brass weight. Any change of density of the medium in the chamber disturbs the balance. A finger on the balance moving over a graduated scale gives the amount of the alteration of density. An air injector draws the furnace gas from the flues, and it is filtered before entering the balance case. An ingenious mercurial compensator counterbalances any effect due to change of temperature or barometric pressure.

The dasymeter is usually combined with a draught gauge, and an air thermometer or pyrometer in the flue is required if the amount of waste heat is to be calculated. The dasymeter requires, initially, exceedingly delicate adjustment, and its indications must be checked from time to time by a Bunte's burette. It is set to read zero with pure air, and then any increase of density due to CO_2 is read as a percentage on the scale. When in adjustment, it is as easy to read the percentage of CO_2 in the furnace gases as to read the pressure on a pressure gauge. When the dasymeter is fitted to a boiler, the stoker has directions to adjust the supply of air so that the furnaces gases have about 12 per cent. of CO_2. With practice he learns what alterations of the damper or fire-door, or thickness of fuel on the grate, are necessary, or whether an alteration of grate area is desirable. After a little time the percentage of CO_2 can be kept very constant.

Isherwood's Experiments on Marine Engines.

About the year 1860, Mr. Isherwood, Chief Engineer of the United States Navy, began a series of systematic tests of engines and boilers on a very large scale, and with resources only available in a Government establishment. The trials were made with skill and determination, and the substantial accuracy of the results, startling as they were, has never been questioned.

All Isherwood's trials of large marine engines showed that when expansion was extended beyond exceedingly small limits, it caused not an economy, but a waste. In his second volume he sums up his results as proving that when cut-off is earlier than 0·6, or perhaps even 0·7 of the stroke, the consumption of steam reckoned on the work done is increased. Curiously enough, this led him to attack the compound engine. From the quantities in the table of experiments, he says, "it will be seen how useless in point of economic gain is the preposterous arrangement of steam engine known as the double cylinder, Woolf, or Hornblower engine. . . . Opposed to these facts, the declarations of interested patentees and engine builders must be classed in value with those set forth by quacks in advertisements of their nostrums." This is from a paper dated 1865, and it is curious, because Isherwood generally saw clearly enough the danger of drawing sweeping conclusions from narrow experimental premises.

The proper lesson from Isherwood's results was merely that

certain conditions must be observed to secure economy in expansive working. Unfortunately, more generally the conclusion was drawn that the Cornish results were not to be trusted, and that expansion was not economical, and Isherwood's own language lent authority to the least accurate view of his results. To obtain greater insight into the true action in the cylinder, and to find a reconciliation of the Cornish and American tests, experiments of a much more refined character were wanted and insight due to wider scientific knowledge.

The Physical Properties of Steam.—Regnault.

No useful progress could be made with a theory of the steam engine, no accurate reduction even could be made of the results of engine tests without exact determinations of the relations of pressure, temperature, volume, latent heat and liquid heat of steam. It was fortunate, therefore, that about 1840 M. Regnault obtained the means from the French Government to make a series of researches on the physical properties of steam with splendid instrumental appliances. He wisely carried out his determinations over a very wide range of conditions, and spared no labour or trouble in attaining accuracy. Regnault's results were of the greatest importance as a foundation for accurate study of the steam engine.

The Foundation of Thermodynamics. Carnot and Joule.

The next important step was the discovery of the equivalence of heat and work. Joule's investigations began with an attempt to improve Sturgeon's magnetic engine. He was so led to consider motive power problems from the engineer's standpoint, as a question of duty, or of something obtained for something expended. He ascertained the amount of electric current produced by the chemical combustion of a given amount of zinc, and comparing his results with those obtained in good steam engines, he concluded that, making the largest allowance for possible imperfections of his magneto engine, it was never likely to be a rival in economy to the steam engine. That was a negative but a useful result. It closed one direction of useless endeavour only too likely to attract the inventor.

One of the effects of electric action which Joule noticed was the heating of his conductors, and it was to the measurement of this heating effect he next addressed himself. The heat developed in the conductor by the electric action due to elements combining in the galvanic cell was found to be identical with that which would be generated by the direct combustion of the same elements. Finally, he came to consider the relation between the mechanical work expended in driving a magneto electric machine, and the heat developed in the external circuit of the machine. He concluded that for 838 foot lbs. expended a pound degree of heat was generated. Later experiments corrected this value, but the discovery of the equivalence of heat and work was made.

As early as 1824, twenty years before Joule's discovery, Sadi Carnot, in a remarkable pamphlet on the "Motive Power of Heat," demonstrated the fundamental principle that the amount of work obtainable from any given quantity of heat cannot exceed a quantity proportional to the fall of temperature. Unfortunately adopting, though with hesitation, the view held in his time that heat is material and indestructible as heat, he coupled with his true principle the false corollary that all the heat entering an engine is discharged in the condenser. Joule, in 1845, found this principle of Carnot, and looking to the corollary as essential, supposed the principle itself to be false. He failed to perceive that Carnot's principle was the essential supplement to his own discovery, and that it showed why the apparent efficiency of the steam engine is so low. It took six years before Joule's and Carnot's principles were reconciled, and for three of them even Lord Kelvin refused to accept Joule's discovery, because it apparently conflicted with the principle of Carnot.

The Founders of the Rational Theory. Rankine, Clausius, Zeuner.

The impetus given to the study of thermodynamics by the discovery of Joule, and the perception of the fundamental importance of Carnot's theorem, was enormous. Heat problems could now be brought out of the region of mere empirical solutions, and treated from the rational standpoint of an exact science, and the steam engine, as the most important example of heat transformation, attracted at once the attention of scientific men of commanding intellectual ability. In a very few years Rankine and Clausius had built up a strictly rational mathematical theory of the steam engine, and, a little later, Zeuner carried further the analysis of some of the more subordinate details. The theory with one exception, to be referred to presently, took account of all the actual conditions under which steam is used, and furnished exact rules for the relation of steam expended and work done for all arrangements of the actual steam engine practically adopted.

It was just at this time that the experiments of Isherwood were published, and a comparison of experimental results and theoretical calculations showed directly a very large discrepancy. The steam consumption in some trials was 30, 40 or 50 per cent. more than it should have been in the assigned conditions of working according to the rational theory. Some action of quite governing importance had obviously been neglected in the theoretical analysis.

The Experimental Theory.—Hirn and the Alsatian School.

A year or two before Isherwood began his experiments, an Alsatian engineer, M. Hirn, had discovered and measured cylinder condensation.

Joule's discovery attracted Hirn's attention, and he set to work in 1854 to verify, by an exact engine test, whether the difference between the heat received by an engine and discarded in the condenser was the equivalent of the work done. His two most important memoirs relating to the steam engine, are a memoir on the utility of steam-jackets in 1855–6, and another on the use of super-heated steam in 1857. In these researches he devised a method of accurate engine tests, involving the measurement of all the quantities of heat received by or rejected from the engine, which, with hardly any change at all, is the method of accurate engine testing adopted ever since. Under his influence and direction, engine tests were carried out in Alsace for many years, and the results exactly analysed. It may be recalled that the admirable series of engine tests, the first tests in which the heat quantities were accurately measured in this country, which were made by Mr. Mair Rumley, and described in three papers on "Independent Engine Tests" in the *Proceedings* of this Society in 1882, 1885, and 1886, were trials carried out strictly in accordance with Hirn's methods.

As with Lord Kelvin, so with Hirn. It was the recognition of an apparent conflict of Joule's discovery with Carnot's law which first attracted his attention. It was the attempt to determine whether part of the heat supplied to an engine disappeared as work which determined the form of his trials. His experiments of 1854 showed that "heat in a steam motor is not only dispersed, but actually disappears, and the power obtained is exactly proportional to the heat which disappears as heat to reappear as motive power." Some rather later and more careful experiments enabled him to verify Joule's equivalent by the actual results of a large engine test to an accuracy of about one per cent.

The discovery of initial condensation, and the proof of the powerful action of a small amount of heat transmitted from the jacket, both pointed to the conductivity of the cylinder wall as the cause of the large waste of steam which the constructors of the rational theory had neglected. The cylinder is cooled during expansion, and still more during exhaust by an action analogous to internal radiation to the condenser. Before any work can be done in the next stroke, the wall has to be reheated by condensing fresh steam. The extreme facility with which steam yields or abstracts steam by condensing and evaporating, accounted for the rapidity of the action. The magnitude of the condensation increases with the range of temperature to which the cylinder wall is subjected. It is larger in condensing than in non-condensing engines, and larger with high ratios of expansion.

Some time ago I ventured to say that there was no trustworthy engine test which showed that the consumption of steam with a jacket is greater than without the jacket. I believe that is still true, but undoubtedly the economy due to the jacket varies in different cases from 30 per cent. to very nearly zero. Roughly, the jacket is more useful with small engines than with large; with slow engines than with fast engines; but all this amounts to little more than saying that the jacket is most useful in those cases where the initial condensation is largest. Just in proportion as the engine, whatever its type, is of the highest class and most scientific design, the jacket is less useful.

The jacket reduces, but it does not prevent initial condensation. Hirn looked for some more powerful way of heating the

cylinder wall without causing condensation: he found it in super-heating. He constructed, in 1855, a super-heating apparatus in the flues of the boiler at Logelbach, which still exists. The experiments with super heated steam were carried out between 1855 and 1856, and showed clearly the effectiveness of the method in reducing condensation. Super-heating came largely into use in the years 1860-70 in this country in marine engineering, the practice having been introduced here by John Penn. In every case in which it was used an economy of coal was realised. Generally the economy amounted to from 15 per cent. to 20 per cent. It was ascertained that this was due strictly to economy of steam, and not to the utilisation in the boiler of heat previously wasted. But the use of super-heated steam in this country was gradually abandoned, partly no doubt from some practical difficulties, but chiefly, I believe, because practical engineers had no clear idea why super-heating should produce so large an economy, and they were not indisposed to abandon a complication, the action of which they could not satisfactorily explain to themselves.

No possible improvement of the steam engine, of which we have any knowledge at this moment, offers anything like so great a chance of important economy as the reintroduction of super-heating, and especially of super-heating to at least 100° or more above the saturation temperature of the steam. I obtained in Alsace on a very good 500 H.P. compound mill engine with jackets, and every appliance for economical working, an economy of 15 per cent. Mr. Mair Rumley has fitted a super-heater to a Babcock boiler supplying a triple engine, and has obtained an economy of 10 per cent. In both cases the economy is economy of steam, and therefore is not due to any increase of boiler surface or increase of efficiency in generating the steam. Lately Prof. Schröter, of Munich, has been experimenting with a small special compound condensing engine of only 60 I.H.P., running at the moderate piston speed of 380 feet per minute, and with the not excessive boiler pressure of 165 pounds per square inch. The H.P. cylinder is not jacketed. The L.P. is jacketed with receiver steam. In this case in a tube super-heater of a rather special construction in the uptake of the boiler, the steam is superheated to 670° F., or nearly 300° above the saturation temperature corresponding to the pressure. In two trials of six and eight hours' duration, periods quite long enough for accurate determination of results with so accomplished an observer as Prof. Schröter, the consumption of steam was only 10·2 pounds per I.H.P. hour, and the consumption of German coal of moderate quality only $1\frac{1}{4}$ pounds per I.H.P. hour. The steam consumption is the lowest on record for any engine of any type or size, and is very remarkable for so small an engine.

Conflict of the Rational and Experimental Theories.—Zeuner, Hirn, and Hallauer.

On the appearance of Isherwood's researches in 1863, the discrepancy between the rational theory and the results of experiments was recognised by Rankine and others. But the conditions of cylinder condensation are so complex, that for a long time the more theoretical writers practically ignored both Hirn's and Isherwood's results. Zeuner, perhaps, had pushed the rational theory to the furthest limit of detail, and with the greatest insight into practical conditions. But it was not till 1881 that he began to explicity admit the largeness and importance of the condensing action in the cylinder. Zeuner then was disposed to attribute initial condensation to the presence of a permanent and not inconsiderable mass of water in the clearance space of the engine. No doubt it is simpler analytically to deal with the thermal changes of the steam plus a given mass of water than with the thermal changes of steam, water, and a varying area of solid cylinder wall. In opening a discussion with Hirn in 1881, Zeuner wrote that if the presence of water in the clearance space was conceded, the Alsatian calculations would be greatly shaken, and "the enormous influence which they attributed to the cylinder wall would in future be attributed in part, perhaps chiefly, to the water in the clearance space." He thought it conceivable that in certain cases the whole of the initial condensation was due to water in the clearance space. There thus arose a rather angry controversy, which has been summed up in the question, "Is it water or iron?" I do not know that this controversy has been as yet completely decided, or that perhaps an absolute decision is possible. I cannot help thinking that Hirn, with the clearness of view due to his experimental work, had on the whole the best of the controversy, and I do not know that anything better or more instructive can be said than the words in which he finally summed up his position. "We recognise," he said, "that the interpretation of the Alsatians differs from that of M. Zeuner, not at all in that it denies the possible presence of water in the cylinder (we are not so hydrophobic), but in that it admits that the water, varying in quantity, is presented only temporarily, is carried away and renewed stroke by stroke, and acts chiefly as the medium between the steam and the cylinder wall. In the Alsatian explanation the action of the water raises the thermal action of the sides. In Prof. Zeuner's view, the water is permanently present and acts independently of the cylinder sides."

Recent Systematic Experiments. Willans.

It has been quite impossible in this lecture to do more than select one or two of the most important of the experimental investigations during the last fifty years. But I should not like to omit all reference to the two series of experiments of the late Mr. P. W. Willans. Mr. Willans' work is no doubt well known to all steam engineers, and needs no detailed description. However purely practical the object Mr. Willans had in view, his experiments were made in the true spirit of scientific research. No trouble was too much to secure accuracy to the last decimal, no possible cause of error was so trivial that its investigation was reckoned unnecessary. A few experimenters, Isherwood, Gately and Kletsch and others had made experiments on a methodical system, varying a single factor at a time. Willans carried out the method of experiments in series on a scale which, till he proved that it could be done, no one would have supposed possible. There is a series of non-condensing and a series of condensing trials; in each there are trials of simple, compound, and triple engines; and for each of these, again, trials with initial pressure varied, with expansion varied, and with speed varied. The results, tabulated in the clearest way, form a quarry of scientific data, but at present, in the main, an unworked quarry. Perhaps that statement will seem surprising, and of course I am expressing only my own view, for which I claim no infallibility. What Mr. Willans might have done had he been spared, it is impossible to say. He had the most active mind and the widest experience devoted, perhaps, at any time to the study of steam problems.

Let me protest as strongly as possible, again with the reservation that I am stating my personal view, against the tendency to suppose that the great work of Willans can be summed up in a so-called Willans' law, or that that law, handy as it may be for practical steam engineers, is more than a quite subordinate part of Willans' work. The Willans' law is nothing more than the empirical descriptive statement that the relation of total steam consumption and indicated or effective horse-power can be very approximately expressed by a linear equation, for the case of an unjacketed engine working with a fixed cut-off. Further, nothing is done in Willans' papers to fix what is the linear equation for any given engine. So far as those papers go, and until some kind of theory taking account of initial condensation is discovered, we can only find the relation of steam consumption and horse-power for any given engine by making two accurate trials of the engine itself. Willans' law leaves us in regard to any given engine in the same position as an astronomer with a new comet. When the comet has been observed for a sufficient period, and some of its positions fixed, a probable orbit can be calculated. The straight-line law leaves the steam consumption of a new engine as unknown as the elliptic law the orbit of a new comet.

Willans himself says nothing whatever as to any possible rational basis for the Willans' law. He put it forward purely as the result of plotting his experiments. Later, Captain Sankey showed that the total steam consumption of an engine working adiabatically with fixed ratio of expansion would also follow nearly but not exactly a straight-line law if all clearance losses, radiation, and exhaust waste and back pressure loss were neglected.

If we assume isothermal expansion (and really so far as the area of the diagram is concerned, it matters little what law of expansion is assumed), it is easy to find a formula for the total steam consumption of an engine working without clearance loss or exhaust waste. I have found such a formula, and plotted the results both for a condensing and a non-condensing engine in the diagram. It is found that the lines plotted are not exactly, but very nearly, straight lines. That carries us a certain way, but it is an enormous jump to assume without examination that the steam wastes in the engine, amounting to from 20 to 50 per cent. of the steam used, and arising from causes of the most com-

plex kind, depending on the volume of the clearance, the action of the cylinder wall, the loss of the toe of the diagram, the waste expansion between the cylinders, and other causes of loss, that these also can be expressed as a simple linear function of the horse-power.

Now, in the first edition of his treatise on the steam engine, which appeared in 1878, Prof. Cotterill had seriously attacked the problem of cylinder condensation from the theoretical side. Prof. Cotterill found it possible to give a partly rational, partly empirical, formula for cylinder condensation.

But, according to his formula for unjacketed simple engines, the initial condensation has a fixed ratio to the steam present at cut-off. In the diagram, lines for steam present at cut-off are given, calculated in the manner already described. Above these has been set up the condensation by Cotterill's law, and the total steam consumption at various loads is then given by a line very nearly straight and closely agreeing with a Willans' line.

The curves on the two diagrams agree well with Willans' results, and they differ from Willans' lines in being obtained entirely by calculation without experimenting on the engine. It would not be right to make too much of the coincidence, but I thought it would be interesting to show that theory and experiment converge. A good deal has yet to be explained, but the discussion in Prof. Cotterill's treatise has done more than anything else to throw light on the conditions which promote or hinder cylinder condensation, and on the means useful in securing economy of working.

Since 1845, purely scientific men, scientific experimenters, and practical engineers have all been engaged in the study of the steam engine. I do not believe that any one of the three can claim all the credit for the improvement of the steam engine to the exclusion of either of the others.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Oxford.—At a Congregation of the University held on Tuesday, 21st inst., the proposed Statute on Degrees for Research was passed in its final form, *nemine contradicente*. It only remains for the Statute to be passed by Convocation, and it will come into force.

At the same meeting, the addition to Statute conferring the title of Professor of Anthropology on Dr. E. B. Tylor, so long as he shall hold the office of Reader in Anthropology, received the final sanction of Congregation. A proposal recognising Anthropology as a subject for the Final-Honour School of Natural Science was then brought forward. After some debate the preamble was passed. Placets 24, non-placets 16.

Cambridge.—The following is the Speech delivered by the Public Orator, Dr. Sandys, in presenting for the honorary degree of Doctor in Science, Mr. Francis Galton, F.R.S.

Sedes olim sibi notas hodie revisit alumnus noster, qui flumine Nilo quondam explorato, et Africa Australi postea perlustrata, velut alter Mercurius omnium qui inter loca deserta et inhospita peregrinantur adiutor et patronus egregius exstitit. Idem, velut alter Aeolus, etiam ipsos ventos caelique tempestates suae provincae audacter adiunxit. Hodie vero Academiae nemora nuper procellis nimium vexata non sine misericordia contemplatus, e frondibus nostris caducis capiti tam venerabili coronam diu debitam imponi patitur. Tempestatum certe in scientia iamdudum versatus, ventorum cursus tabulis fidelibus olim mandavit, gentesque varium caeli morem praediscere docuit, laudem philosopho cuidam antiquo a Nubium choro Aristophanico quondam tributam uno saltem verbo mutato meritus: *οὐ γὰρ ἂν ἄλλῳ γ' ὑπακούσαιμεν τῶν νῦν μετεωρολογούντων.* Longum est avorum et proavorum ingenia magna in ipsorum progenie continuata ab hoc viro, Caroli Darwinii cognato, virorum insignium exemplis illustrata percensere. Longum est tot honores titulosque ab ipso per tot annos cumulatos commemorare. Hoc autem in loco, eloquentiae eius undecim abhinc annos conscio, instituti anthropologici praesidem non corporis tantum sed etiam mentis humanae mensorem appellaverim. Inter antiquos quidem celebratum erat illud Protagorae, omnium rerum mensuram esse hominem. Inter recentiores autem notum est hunc praesertim virum hominum omnium, imprimis pessimorum, mensuram ad amussim velle exigere. Ceterum plura hodie dicere supervacaneum est; constat enim ne optimorum quidem virorum a laudibus abesse debere mensuram.

Duco ad vos virum de scientia anthropologica et meteorologica praeclare meritum, caeli et terrae indagatorem indefessum, studiorum denique geographicorum etiam inter nosmet ipsos fautorem insignem, Franciscum Galton.

Lord Acton will deliver his inaugural lecture as Regius Professor of Modern History on June 11, at noon.

Prof. Lewis announces courses of lectures and demonstrations in Crystallography during the Long Vacation, beginning on July 9.

Prof. Roy announces a practical course in Bacteriology, to be given by Dr. Wesbrook and Dr. Lazarus-Barlow, in the Long Vacation, beginning on July 8. There will also be a course of lectures with practical work in Elementary Pathology, beginning on July 9.

Mr. H. F. Baker, Fellow and Lecturer of St. John's College, has been appointed University Lecturer in Pure Mathematics, in the place of Dr. Forsyth, now Sadlerian Professor.

A year ago a committee, representing various educational bodies, was formed, at the instance of the Association of Headmasters, to formulate an examination syllabus on which to award major scholarships, offered by County Councils and similar bodies, and tenable at places of higher education. All who know how very different are the scholarship schemes of the County Councils, agree that a larger degree of uniformity should prevail in the examinations held for the purpose of selecting candidates for the scholarships. The Association's scheme for major scholarships has been so drawn up that only candidates showing ability and intelligence distinctly above the average can be elected. Candidates must not be more than eighteen years of age in case of boys, and nineteen years in case of girls, and must have passed a preliminary examination to test their general education before they can compete for the scholarships. The scheme provides that the final scholarship examination shall consist of three groups—science, commercial, and literary—each containing obligatory and optional subjects. The subjects of examination for science scholarships have been carefully selected, and with due consideration to the claims of practical work.

The second annual report of the Technical Education Board of the London County Council appears in the *Technical Education Gazette*. A sum of nearly £15,000 was granted, during the year covered by the report, to intermediate and secondary schools. The fact that the Board has now over six hundred scholars attending these schools indicates how seriously it is concerned with technical education. While the question of the Teaching University for London has been in abeyance, the Board has not been able to act upon the proposal in Mr. Llewellyn Smith's report to contribute £10,000 a year towards the technical departments of the University. It was thought undesirable, however, to wholly abstain from helping institutions of university rank until the establishment of the Gresham University, so a grant of £1000 was made to University College, and £500 to Bedford College. The polytechnic institutes are rapidly increasing in extent and advancing in efficiency. The total annual expenditure of the eight institutions open last year amounted to about £90,000, their total number of students to over 27,000, and their aggregate number of separate classes or courses of lectures to over 1250. It is believed that the polytechnics now give probably nine-tenths of all the evening instruction in technological subjects in London, and three-fourths of the evening science instruction. All this represents an immense advance on the state of things five years ago, and indicates that the Board has remarkably extended the facilities for technical education during the two years it has been at work.

The fourth annual report (2 vols.) of the U.S. Commissioner of Education has been received. The volumes provide a mine of information on educational methods in France, Austria, Germany, Sweden, Switzerland, Alaska, the United States, and our own country. A full account is given of the character and development of German Universities, by Prof. Paulsen, of Berlin, supplemented by a statistical review of the subject by Prof. Conrad, of Halle. School museums in various parts of the world form the subject of a separate chapter. There is also an elaborate paper in which methods of physical training are very fully treated.

SCIENTIFIC SERIALS.

Bulletin of the American Mathematical Society, vol. i. No. 7, (April 1895).—"Riemann and his significance for the development of modern mathematics," is the translation, by A. Ziwet, of an address delivered by Prof. F. Klein at the general session of the Versammlung Deutscher Naturforscher und Aerzte in Vienna, September 27, 1894. In it the author attempts to give an idea of the life-work of Bernhard Riemann, "a man who more than any other has exerted a determining influence on the development of modern mathematics."—Prof. Cajori contributes a note on the multiplication of semi-convergent series, in which, following up his work in a recent number of the *Bulletin*, he further extends results arrived at by Pringsheim (*Math. Ann.* vol. xxi. pp. 327–378) and by A. Voss (*Math. Ann.* vol. xxiv. pp. 42–47).—Mr. L. E. Dickson discusses Gergonne's Pile Problem (*cf.* Ball's "Recreations," pp. 101–6), and points out one or two slight inaccuracies in a proof given by Dr. C. T. Hudson in *Educational Times* Reprints, vol. ix. pp. 89–91.—Prof. Ziwet gives an account of the Repertoire bibliographique des Sciences Mathematiques, *i.e.* a card catalogue of mathematical literature which has been widely circulated amongst mathematicians. Notes, and new publications, as usual, close the number.

Bulletin de l'Académie Royale de Belgique, No. 3.—On Chandler's formulæ, by F. Folie. The author criticises the latest formula enunciated by Chandler for the variation of latitude. Even when compared with the Strassburg observations, which most closely accord with the formula, it is evident that the periods are not correctly rendered. The constants in the formula require further empirical research. On the equations of the physical field, by Ch. Lagrange. The form, *i.e.* the law of distribution of a quantity of matter round its centre of inertia, constitutes in physics a principle as important as the quantity of matter itself, or its mass. Besides the principle of concentration, there is a principle of direction, and the latter is as important as the former. The author investigates the equations of motion in a medium consisting of rigid points, and introduces the conception of axial matter (*matière axée*), in which account is taken not only of the mass of a point, but also of all the qualities depending upon the shape of the mass. The density of a point is simply the intensity of one of the parameters determining its action, but a large number of other parameters of known form remain to be considered. The consideration of axial matter leads in a manner analogous to that which obtains in Kelvin's theory of the intensity of magnetisation, to theorems upon wires, plates, and leaves of similar substance, and then upon bodies made up of these structures.—On the colour, density, and surface tension of hydrogen peroxide, by W. Spring. This substance, which is highly explosive in the anhydrous state, has a blue colour when seen in a thickness of 100 cm. The colour resembles that of water, but is 1·83 times as intense. The density of the anhydrous substance is 1·4996. When 60·0445 gr. of it are contained in 100 cc. of an aqueous solution, the density is 1·2540. The surface tension is 0·456, that of water being 1. The addition of 6·4 per cent. water raises the surface tension by 102·5 per cent. Action of certain hot gases upon red phosphorus, by A. J. J. Vandevelde. Retger's supposition that phosphamine is produced by passing hot hydrogen over red phosphorus is not correct. Vapour of phosphorus is formed and carried off by the hot gas, exhibiting the phenomenon of spontaneous combustion on emerging into the air. Other hot gases, such as nitrogen, CO_2, CO, SH_2 and dry HCl gave rise to the same phenomenon.

Wiedemann's Annalen der Physik und Chemie, No. 4.—On luminescence, by Eilhard Wiedemann and G. C. Schmidt. An important distinction must be drawn between luminescence due to physical and that due to chemical causes. A prolonged afterglow makes the presence of chemical luminescence very probable. Thermoluminescence occurs after the body has been exposed to a temperature far below incandescence. A phenomenon now called "lyoluminescence" occurs with some substances during solution, when they have been previously exposed to strong light. The authors show that luminescence under cathode rays is always accompanied by chemical action. Mixtures of calcium and manganese salts show luminescence phenomena of great brilliance under cathode rays, and when subsequently heated. On normal and anomalous dispersion of electric waves, by L. Graetz and L. Fomm. The dielectric constant and the conductivity of a body are not perfectly independent quantities, but are connected by the constitution of the body in a manner similar to that in which refraction and absorption are connected in optics.—Magnetisation of iron by very small forces, by Werner Schmidt. Steel obeys very small magnetising forces more rapidly than iron. The limit of proportionality between magnetising forces and magnetic moment may with practically sufficient accuracy be placed at a field intensity of 0·06. Otto von Guericke's original air pump, by G. Berthold. The pump in the Royal Library at Berlin cannot be considered as Guericke's original air pump, since the latter was bought by the Archduke of Saxony, and taken to Sweden by Dr. Heraeus, where it was used as late as 1726 as a lecture instrument. When last heard of, in 1734, it was in charge of the Professor of Mathematics at Lund.—Remarks upon Mack's paper on the double refraction of electric rays, by Wilhelm von Bezold. The different behaviour of wood towards electric radiation along and across the fibre may be shown in a variety of ways. Lichtenberg's figures on wooden plates cut along the fibre show an elliptical shape, like doubly refracting crystals. A similar phenomenon is exhibited by a plate of ebonite rendered anisotropic by sticking strips of tinfoil parallel to each other on the other side. The production of a doubly refracting or even a circularly polarising body for electric rays by embedding conducting rods in a suitable dielectric does not appear to be hopeless.

THE only article of general interest in the *Nuovo Giornale Botanico Italiano* for April is one by Dr. U. Brizi, on the disease of the vine known as *brunissure* or blackening. The plasmode found in the diseased cells of the leaves cannot, he considers, be properly referred to *Plasmodiophora*, as has been done by most authorities hitherto. It belongs to an organism which appears rather to present characters intermediate between the Myxomycetes and the Amœbæ.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, April 25.—Mr. A. G. Vernon Harcourt, President, in the chair.—The following papers were read:—Action of nitroxyl on amides, by W. A. Tilden and M. O. Forster. The interaction of nitroxyl chloride and amides usually results in the exchange of the amidogen group for an atom of chlorine.—Action of nitrosyl chloride on asparagine and aspartic acid; formation of lævorotatory chlorosuccinic acid, by W. A. Tilden and B. M. C. Marshall. Asparagine and nitroxyl chloride interact with formation of Lævo-chlorosuccinic acid.—A property of the non-luminous atmospheric coal-gas flame, by L. T. Wright.—Diortho-substituted benzoic acids. (1) Substituted benzoyl chlorides, by J. J. Sudborough.—Diortho-substituted benzoic acids. (2) Hydrolysis of aromatic nitriles and acid amides, by J. J. Sudborough. In these two papers the author describes a number of new nitro- and bromo-benzoyl chlorides and benzoic acids.—Note on the action of sodium ethylate on deoxybenzoïn, by J. J. Sudborough. When deoxybenzoïn and sodium ethoxide are heated together, stilbene and hydroxydi-benzyl are produced.—A constituent of Persian berries, by A. G. Perkin and J. Geldard. In addition to the substances previously isolated from Persian berries, the authors have obtained a quercitin dimethyl ether which they term rhamnazin.—Potassium nitrososulphate, by E. Divers and T. Haga. The potassium nitrososulphates, by described Hantzsch, and by Raschig, seem to be identical with that first prepared by Pelouze. The milk of the gamoose, II., by H. D. Richmond.

May 2.—Studies on the constitutions of the tri-derivatives of naphthalene. No. 10, the dichloro-α-naphthols and trichloronaphthalenes from 3 : 4-dichlorophenyl-1-isocrotonic acid. No. 11, the trichloronaphthalene derivable from Cleve's 1 : 2 : 2-α-nitrochloronaphthalenesulphonic chloride. No. 12, the trichloronaphthalene, derivable from Alén's α-nitronaphthalene-2 : 2′-disulphonic chloride. No. 13, the α-naphthylamine-2 : 2′-disulphonic acid of Freund's German Patent, 27346. No. 14, the fourteen isomeric trichloronaphthalenes. The non-existence of a trichloronaphthalene melting at 75·5°: the formation of chloro-derivatives from sulphonic chlorides, by H. E. Armstrong and W. P. Wynne. In these six papers the authors describe thirteen out of the fourteen possible isomeric trichloronaphthalenes, together with a large number of compounds obtained during the preparation of these halogen derivatives. The solubilities of gases in water under varying pressure, by E. P. Perman. Henry's law holds for chlorine, bromine, carbon dioxide, and hydrogen sulphide, but large deviations are observed with ammonia, hydrogen chloride, and sulphur dioxide.—The existence of hydrates and of double compounds in solution.

Part 1, by E. P. Perman. From experiments on the pressure of gases dissolved in various solutions the author concludes that sodium sulphate exists in aqueous solution as the hydrate $Na_2SO_4.10H_2O$, and that silver chloride exists in ammoniacal aqueous solutions as the compound $AgCl.3NH_3$.—Derivatives of π-bromocamphoric acid, by F. S. Kipping.—Paraheptyltoluene and its derivatives, by F. S. Kipping and O. F. Russell.—Note on the formation of a phosphate of platinum, by R. E. Barnett. On passing phosphorus pentoxide vapour and oxygen over red-hot platinum, a yellow phosphate PtP_2O_7, insoluble in aqua regia, is obtained.

Linnean Society, May 2.—Mr. C. B. Clarke, President, in the chair.—Dr. O. Nordstedt of Lund, Dr. Rudolph Philipppi of Santiago, and Dr. M. Woronin of St. Petersburg, were elected foreign members. Mr. H. M. Bernard showed under the microscope the circumscribed patches of setæ above and below the stigmatæ on the pupa of the vapourer moth (*Orygia antiqua*). The arrangement suggested a vanished notopodium just where, in the Hexapods, a dorsal branch of a parapodium ought to have vanished, according to the exhibitor's method of deducing the different groups of the Arthropoda from their Annelidan ancestors, as sketched in his recent paper on the *Galeodidæ*.—Mr. E. M. Holmes exhibited some new British Algæ from Dorsetshire and Surrey; amongst others, *Ulvella confluens* and *Ectocarpus Reinholdi*, both discovered last month at Weymouth, and the latter previously known only from the Baltic.—Mr. J. E. Harting exhibited and made remarks on a specimen of *Cuculus canorus* in the rare hepatic plumage (*Cuculus hepaticus*, Sparrman), recently obtained at Bishop's Waltham, Essex.—Mr. W. T. Thiselton-Dyer, C.M.G., then gave an abstract of a paper by the late Mr. John Ball, F.R.S., on the distribution of plants on the southern side of the Alps, prefaced by some account of the author's life, and special work in relation to the Alpine flora.

Mathematical Society, Thursday, May 9.—Major P. A. Macmahon, R.A., F.R.S., President, in the chair.—Dr. Hobson, F.R.S., made a communication on the most general solution of given degree of Laplace's equation.—Prof. M. J. M. Hill, F.R.S., read two short notes: (1) a property of a skew determinant; (2) on the geometrical meaning of a form of the orthogonal transformation.—Prof. Greenhill, F.R.S., and Mr. T. I. Dewar gave an account of results relating to the spherical catenary. The investigations given in NATURE, p. 262, January 10, 1895, when the parameter of the associated elliptic integral of the third kind is of the form $4\omega_3/\mu$, where ω_3 is the imaginary period and μ is an integer, worked out in detail for $\mu = 3, 4, 5$, and 8, have been extended by Mr. Dewar to the cases of $\mu = 6, 7, 9, 10$, and 12.

In particular, when $\mu = 10$, the catenary is given by an equation of the same form as for $\mu = 5$,

$$(1 - z^2)^{\frac{5}{2}} e^{5\chi i} = Hz^5 + H_1 z^4 + H_2 z^3 + H_3 z^2 + H_4 z + H_5 + i(Lz^3 + L_1 z^2 + L_2 z + L_3)\sqrt{Z},$$

where

$$Z = (1 - z^2)(z - h)^2 - A^2,$$

and

$$\chi = \psi - p\int Z^{-\frac{1}{2}} dz;$$

and it was found that p could be made to vanish, so that the catenary becomes a closed algebraical curve on the sphere, by taking

$$h = \frac{1}{2}\sqrt{\frac{17}{3}},\ A = -\frac{1}{10}\sqrt{\frac{5}{3}},\ L = -\frac{5}{6}\sqrt{\frac{5}{3}},$$

$$L_1 = -\frac{5}{36}\sqrt{85},\ L_2 = \frac{35}{72}\sqrt{\frac{5}{3}},\ L_3 = \frac{13}{144}\sqrt{85};$$

$$H = \frac{1}{6}\sqrt{\frac{17}{3}},\ H_1 = 0,\ H_2 = -\frac{5}{12}\sqrt{\frac{17}{3}},\ H_3 = -\frac{25}{108},$$

$$H_4 = \frac{65}{288}\sqrt{\frac{17}{3}},\ H_5 = \frac{25}{144}.$$

A model was exhibited of this spherical catenary, formed by a chain wrapped on a terrestrial globe; and so far this appears to be the only real algebraical case, for which it is possible for p to vanish.—Mr. G. Heppel exhibited a set of Napier's Bones, of date 1746, and explained how they were used in calculations, referring for a further description of them to the *English Cyclopædia*.—The following papers, in the absence of their authors, were taken as read:—On those orthogonal substitutions that can be generated by the repetition of an infinitesimal orthogonal substitution, by Dr. H. Taber.—Notes on the theory of groups of finite order (continuation), by Prof. W. Burnside, F.R.S.—Applications of trigraphy, by Mr. J. W. Russell: and the reciprocators of two conics, by Messrs. J. W. Russell and A. E. Jolliffe.

Zoological Society, May 7.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—A letter was read from Dr. F. A. Jentink, concerning a monkey lately described as *Cercocebus aterrimus*, of which the type had lately been acquired by the Leyden Museum. Dr. Jentink considered this monkey to be the same as *Cercocebus albigena*, Gray.—Mr. J. H. Gurney exhibited and made remarks on a rare kingfisher (*Alcedo beavani*) obtained in Ceylon by Mr. A. L. Butler.—Mr. G. F. Scott Elliot made some remarks on the fauna of Mount Ruwenzori, in British Central Africa. Mr. Scott Elliot stated that elephants occur in great numbers on the east side of Ruwenzori. There were also many still living and vast stores of ivory in the Congo Free-State, just beyond the south-west corner of the English sphere of influence. He pointed out the presence of the hippopotamus in the Albert-Edward Nyanza, and its extraordinary abundance in the Kagera River. The rhinoceros was found frequently in the country of Karagwe, usually near the marshy lakes leading to the Kagera.—On the alluvial plains about the east of Ruwenzori, Jackson's hartebeest (*Bubalis jacksoni*), the kob (*Cobus kob*), and another waterbuck (perhaps of a new species) were common. No buffaloes were seen. A bushbuck also occurred on Ruwenzori from 7000 to 8000 feet. Of monkeys, Mr. Scott Elliot had noticed the presence of a black and white *Colobus*, which he could not identify, and of at least two other species, probably a *Cercopithecus* and a baboon. Some small mice brought home had not yet been identified. Leopards were numerous, and lions were also common on the lower grounds. Two species of sunbird were brought back, one of which ascends to 11,000 feet on Ruwenzori. Mr. Scott Elliot concluded by remarking that the general idea of distribution gathered from the flora seemed to confirm such data as he could gather from the fauna of the country which he traversed during his journey.—Mr. F. E. Beddard, F.R.S., and Mr. P. Chalmers Mitchell made a communication on the structure of the heart in the alligator, as observed in specimens that had died in the Society's menagerie.—Mr. Chalmers Mitchell described the anatomy of the crested screamer (*Chauna chavaria*), pointing out some resemblances between the alimentary canal of that bird and the ostrich, and giving a detailed comparison of the structures of *Chauna chavaria* and *Palamedea cornuta*.—A communication was read from Dr. Percy Rendall, containing field-notes on the antelopes of the Transvaal.—Dr. Mivart, F.R.S., read a paper on the skeleton of *Lorius flavopalliatus* as compared with that of *Psittacus erithacus*.

Geological Society, May 8.—Dr. Henry Woodward, F.R.S., President, in the chair.—The Stirling dolerite, by Horace W. Monckton. The rock described in the paper forms a mass of about eight miles in length, with an average width of about a mile; it is intruded into the lower part of the carboniferous limestone series. There is little doubt that the Abbey Craig rock, north of the Forth, is connected with the Stirling rock; and there is reason to think that the igneous rocks of Cowden Hill and of the hills around Kilsyth are outlying portions of the Stirling rock, being connected with it underground. All these patches, as well as the main mass, are for the most part composed of a more or less coarse-grained dolerite, the marginal part always becoming finer-grained, whilst the actual edge has apparently been a tachylyte now devitrified. The author gave the results of his macroscopic and microscopic examination of the rocks from various parts of the mass.—Notes on some railway cuttings near Keswick, by J. Postlethwaite. Several cuttings have recently been made on the Cockermouth, Keswick, and Penrith Railway, chiefly through drift, though some occur in the Skiddaw slates, and in one case a diabase dyke (much decomposed) was met with. The author described the drifts as blue clay beneath, and brown clay above, and considered that these two clays were produced during two separate periods of glaciation, with no long interval between. In some places near Keswick water-borne gravel may be seen surmounted by blue clay; this gravel was considered by the author to be of fluviatile origin.—The shelly clays and gravels of Aberdeenshire considered

in relation to the question of submergence, by Dugald Bell. The drifts of this region have been described by Mr. Jamieson, and also in the publications of the Geological Survey. The two authorities agree that the lower (grey) boulder clay of the district was produced by a local glaciation. The geological surveyors, however, maintain that the intervening sands and gravels with marine shells were produced during a submergence of 500 feet or upwards, whilst the upper (red) boulder clay was formed by an ice-sheet from the south. Mr. Jamieson, on the other hand, assigns a purely glacial origin to the middle sands and gravels, and considers that the red clay (which contains a few fragments of marine shells) indicates a submergence. The author discussed these views, and maintained that submergence is not proved in the case of either middle gravels or red clay, but that the former are, as Mr. Jamieson maintained, truly glacial, whilst he advocated the existence of extra-morainic lakes to explain the latter.

PARIS.

Academy of Sciences, May 13.—M. Marey in the chair.—On the cœlostat, a mirror apparatus giving an image of the sky which remains fixed with regard to the earth, by M. G. Lippmann. A plane mirror is mounted on an axis resting on fixed bearings. The mirror and its axis are parallel to the polar axis. A motor turns the system at a uniform speed once round in forty-eight sidereal hours, in the same direction as that of the celestial sphere. The author gives a proof that this mirror fulfils the necessary conditions, and points out wherein it differs from the ordinary siderostat. He shows how the siderostat can be used to demonstrate the principle of the cœlostat, and how the latter instrument can be employed in place of an equatorial. Thermochemical relations between the isomeric forms of ordinary glucose, by M. Berthelot. Three forms of glucose are distinguished: α, the ordinary form, for which $\alpha_D = +106^\circ$; β, produced by transformation of α at 100°, giving $\alpha_D = +52^\circ.5$; and γ, formed from α at 110°, having $\alpha_D = +22^\circ.5$. These rotations are observed immediately on solution; left for some time all are converted into the β form in solution. The change of α into β glucose absorbs 1·55 Cal., the corresponding change of γ into β glucose absorbs 0·67 Cal., in the anhydrous state.—On an automatic registering measuring machine for the comparison of end measures, by M. L. Hartmann.—Researches on the hatching of "l'œuf des sexués" of the vine Phylloxera, by M. L. J. Leroux.—The works printed in the correspondence are: A necrological notice on Ernest Mallard, by M. A. de Lapparent. Petroleum, asphalt, and bitumen, from the geological point of view, by M. A. Jaccard. Invasions of locusts in Algeria, by M. J. Künckel d'Herculais.—Demonstration of Tchébychef's theorem, by M. André Markoff.—On the equivalence of six different forms of expression of the quadratures of algebraical differentials reducible to elliptic integrals, by M. F. de Salvert.—On the integration of the system of differential equations, by M. A. J. Stodolkievitz.—On a new method for the production of fringes with great differences of phase, by M. Gouy. A theoretical paper.—On the electromagnetic theory of the absorption of light in crystals, by M. Bernard Brunhes.—Anomalous rotatory dispersion of absorbent bodies, by M. A. Cotton.—General solution of Maxwell's equations for a homogeneous and isotropic absorbent medium, by M. Birkeland.—On argon and helium. An extract from a letter by Prof. Ramsay to M. Berthelot. An account is given of a sample of gas obtained from a meteoric iron from Augusta County, Virginia, U.S.A. After sparking with oxygen and over caustic soda, the residual gas gave spectroscopic evidence of the presence of argon and helium. Only the lines of argon and helium were observed. This evidence is taken as proof that argon exists in extra-terrestrial bodies, though it has not been noticed in the sun. Helium is found in most of the rare earth minerals examined by Prof. Ramsay.—On the definite combination in copper-aluminium alloys, by M. H. Le Chatelier. The author corrects his previous announcement of the alloy AlCu. The substance had been more profoundly altered by the reagents used than was at the time suspected.—Estimation of sulphur in cast-irons, steels, and irons, by M. Louis Campredon.—Researches on mercurous chloride, bromide, iodide, and oxide, by M. Raoul Varet. A thermochemical paper giving details concerning the heats of formation of these salts.—On the molecular origin of the absorption bands of cobalt and chromium salts, by M. A. Etard. The conclusions are drawn: (1) That chromium salts and the red cobalt salts have fine spectroscopic bands, just as is the case with the rare earth and uranium salts. (2) That these are spectra of molecules like the spectra given by organic substances of the chlorophyll type. (3) The hypothesis that each band of the spectrum of a rare earth corresponds to an element is not necessarily true, according to the evidence of cobalt. (4) The bands may be displaced or disappear for one and the same element according to the nature of the molecules in solution or of the compound observed.—On the molecular modifications of glucose, by M. C. Tanret.—On the use of carbon tetrachloride as a means of separating methylene from ethyl alcohol, by M. Maxime Cari-Mantrand.—On a brown pigment in the elytra of *Curculio cupreus*, by M. A. B. Griffiths.—On the aeration of the soil in the Paris promenades and plantations, by M. Louis Mangin.—On the existence of numerous crystals of orthoclase felspar in the chalk of the Paris basin; proofs of their genesis *in situ*, by M. L. Cayeux.—On gypsum from the neighbourhood of Serres (Hautes-Alpes) and Nyons (Drôme), by M. Victor Paquier.—On the miocene near Bourgoin and Tour-du-Pin, by M. Henri Douxami.—On the presence of *Ostrea (Exogyra) virgula* in the upper Jurassic of the Alpes Maritimes, by M. Adrien Guébhard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Study of "Primitive" Man: E. Clodd (Newnes).—Catalogue of the Fishes in the British Museum, 2nd edition, Vol. 1 (London).—Year-Book of the Scientific and Learned Societies of Great Britain and Ireland, 12th annual issue (Griffin).—L'Industrie Chimique: A. Haller (Paris, Baillière).—My Climbs in the Alps and Caucasus: A. F. Mummery (Unwin).—Transmissions par Cables Métalliques: H. Léauté and A. Bérard (Paris, Gauthier-Villars).—Lessons in Elementary Physics: Prof. J. B. Stewart, new edition (Macmillan).—Agriculture, Practical and Scientific: Prof. J. Muir (Macmillan).—A Monograph of the Order of Oligochæta: F. E. Beddard (Oxford, Clarendon Press).—Fingerprint Directories: Dr. F. Galton (Macmillan).—First Principles of Astronomy: Prof. S. Cooke, 5th edition (Bell).—First Principles of Chemistry: Prof. S. Cooke, 6th edition (Bell).

PAMPHLETS.—Rapport Annuel sur l'État de l'Observatoire de Paris, 1894 (Paris).—Jamaica in 1895 (Kingston, Jamaica).—The Rise and Development of the Bicameral System in America: T. A. Moran (Baltimore).—The Pocket Gophers of the United States: V. Bailey (Washington).—The Student's Practical Chemistry: Test Tables for Qualitative Analysis: Prof. S. Cooke, 3rd edition (Bell).—Report of the Departmental Committee upon Merionethshire State Mines (Eyre and Spottiswoode).

SERIALS.—Journal of the Franklin Institute, May (Philadelphia).—Royal Natural History, Part 19 (Warne).—Aus dem Archiv der Deutschen Seewarte, xvii. Jahrg. 1894 (Hamburg).

CONTENTS.

THURSDAY, MAY 30, 1895.

THE SPIRIT OF COOKERY.

The Spirit of Cookery, a Popular Treatise on the History, Science, Practice, and Ethical and Medical Import of Culinary Art. By J. L. W. Thudichum, M.D., F.R.C.P.Lond. (London: Baillière, Tindall, and Cox; Frederick Warne and Co., 1895.)

THE scientific branch of culinary literature has just received in Dr. Thudichum's book an addition which cannot fail to attract the attention of those who give to the selection and preparation of food the consideration that the subject undoubtedly deserves. Of works which come under the denomination of kitchen text-books we have had of late years more than enough perhaps, but treatises on the culinary art from an academical and philosophical point of view have been few. "I could write," said Dr. Johnson, "a better book about cookery than has ever yet been written: it should be a book upon philosophical principles. Pharmacy is now made much more simple. Cookery may be so too. A prescription which is now compounded of five ingredients had formerly fifty in it. So in cookery. If the nature of the ingredients is well known, much fewer will do. Then, as you cannot make bad meat good, I would tell what is the best butcher's meat, the best beef, the best pieces; how to choose young fowls; the proper seasons of different vegetables; and then how to roast, and boil, and compound." The author of "The Spirit of Cookery" has evidently been guided by a similar recognition of the requirements of the case; and seeing that he is a member of a scientific profession which may be said to endow with special advantages those of the cloth who turn their attention to the study of food-stuffs and their treatment, it may be taken for granted that he has executed his task with competence and ability. His object has been "to produce such a system of general rules as will enable those who thoroughly master them to perform the principal culinary operations without reference to the frequently unintelligible records of the details of mere empiricism. These rules," continues he, "are based in the first place upon unimpeachable scientific data or fundamental truths which admit of no circumvention or compromise, and have to be obeyed under pain of certain failure. This obedience has at once its ample reward in clearing the subject of a mass of errors and delusions which disfigure it as a science, and impair its utility, and in placing into the hands of operators the means of attaining their object with certainty and elegance."

Strictly speaking, "The Spirit of Cookery" is a compendium of very useful information gathered, for the most part, from trustworthy sources; its theories are, generally speaking, sound, its principles excellent, and its rules good; but it can scarcely be called a practical work from an executive point of view, for the author rarely allows his descriptions of a process or a dish to go further than a mere sketch. Each branch of the art is nevertheless dealt with, and the principal methods of cooking, if not absolutely worked out in detail, are at all events carefully analysed.

After a glance at the objects of cookery, its literature in the past and present, the requirements of the kitchen, and the processes which appertain thereto, Dr. Thudichum comes to the subject of soup-making. That this is haustively treated, may be gathered from the fact ha more than one hundred pages are devoted to it. The salient feature of this discussion is an exposition of wha the author calls "the complete fallacy of the proposition that bones can either make, or help to make, any liquid that can have any value in cookery." This argument new, or rather let us call it a revival of an old controversy which has been forgotten. That a scientific writer as earnest and experienced as Sir Henry Thompson should have acknowledged, comparatively recently, the value of bones in cookery, in his work "Food and Feeding," would in itself justify our questioning Dr. Thudichum's rather peremptory dictum on this point. Speaking, however from absolutely practical experience to the contrary, we are forced to deny the accuracy of the contention. As a matter of fact, we have been in the constant habit of producing fragrant and savoury broths from the bones poultry and game, both cooked and uncooked, which we have found very valuable in sauces; while in soup-making our working has proved that after six hours cooking on the lines of the *pot-au-feu*, a very perceptible gelatinous element is produced from the bones, which contributes to the quality of the stock. In all circumstances it is of course essential that the bones be broken as small as possible and in the case of those of poultry and game that they be pounded roughly in the mortar. The latest method, viz. that of setting the bones of meat and carcases of poultry intended for the stock-pot to be browned in the oven before addition, is an undoubted improvement, to which the author of "The Spirit of Cookery" would not object perhaps, the addition being made after the first stage of the broth-making, *i.e.* after the liquid (containing the meat alone) has been permitted to come to boiling point for the first time, simmering being conducted afterwards for the allotted period.

Touching the alleged costliness of extracting gelatine from bones, we think that Dr. Thudichum has lost sight of the fact that, inasmuch as kitchen fires are always burning, space can generally be found on the hot-plate for a vessel containing bones where it can simmer without any additional expense in the matter of fuel. We have found that in this way, with the aid of a few vegetables and herbs, very useful broths can be made for the moistening of stews, *purées*, &c., while it is well known that at Aldershot good wholesome pea and lentil soups are made on a bone-stock basis, which form an addition to the soldier's dietary that is much appreciated, and for which no better medium, considering the limited resources of the military kitchen, could be concocted.

We confess that we are surprised at Dr. Thudichum's apparent indifference to vegetables as a factor in the production of a good *bouillon*, for constant practice has satisfied us that all its fragrance and a large share of its pleasant flavour come to the *pot-au-feu* or soup from a very careful proportioning of the vegetables to the meat by weight. In a case of this kind it is idle to speak of "an onion" or "a carrot." We also wonder that he should mention the now obsolete method of clarifying broths with whites of egg and lemon-juice. The object is now attained by raw beef reduced to a pulp, mixed with both the yolk

and white of egg, by which the loss of flavour by the old process has been overcome.

In regard to the author's condemnation of the statement that "the French cook makes excellent and nutritious soup out of materials which the English housewife throws away as useless, while her *pot-au-feu* is composed of stray scraps carefully husbanded, which cost her nothing, but which when skilfully combined constitute a useful and inexpensive food," we would observe that the use of the word *pot-au-feu* is obviously a mistake, but that had *marmite* been substituted there would have been no cause for objection. What says Sir Henry Thompson? "This the *pot-au-feu* is a different thing from the common 'stock-pot' of the French peasant, so frequently termed a *pot-au-feu* and confounded with it. The primary object of the 'stock-pot' is to make a decoction for soup—of animal food if possible—and every morsel of flesh, poultry, trimmings from joints, bones well bruised, &c., which are available for the purpose are reserved for it." This turning to account of scraps is, to our thinking, by no means a "delusion," but a thing that should be encouraged in every economically conducted kitchen. In nearly every other respect we are able to concur with Dr. Thudichum. He is undoubtedly right in pronouncing against the so-called clear soups of restaurants and hotels, in denouncing the free use of wine to smother defects, and the heedless use of cream and butter in *potages liés*, *bisques*, and *purées*.

Turning to his precepts concerning processes, we also find much that we can accept as excellent. Here and there are points, of course, in regard to which the best authorities differ. We would never put fresh meat or poultry, when either has to be cooked for the table by boiling, into *cold* broth or water, having found the method advocated by Sir Henry Thompson better than any other, viz. to immerse the joint or bird in a boiling medium to solidify or coagulate the albumen which pervades the outer layer of meat, and after five or six minutes at that temperature to reduce the heat beneath the vessel to simmering point, never exceeding 180 F. We apply the same principle to the preparation of fish with equally satisfactory results, having proved the accuracy of Sir Henry's axiom that boiling fish in the ordinary manner is of all systems the most wasteful and unsatisfactory. There can be no doubt, though it is contrary to Dr. Thudichum's theory, that the greatest benefit is to be derived from broth made from fish-bones and "cuttings" of white fish, assisted by herbs and vegetables. This we employ as a moistening in our method of fish-poaching, and consider it superior to *court bouillon* with its excessive amount of wine, which Dr. Thudichum very properly condemns.

There is another point on which the doctor's advice is open to question. We refer to his definition of braising as a species of "roasting." Surely this is contrary to the teaching of the best authors. "Braiser la viande," says Dubois, "c'est la cuire à l'étuvée dans un bon fonds de façon à l'atteindre complètement, en lui conservant ses sucs nutritifs." How can a piece of meat be said to be "roasted" when it is moistened in the *braisière* with *bouillon* "*à hauteur*"? The fact is there are varieties of braising. The French cook adopts one method, for instance, for white, and another for brown meats, and, as we read in "Food and Feeding," these vary in treatment. In all the predominating feature is stewing, though the part of the meat exposed by the gradual reduction of the moistening broth may be browned by heat transmitted downwards from hot cinders on the lid of the vessel. The meat is really part stewed, part steamed, and superficially toasted. Dr. Thudichum says nothing of the amount of moistening *mirepoix* necessary for braising, the preliminary browning of the meat, the *couche de racines et oignons émincés* on which it should be placed, the reduction of the first partial moistening, and then the final filling up level with the top of the meat. Without these instructions, how is the student to have placed in his hands "the means of attaining his object with certainty and elegance?"

But the few points to which we have taken exception are of no great consequence in a work which covers as much ground as "The Spirit of Cookery." Some of them might perhaps have been passed over as appertaining to practical work, which Dr. Thudichum may not have intended to explain minutely. There is, as we have said, a great quantity of information which is beyond criticism, plenty of advice which is full of common sense, and a painstaking classification of the principal sections of the art which cannot but be useful to students of cookery. The scientific principles, by which all intelligent work should be guided, are very clearly laid down. The notes on the preparation of food for the sick-room and the camp are excellent, and all who recognise the necessity of encouraging cookery for the palate rather than for the eye will concur in Dr. Thudichum's observations regarding the vulgar folly of over-ornamentation.

WEATHER OBSERVATION AND PREDICTIONS.

Meteorology, Weather, and Methods of Forecasting, Description of Meteorological Instruments, and River Flood Predictions in the United States. By Thomas Russell, U.S. Assistant-Engineer. (New York: Macmillan and Co., 1895.)

Results of Rain, River, and Evaporation Observations, made in New South Wales during 1893. By H. C. Russell, B.A., C.M.G., F.R.S. (Sydney: C. Potter, 1894.)

THE first of these two books has for its aim the instruction of those who are interested in the weather, and wish to make forecasts on scientific lines, or to understand the principles which underlie the predictions issued by responsible authorities. The expression "scientific lines" is, perhaps, not justified. Experience plays, probably, as large a part as science. The knowledge of the character of the weather that has followed certain definite atmospheric conditions in former cases, is to some extent a guide as to what will happen when those conditions again present themselves, and possibly as true a guide as any result based on the wider knowledge of the general circulation of the atmosphere. Especially has the particular study of the direction and rate of motion of cyclonic areas, with their attendant phenomena of rain, and change of temperature permitted a greater amount of security in weather predictions for

short intervals of time. But this great certainty is based upon experience and observation, rather than upon purely thermo-dynamic principles.

The evidence of decisive progress in forecasting is wanting. Nor does the author hold out a very sanguine hope of the possibility of issuing in the immediate future successful weather forecasts over large districts from a central bureau. There are not more than six to twelve occasions, in the course of a year, for any part of the country, he tells us in the preface, "where successful predictions can be made, and for some places successful predictions are never possible." "Successful continuous predictions for every day are not possible." This is the opinion of one who apparently has ample means of forming an adequate judgment. It is the outcome in a country where the opportunities of framing forecasts are many and favourable. The service is well supplied both with funds and officers, the vast telegraphic system of the country is at the disposal of the Weather Bureau, the area over which the data are collected is extensive enough to enable the whole development of a storm to be watched and reported, while the favourable situation of Washington, in the extreme east of the continent, is a point not to be omitted. Yet after years of trial, the opinion of one who apparently has official connection with the system, or is at least well supplied with information from the Bureau, is, that the complete solution of the problem is not only impossible, but is only practically effective on the average less than once a month. If this be the result under favourable conditions, what, it may be asked, is the system worth in England, where our insular position cuts off the supply of any information from the West, the direction in which our principal storms approach, and the intelligence from the East has to be supplied by the courtesy of many nationalities, and more or less hampered by different telegraphic systems.

To return to the book, however, which in some respects is a little disappointing. There is an occasional appearance of hurry in the compilation of the work, which has sometimes prevented the author expressing himself with sufficient clearness, and with the reservations which are sometimes necessary. For instance, we are told, on p. 3, that there is less oxygen in the air when the wind is from the south, than when the direction is north. This may be true for the district in which the author lives, but as there is no indication where this particular locality is situated, and the preface is not even dated, we are left to infer that the remark applies to the earth generally, which can scarcely be correct. Again, on p. 184, in the description of secondary low pressures, occurs this sentence. "In Fig. 29, thunderstorms are very apt to occur with secondary low pressures." This statement is certainly a puzzle. On p. 190 we are referred to a map on the adjoining page. There is no map there, although this map is referred to in the list of illustrations. Readers will, however, find it at the end of the book. Sometimes, too, facts which are easily verified are not quoted with accuracy. On p. 5, the dates of the earth's perihelion and aphelion passage are given as December 23 and June 21 respectively. The area of the Caspian Sea is given on p. 101 as over 200,000 square miles, and on p. 201 as 180,000 square miles. But these and many other small blemishes can be removed in a future edition.

We are more concerned to look at the work as a whole, and to consider what special service is it likely to render among the host of meteorological treatises that are continually appearing on one or other side of the Atlantic We have, of course, the ordinary chapter on meteorological instruments ; we have the cloud classification ; we have the description of the rain and hail and snow, that too frequently make life unpleasant ; together with all the winds that blow, or are likely to blow. And the oft-told tale, it must be confessed, is repeated in rather a jerky manner, partaking of something of the manner of a dictionary, wherein one is treated to a collection of definitions. The last chapters of the book are undoubtedly the best. There the author has something to tell us of processes not generally described in books like the present. To the charm of novelty is added the advantage that we feel we are listening to a practical expert, who can tell us all that is worth knowing about river-floods and overflows.

We come now to the second volume under notice. Fortunately in this country we are not frequently troubled by the overflow of rivers and the consequent destruction of property on the banks, and therefore the subject with us receives scant attention. Probably for this reason the report of the Meteorological Council is silent on such matters, though at times like last autumn, the inhabitants of Eton, Oxford, and the Thames Valley would have been gratified by a timely warning. It may have been that warnings were given, but from the absence from the Report of any mention of machinery adapted to this end, one would infer that this is an inquiry the Council do not consider worthy of their attention. Far different is it with the Astronomer at Sydney, whose latest report is mentioned at the head of this notice. There the subject is forced on the attention of scientific men : and on the unscientific, too, if he happen to live in a district where, as Mr. Russell reports, the rise of a river was so rapid that in less than two hours a part of a town was covered to a depth of three or four feet, and the people were glad to escape with their lives at the sacrifice of their property. Mr. Russell has great difficulties to contend with. He has not only the small equipment peculiar to a comparatively new colony, imperfect data, and the slow accumulation of facts, but the first warning of the rise of a flood may occur in uninhabited or thinly populated districts, with which communication is slow and uncertain. The American Bureau has not to struggle against these disadvantages, but the problem depends upon so many variable quantities that the complete solution is practically impossible.

The author of the treatise on meteorology lays it down that very little connection can be traced between meteorological laws and river floods, except perhaps in cases where the quantity of water is dependent upon the melting of the snow. In temperate zones, floods occur without any very noticeable great rainfalls. Intermittent rain may cause a river to rise very slowly, and almost imperceptibly, till it be bank-full, when a moderate rain makes the river overflow. Neither is there any decided connection between the river slopes and velocity, so that

the velocity of the flow cannot be computed from a knowledge of the slope. The character of the ground over which the rain falls—that is, the degree of permeability—is a fruitful source of uncertainty in predicting the probable rise. There are many other obvious sources of error, so that no one can be surprised to learn that the theoretical determination of a river rise cannot be treated as a problem in hydraulics. Without a system of gauges along the river, predictions are scarcely possible. With their employment, the problem becomes more or less one of practice and experience. This remark may be illustrated by showing how the rise of the river may be predicted for Pittsburg, a place where the observations of rainfall simply, are of little use in foretelling with accuracy the height to which the river will rise. The rise is predicted from observations of the rise at stations above the town, or on tributaries. Gauges are maintained at Oil City, Brookville, Confluence, Rowlesburg, Weston, and Johnstown. These towns lie both north and south of Pittsburg, and the greatest separation may amount to two hundred miles. The height of the river and its tributaries at each of these places not only exercises a different effect at Pittsburg, presumed to be proportional to the square root of the area drained by the rivers at each station, but the height of the river at Pittsburg itself has also to be taken into the account. The higher the stage at Pittsburg, the less will the river be affected by the same rise at the upper stations. "It is assumed that the rise multiplied by the mean stage during the rise is comparable throughout different stages for Pittsburg." The factors deduced from the area drained vary from 2·1 for Oil City to 0·1 at Weston, and the observed rise between two consecutive days multiplied by these factors can be easily tabulated to exhibit the expected rise at Pittsburg. Mr. Russell has worked out some examples to show the successful application of this method. On February 16, 1891, the calculated height of the stage was 31·3 feet; the observed, 32 feet. On February 6, 1893, the calculated height was 23 feet; the observed, 23·1. It does not appear how far these examples are illustrative of the success attending the general application, but the system seems to leave nothing to be desired. The author takes us regularly down the Ohio River to Cincinnati, Louisville, and Cairo, the junction with the Mississippi, illustrating the modifications which varying conditions may render necessary. The Missouri and the Mississippi also receive their share of attention, and the book forms a very practical guide for those interested in such matters. The value of the whole process rests on the provision of a sufficient number of well placed gauges, and a long series of observations, from which may be learnt the probable behaviour of the river under all circumstances. It is in this direction, apparently, that Mr. Russell, of Sydney, finds his opportunity, and the great mass of facts that he is collecting will be of the greatest use as the colony becomes more thickly peopled. We do not understand that he has yet arrived at the stage of predicting with accuracy and confidence the vertical rise and fall of the rivers over which he watches. His part, if apparently less interesting, is not less useful; and he is to be congratulated on the spread of his system of observations and his successful overthrow of many old theories.

AN ALBUM OF CLASSICAL ANTIQUITIES.

Atlas of Classical Antiquities. By Th. Schreiber. Edited for English use, by Prof. W. C. Anderson, of Firth College, Sheffield. (London: Macmillan, 1895.)

THIS work should hardly be called an Atlas, since, though it contains a vast amount of matter, the disjointed arrangement is by no means that of an Atlas. The abundance of illustrations, however, makes the book exceedingly valuable to the student.

But although there may be, and is the *facundia*, the *lucidus ordo* is frequently wanting. Still, by the help of the excellent trilinqual index, supplied by the English editor, this defect is much remedied.

The book should also be judged by reference to what it aims to be. If considered as a work addressed to artists or specialists, great deficiencies in the technical execution of the plates would have to be complained of; but it should be looked at mainly as a series of rough sketches of ancient life as revealed to us through art, for the instruction of students in literature and commencing archæologists, or as a general book of reference. The above remarks refer entirely to Herr Schreiber's plates; nothing but praise should be accorded to Prof. Anderson as translator and expositor. The aim of the work is sufficiently stated in the preface.

"There springs up a desire for facts—facts as to the life of the ancients, their laws and their customs, their beliefs and their cults. Because no fact is despicable from the point of view of science, we further look into their daily life—the fashion of their dress and their houses, the arrangements of the theatre and the market-place. And since no source of facts can be so perfectly trustworthy as the works of contemporary art, those works gain an interest, arising not merely from their own beauty, but as the crystallisation of the visible life of the people, a mirror of their thought preserved to us like many actual Greek mirrors in the graves of the dead."

The series of plates begins with theatres and acting; and with respect, at least, to Roman or Romanised Greek theatres, they are very fully illustrated, both as regards the fabric and the actor, but there is a remarkable absence of the characteristics of the Greek theatre as distinguished from the Roman, which have been much under discussion of late years. Plate iii., Fig. 3, however, introduces a representation of the raised stage or λογεῖον, which, if the date ascribed to it in the text be accurate, bears strongly against the theory that all the action took place on the level of the orchestra until the raised *pulpitum* was introduced by the Romans. In Plate ix. we see that some of our modern building appliances have been in continuous use since classical times. In Plate x., Fig. 3, after Durm, the contrivance of the wooden blocks and pin in the joints of the columns of the Parthenon is not accurately shown, and it is therefore not surprising that in the text a difficulty is hinted at. The smaller shallow circle was not provided for the purpose of receiving a wooden cylinder to turn in. This was the function of the smaller pin or cylinder of hard wood, which was centred in the square wooden blocks which were fixed in each bed of the joint. The shallow circle in the stone was provided to receive the detritus caused by rubbing the stones together. In the same plate ornament is shown on the echinus of the Doric capital.

Decoration of this member is, to say the least, extremely doubtful. Two valuable plates follow of Olympia, in plan and perspective. The restored view of the Acropolis of Athens, however, is hardly so successful. The drawing is coarse, and it gives a very inadequate idea of the way in which the Acropolis dominates the valley to the south of it. In Plate xiv., Figs. 1 and 2 (the latter from a vase) are interesting from their connection with the Eleusinian mysteries. As many of the illustrations are necessarily taken from vases, it would have been serviceable for beginners if some representation with a short description of different kinds of vases, such as the cylix, the lecythus, &c., had been given. Plate xv. shows that votive offerings of models of diseased limbs and other bodily members, suspended at the altars of favourite saints, had their origin in classical times. In its reference to Fig. 2, of Plate xviii., the text gives a valuable reference to the recent discoveries at the Pantheon, which were lately made under the direction of the French architect, M. Chedanne.

Plate xix., Fig. 15, is interesting as showing that the division of the heavens into different *houses* of the mediæval astrologers had its origin in classical augury. Plates xx. to xxiv. are devoted to athletics. In Plate xxii. are illustrations of the method of throwing javelins by means of the *amentum*, a kind of sling attached to the shaft. Some are shown as being thrown overhand, and others underhand, and a curious method by which aid was given to jumping by means of weights held in the hands. Plates xxvii. to xxxiii. are devoted to games and arena combats. Fig. 4 in the first of these plates, from a wall-painting from Pompeii, is an interesting illustration, described thus in the text: "This painting is unique as a contemporary picture of an historical event. Tacitus ("Annals," xiv. 17) mentions a riot between the people of Nuceria and Pompeii which arose out of a gladiatorial show given by Livineius Regulus. It began with mutual taunts, and then stones were thrown and weapons used. The Pompeians were naturally the stronger party, so that many of the Nucerians were badly wounded, and several slain. As a consequence, Nero stopped the games for ten years. The painting shows the fighting going on in and about the amphitheatre." Fig. 1 in Plate xxxiii., from Brescia, shows that combats with wild beasts were still practised in 530 A.D. in Italy. In Plate xxxiv. we have representations of early Greek warriors and weapons, and also, but of later date, a besieged city from the Nereid tomb in the British Museum, and in Plate xli. a useful diagram showing the arrangement of a Roman camp. In the same and following plates Roman soldiers and their armour are well given, and Greek and other helmets. Young students of Cæsar "de Bello Gallico" will be thankful for the illustrations of the Rhine Bridge in Plate xliv. In Plate xlvi. is the difficult subject of the trireme and its oars. It contains only one original document (Fig. 8), namely, the sculptured relief found near the Erechtheum; the other figures are reconstructions in which the difficulty does not appear to have been grasped. The ancient relief certainly implies oars of different lengths; thus much cannot be controverted, but the only possible means by which the rowers on the different banks could have kept time would have been by an inversely corresponding difference given to the surface of the blades of the oars, which the reconstructions do not show.

Plates xlviiii. to li. are occupied by town gateways and fortifications. Then follow private houses, aqueducts, bridges, baths, and calculating boards. In Plate lxii. ancient sundials, which divided the day from rising to setting sun into twelve hours, irrespective of the difference of their lengths in summer or winter. Then follow various agricultural operations, and in Plate lxvi. a warehouse scene, the weighing silphium, a plant used in medicine, grown in Cyrene; a group of decidedly Egyptian type. Then ovens, Plate lxvii., for baking bread; Plate lxviii., for pottery. From Plates lxix. to lxxvi., various arts and crafts. The triclinium is shown and explained in Plate lxxvii. Then follows jugglery and games. Plate lxxxi. shows bridal scenes, followed by female dresses and costumes. In Plate lxxxvii. is a relief from the arch of Constantine, introducing several details of the Roman forum. Plate lxxxviii. follows with a graphic scene of civic life from a wall-painting of Pompeii. Then school scenes are illustrated with wax tablets and writing materials; there is also a pair of proportional compasses, having much analogy to the instrument in modern use. Plates xcii. A and xciii. tell the "tale of Troy divine," from a relief of the Augustan age, representing the Iliupersis, found near Bovillae; and the work concludes with a very complete series of burial scenes—that is, of interment—for there are no representations of cremation. But notwithstanding this and some other omissions, the hundred crowded plates of this volume, from which we have made only a few extracts, contain a vast store of objects for reference, and they are all very much enhanced in value by the descriptions and notes with which Mr. Anderson has enriched the book.

A DESIDERATUM IN MODERN BOTANICAL LITERATURE.

A Hand-book of Systematic Botany. By Dr. E. Warming, Professor of Botany in the University of Copenhagen. With a Revision of the Fungi, by Dr. G. Knoblauch, Karlsruhe. Translated and edited by M. C. Potter, M.A., F.L.S., Professor of Botany in the University of Durham College of Science, Newcastle-upon-Tyne. (London: Swan Sonnenschein and Co., 1895.)

It is a curious, and not altogether a pleasant reflection, considering the activity which has been displayed by the botanists of this country within recent years, that we should still be largely dependent on foreign sources for our text-books in more than one main division of this particular science. It is doubtless true that the books are sometimes more or less edited, before they are presented to the English student, but still one can hardly help feeling that an entirely home-grown article, if issuing from first-rate hands, would prove a most welcome change.

It is with somewhat mixed feelings, then, that we greet the appearance of Prof. Warming's "Hand-book of Systematic Botany" in its English form. Moreover, we feel a little inclined at the very outset to quarrel with the title of the book before us; a *hand-book* of systematic botany embodying critical morphological discussion, is exactly what is now wanted—something which may be to us what Eichler's celebrated *Blüthendiagramme* was, and

indeed still is, to our German neighbours. But one can hardly allow that the present volume rises above the rank of a text-book, and of these we have plenty with us. Not that it is intended to depreciate the value of Prof. Warming's book; it is chiefly the question whether an increase of this particular kind of book is just now wanted at all, whilst there is no question whatever but that a genuine "hand-book" is very much needed indeed. As far as the work goes it is very good, at least in its manner of dealing with the Angiosperms, but it does not go far enough. Thus the order Cucurbitaceæ, as an example taken at random, is dismissed with something less than four pages, and yet the plants included in this order abound in interesting characters. To treat these and others of a similar nature in a brief dogmatic fashion is to abandon the most interesting side of the subject, to say nothing of the educational opportunities which have been missed. But notwithstanding these features of the work, which, professing as it does to be a hand-book, appear to us to be serious defects, we readily admit that, taken as a whole, the account given of the flowering plants is one of the best existing in the English language. The lower groups of plants are less satisfactorily dealt with. In the Fungi, the general method of arrangement followed is that based on Brefeld's researches, but the difficulties connected with *Eremascus* are not touched upon. It may be doubted whether the student will gain a very clear idea of *Oidia*, which, he is told, must be distinguished from "true chlamydospores." The definition runs thus: "The former (*Oidia*), are more simple, the latter are somewhat more differentiated form of carpophore fundaments, which serve for propagation in the same manner as spores." But exactly wherein the difference really consists we seek in vain to find. A purist might object to the expression "brand"-fungi, which is used instead of the more familiar one of smut-fungi; a practical farmer, in this country at least, would also probably smile at the description given of the method of application of blue vitriol as a preventative of the disease caused by these organisms in cereal crops.

The treatment of the Muscineæ strikes us as far too cursory, especially in regard to the considerable amount of work recently done in connection with these plants. The brief statement of Celakovsky's view as to the homology of the moss sporogonium is only calculated to confuse the mind of a student by introducing purely idealistic notions, and its value without a full explanation is absolutely inappreciable. The catalogue of "orders" of mosses, given on pp. 196–197, is also particularly depressing.

The treatment of the vascular cryptogams is decidedly weak, and this is the more surprising, considering the activity which has long been displayed in the investigation of this division of plants. The general description of the embryo, given on p. 201, only applies to a few families, and is not by any means true for most of the groups. Again the usual mistake is made as regards the sporangium of Isoetes, which is stated to be divided into "compartments one above another"; the fact, of course, being that it is not divided into "compartments" at all, as an inspection of a tangential section will suffice to show.

It is surprising, in a work issued in 1895, to find the old erroneous description of the germination of the gymnosperm pollen-grain still maintained. We note, however, with satisfaction that a popular mistake (which appears also in the text) is corrected in an editorial note, in which it is rightly stated that Cycads commonly *do* branch in a state of nature.

From what has been said, it will be clear that the treatment of the lower plants is inadequate, and it is to be regretted that Prof. Potter did not see his way to using his editorial discretion more freely. It is, however, easy to find fault with most books; but we have already said that, as regards the latter half of the volume, it is deserving of commendation, and we may add that it is well illustrated, and that, further, it contains, in the form of an appendix by Prof. Potter, a brief account of the chief methods of classification which have been used in arranging the members of the vegetable kingdom.

OUR BOOK SHELF.

The Noxious and Beneficial Insects of the State of Illinois. Eighteenth Report of the State Entomologist. Seventh Report of S. A. Forbes. For the years 1891 and 1892. (Springfield, Ill., U.S.A., 1894.)

THIS report is mainly devoted to insect attacks affecting "Indian corn" (sometimes known with us as "maize," in the U.S.A. shortly as "corn"), and coming from the trustworthy and well-qualified pen of Prof. Forbes, will be of much service in the country of the crop dealt with, and, in points noticed regarding such of these "pests" as are of very similar habits with our own, may be studied here with much advantage.

The "Monograph of Insect Injuries to Indian Corn" extends to 165 pages, dealing with insects of very various kinds, including amongst them what, without entering here on their scientific appellations, may be generally described as ants of various kinds; beetles, including allies of our turnip flea beetle, wireworms, with click beetle parents, and chafers, with their grubs (truly noted as "the immemorial enemies of agriculture on both sides of the Atlantic"); aphides, or plant lice of various kinds, and some other insects.

The information is the result of ten years' investigation of the economic entomology of the Indian corn plant by the official entomologist of Illinois, joined to such additions from published matter as it appeared desirable to embody with his original observations; and in the words of the writer, whilst a portion of the information is such as he hopes will be "intelligible and practically useful to the actual tiller of the soil," he has also incorporated with this, for "the special benefit of the entomologist, more detailed and thorough-going discussions of the insects themselves, and of their life-histories, habits, and injuries, together with descriptions of the species in all stages as yet recognised."

These minute descriptions, especially of the early stages (so important to the economic entomologist, and so difficult, too often, to obtain) in themselves give the work a high value, and in the practical part there is much to be studied with great benefit. To give a single instance—the indifference of wireworms to various kinds of poisons prepared for their consumption on seed placed for their use (p. 49).

The report is greatly to be recommended to the study of economic entomologists, and its value is added to by fifteen well-executed full-page plates of many of the insects referred to, also by an exhaustive index of thirteen pages, so complete and well arranged as in some instances almost to give headings for a life-history of the insect referred to.

F. A. O.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Origin of the Cultivated Cineraria.

REFERRING to records of the history of cultivated Cineraria, I found (1) that considerable sports, or seedlings presenting notable and striking variations, arose in the early days of the "improvement" of the Cineraria; (2) that there is evidence that the improved varieties were of hybrid origin. I concluded, therefore, that Mr. Dyer's statement that our Cinerarias have been derived from *C. cruenta* "by the gradual accumulation of small variations" was misleading in two respects. As we have now had the benefit of a fuller statement of Mr. Dyer's case, I ask leave to explain why it is that I still hold to my original conclusion.

Meanwhile, however, Prof. Weldon, intervening, has offered an apparently sustained criticism of my evidence, which to those no better prepared may have a formidable look.

We will first examine some of Prof. Weldon's minor points. In preface let me say that I do not contend that *no* sports or named varieties have ever been believed to have arisen directly from *cruenta*, or from plants so-called (for, as Willdenow hinted,[1] the name may have been misapplied to hybrids in the past as now); and, indeed, I gave Drummond's words that his *cruenta* "sported freely from seed."

Something was made also of the wise caution which Burbidge gives in his general "Introduction" (p. 118), putting the reader on his guard against specific assertions as to the origin of hybrids. I mention, therefore, that I have received from Mr. Burbidge a letter warmly supporting the opinion given in the body of his book (p. 249) that the Cinerarias are of hybrid origin.

But now for what Prof. Weldon takes to be the real strength of his attack. He says that I omitted passages proving that according to contemporary opinion many of the named varieties cultivated between 1838 and 1842 "were not hybrids," but were "believed to be pure-bred *cruenta*." Upon what grounds this statement has been made, the reader shall now learn, not perhaps without astonishment.

The passage on which he chiefly relies is taken from Mrs. Loudon's article (*Ladies' Mag. of Gard.*, 1842, p. 111), to which I referred for the statement that in the writer's opinion the first important departure in the improvement of the Cineraria was made when Drummond hybridised *cruenta* with *lanata*. She goes on to say that, "since that time, numerous experiments have been made and hybrids raised" from several species. Next, that "some of the most beautiful Cinerarias now in our greenhouses, have been raised by Messrs. Henderson, Pineapple Place, particularly *C. Hendersoni* and the King, both raised from seeds of *C. cruenta*." This is the passage I omitted. Prof. Weldon says that this "passage clearly shows that in the writer's [Mrs. Loudon's] belief, the hybrids produced by Drummond and others, had not given rise to two at least of the named varieties of her time," and that she believed the King and *Hendersonii* to be descended from *cruenta* alone. This Prof. Weldon tells us is certain

Now, were we even bounded by the limit Prof. Weldon has set to his own researches on this question, we might hesitate to assume that whenever it is not expressly declared that a plant is a hybrid, we may be sure that the author thought it was pure-bred. As it happens, however, I can meet the charge with a weapon sturdier than the fine point of "dialectic." The answer is quite simple and curiously complete.

I shall now prove that both the King and *Hendersonii* were well known as hybrids both to Mrs. Loudon and to others. Let me point out:

(1) That the words say that the King and *C. Hendersonii* were raised from *seeds* of *cruenta*: as to the *male* parent, nothing is there said.

(2) That even if the evidence ended here, a discriminating reader might have suspected (what I shall presently prove) that Mrs. Loudon's *particular* statement about the King, *Hendersonii*, &c., is merely meant as an expansion of her previous *general* statement that since Drummond made his beginning numerous hybrids had been raised.

(3) That, were the matter doubtful, other passages in Mrs. Loudon's works prove this to be her meaning. For in *Ladies' Comp. to Flower-Gard.*, 1849 (s. v. Cineraria), she states, "the finest hybrids are *C. Waterhousiana*, *C. Hendersonii*, and the kind called the King." Again, in *Ladies' Flower-Gard.*, *Greenho. Plts.*, 1848, p. 178, speaking of the woolly leaves, &c., of *lanata*, she says, "these peculiarities are found in all the numerous hybrids that have been raised from it. Perhaps the most ornamental of these is the hybrid called the King."[1] Of this, therefore, I presume Mrs. Loudon believed *lanata* to be the father, *cruenta* the mother.

(4) Lastly, that in order to have learnt that the King and *Hendersonii* were "between 1838 and 1842" considered to be hybrids, Prof. Weldon need not have gone far. He tells us he has read the articles on *C. Webberiana* (*Paxt. Mag.*, 1842, p. 125) and on *C. Waterhousiana* (*ibid.*, 1838, p. 219), to which I gave references. Will it then be believed that in the first of these very articles *the King is referred to by name as a notable hybrid*: and that in the second article, "*C. cruenta*, var. *Hendersonia*" is with others named as *one of* "*the hybrids raised and grown by Messrs. Henderson, Pine-apple Place!*"

I do not know if it is wished that I should further refute Prof. Weldon's charge of "want of care in consulting and quoting the authorities." I am not unprepared to do so. I shall be glad to explain why Mrs. Loudon was probably right in substituting the name *tussilaginis* for *tussilagofolia*: to show why *Webberiana*, price 10s. 6d. (*Gard. Chron.*, 1842, p. 665), may be called a striking advance on its contemporaries, price 2s. 6d. (*Gard. Chron.*, 1842, p. 633), together with many other matters not yet treated of in this discussion.

My first objection to Mr. Dyer's statement was taken on the ground that there is historical evidence that sports, or seedlings presenting notable variations, occurred in the early days of the improvement of the Cineraria. To this, after reading his letters with great care, I do not find any specific answer. He tells us that the history as he gave it would be "in accord with general horticultural experience." It obeyed then a rule to the proof of which exceptions are indeed not lacking. He says, further, that to improve a plant the only safe way is by "selecting the minutest trace of change in the required direction," and "by patiently and continuously repeating the operation." Now this would be all very well if we knew nothing about the origin of the Cineraria; but against the evidence that seedlings presenting striking variations did as a fact arise, and against the historical evidence that Cinerarias, much as we know them, did as a fact come into existence within some twelve years, such *à priori* expectation is worth nothing at all.

To my second objection, that there is evidence that the chief start in the improvement of Cinerarias came as the result of hybridisation, Mr. Dyer has given more attention. He proposes to meet it by rejecting the whole of the historical evidence as unsound, and preferring the conjecture to which he says an inspection of the modern plants has led him. The historical evidence is to go because we are told certain horticulturists are ignorant men. I premise that this is not a principle which Darwin, whom Mr. Dyer would claim as his master, would have endorsed.

But before judging, let us try to consider what was the objective evidence on which the gardeners made up their minds that the new Cinerarias were hybrids. I may illustrate this by reference to a seedling now growing in the Cambridge Botanic Garden, to which Mr. Lynch, the curator, kindly called my attention. The case is of special interest in view of Mr Hemsley's objection that it requires skill and care to raise a hybrid in the Compositæ. It was with regret I learnt that this careful writer was not with me in this matter.

This seedling was raised from a seed of our plant of *lanata*, which was received from and is exactly similar to those at Kew.[2]

[1] He says ("Enum. Pl. Berol.," 1809, p. 893) that Cinerarias are grown in gardens under the name *cruenta*, though really very different from it, having flowers almost like those of *lanata*. To these he gave the name *C. hybrida*. Moreover, from Bouché's experiment, we know that the seedlings of this *C. hybrida* were very variable.

[1] So famous a hybrid was the King, that I regret that I did not mention it in my first letter. I did not do so, as I found no coloured plate of it. Mr. John Fraser, of South Woodford, kindly informs me that he remembers it as the best of the woody sorts formerly grown. Its flowers were about the size of a penny, rays white tipped with purple, leaves downy and of a silvery hue on the underside. Its seedlings were unreliable.

[2] There labelled *Héritieri* (of DC.=*lanata*, L'Hér.). I note that though otherwise agreeing exactly with the *lanata* described by L'Her., de Candolle and Webb, the inflorescence of these plants differs, being a loose corymb of some twenty heads, instead of the single flowered peduncle (*rami semper monocephali*, Webb) of the old authors. Whether this variation is known in wild plants, I cannot tell.

In habit and size our seedling is not at all like *lanata*, but might be taken for a poor specimen of the common Cinerarias. In several characters it is intermediate between *lanata* and the latter. The stem is rather woody, less so than in *lanata*, but it is thick like those of garden kinds: petioles like *lanata* in having no auricles: leaves, nevertheless, large like those of garden kinds, the backs very woolly, but largely purplish, as in many cultivated sorts. Now this plant must be either (1) a sport from *lanata* in the direction of the garden forms, or (2) an accidental hybrid between *lanata* and one of the cultivated kinds growing in the same house with it (we have no others). The latter seems more likely—an opinion in which Mr. Lynch fully concurs.

Similarly Bouché (*Wittm. Monats.*, xxii. p. 298, orig. not seen, quoted from Focke, *Pfl. Mischlinge*, 1881, p. 201) says that a hybrid between *C. Webbii* (Schlz. Bipont.) and *cruenta* arose in the Berlin Botanic Garden as the spontaneous product of these species growing side by side.

It was, I think, on evidence like this that the parentage of the older hybrids was conjectured; but that Drummond and Henderson certainly—and possibly others—did make definite efforts to hybridise, cannot on the evidence be doubted. That these efforts went no further than the brushing of pollen of some species upon the flowers of others, I fully believe, and that on such evidence the *precise* parentage cannot be assigned is obvious. Nevertheless distinct seedlings resulted. In a few years, as the writer in *Paxt. Mag.*, 1842, p. 125, says (in an article urging to fresh efforts in crossing), this hybridisation "was the means of creating quite a novel and superior race." There were the new plants: how had they arisen? Those who doubt that these new kinds were hybrids must choose the other horn of the dilemma, and accept them as sports pure and simple.

That the historical records may contain errors, I am fully aware; but if they cannot be accepted in detail, should they be altogether rejected? We might perhaps reserve a doubt whether the King came precisely from pure *cruenta* fertilised by *lanata*; whether *cruenta* var. *lutea* was a hybrid between *cruenta* and *populifolia* (as de Candolle surmises); whether *Waterhousiana* was the offspring of true *cruenta* and true *tussilaginis*; whether Mrs. Loudon's statement that the species used were *cruenta*, *lanata*, *aurita*, *tussilaginis*, and *populifolia*, or Moore's belief that *cruenta* and *tussilaginis*, with perhaps *Héritieri* (= *lanata*), *maderensis* (= *aurita*), and *populifolia* ("Cross and Self-Fert.," p. 335, *note*), or Otto's similar declaration (Regel's *Gartenflora*, 1857, p. 66), or that of the *Jour. d'hort. Gand*, 1846, already given, should each be taken without hesitation as full and complete statements of the whole truth, but that they contain a substance of truth is hardly in question.

Against this Mr. Dyer offers nothing but an opinion derived from an inspection of certain modern plants. He who has confidence in the results of this method must suppose our knowledge of the laws of inheritance to be much more complete than I believe it to be. It is not the method Darwin used. Take a well-ascertained case. Who would know from inspection of the Himalaya rabbit that it came directly from the Silver-greys or Chinchillas? (See *Animals and Pl.*, i. p. 113.) It is unlike them, is of sudden origin, and yet breeds true.[1] To suppose that in cross-bred offspring given characteristics of the parents must be found, is to assume the precise question which in a discussion of organic stability is at issue. Let it be remembered that on the hypothesis of hybrid origin for our Cinerarias it is supposed that they result from several species and varieties, crossed not once only, but many times, in wholly irregular ways. Can it be seriously expected that any special resemblance to a given ancestor should be still traceable?[2]

My position then is this. We heard Mr. Dyer's statement; turning to the literature I found an entirely different account, borne out by copious and on the whole fairly attested evidence, pointing irresistibly to the conclusion that the Cinerarias are [illegible] which hybridise freely, and that our modern forms have [illegible] through such hybrid unions.

[1] [illegible] Mr. [illegible] Royal, [illegible] grower, and to [illegible] several interesting points, and especially for the [illegible] featured [illegible] in his strain. [illegible] introduced some plants of a French strain. After this, [illegible] of the cross between [illegible] the French kinds, [illegible] seeders themselves, [illegible] strongly that they have to be kept in a house [illegible] to the exclusion of everything else.

[2] [illegible] Mr. Dyer [illegible] I have assumed, what I [illegible]

Mr. Dyer has well said that "if you take any statement that Mr. Darwin has put forward, you may feel assured that behind it is a formidable body of carefully considered evidence not likely to be upset." By the courtesy of an opponent I have been directed to a passage in "Cross and Self-Fertilization," 1876, p. 335, where (before describing experiments showing considerable self-sterility in the garden Cineraria) Darwin gives this definition of his material, "*Senecio cruentus* (*greenhouse varieties, commonly called Cinerarias, probably derived from several fruticose or herbaceous species much intercrossed*")." It seems, therefore, that in this matter also Mr. Darwin has, to use Mr. Dyer's words, "squeezed out" of the evidence "all that it would profitably yield."

Here I would fain leave the subject. But perhaps it may be suggested that though Darwin's Cinerarias were probably hybrids, our Cinerarias may not be their descendants. Such a suggestion involves the supposition that in some hidden place there was a thin red line of pure *cruenta* waiting for the moment when it should oust the hybrids. If this be seriously suggested, I shall ask where such a strain was kept, and what steps were taken to preserve its purity. In view of the evidence that chance blendings occur freely, to keep a pure strain would require some care. Until this has been proved, we shall not, I think, be wrong in supposing that each grower worked on the material his predecessors had created, and that our Cinerarias are the lineal descendants of the hybrids raised in the first half of this century.

In the course of this discussion, Mr. Dyer has treated me to some hard words, which I do not particularly resent. Whether I have deserved them is not, perhaps, for me to judge. But I will ask Mr. Dyer to point out when, on being asked for the facts upon which I have based a view, I have replied that that was a "matter for future collection." The facts I have been able to collect may be few, but by a study of the writings of my antagonists, I have not been able to add materially to their number.[1]

W. Bateson.

St. John's College, Cambridge, May 26.

It has been pointed out to me that my remarks on Mr. Bateson's account of the Cineraria have been interpreted in a sense of which I did not dream when I wrote them.

I wish, therefore, to say that, although I do not believe Mr. Bateson's reading of the passages I quoted to be the true one, yet I have never questioned his sincerity in suggesting it, and I am pained to find that I have seemed to do so.

May 24. W. F. R. Weldon.

Boltzmann's Minimum Function.

I gather from Mr. Culverwell's last letter (Nature, April 18), and Mr. Bryan's (May 9), that we may regard the following conclusion as established, namely, the proof of the H theorem, for any system depends on a certain condition (A) being fulfilled among the coordinates and momenta of the molecules forming the system. Considering these as elastic spheres, and using Dr. Watson's notation, $f dp_1 \ldots dq_3$ is the chance that a sphere shall have for coordinates and momenta $p_1 \ldots p_1 + dp_1$, &c., and $F dP_1 \ldots dQ_3$ the chance that another sphere shall have $P_1 \ldots P_1 + dP_1$, &c. The condition required is that f and F are independent, even for two spheres on the point of collision.

Otherwise we may express it. Let there be n spheres in space S. Let us suppose Mr. Culverwell to assign to each its position at time $t = 0$, and Mr. Bryan to assign independently to each its component velocities. Then the condition A is fulfilled when $t = 0$.

We can then prove that when $t = 0$ $\frac{dH}{dt}$ is negative, or, as Herr Boltzmann would have us say, is more likely to be negative than positive.

Now arises a question which seems to me to deserve consideration. Assuming our system to be finite, and to be left to itself unaffected by external disturbances, does it necessarily

[1] It has been impossible for me to incorporate in this letter all the mass of information which has been most generously sent me by correspondents since this controversy began. It is suggested that I should point out that Mr. Dyer's use of the word "feral" to mean "wild" is not usual. A correspondent tells me that it was probably first used in the special sense of "run wild" by Hamilton Smith, *Nat. Libr.*, *Mammalia*, 1839, ix. p. 92. It has since been so used by many authors, especially Darwin, *An. and Pls.* i. p. 117, &c.

follow that condition A, being now satisfied, will continue to be satisfied for all time?

If the answer be Yes, then of course $\frac{dH}{dt}$ will continue to be negative, until at length H reaches its minimum, and the system attains to perfection in the form of the Maxwell-Boltzmann law. If that is necessarily the future of our system, then, as Dr. Watson says, the Maxwell-Boltzmann law is not only a sufficient, but a necessary condition for permanence.

I am not aware, however, that this doctrine of (so to speak) final perseverance has ever been proved to be true. I do not think it can be regarded as axiomatic.

It seems to me that if we are to make our finite system reach perfection with any certainty, we must resort to the principle to which I appealed in my first letter on this subject—that every material system is constantly receiving disturbances from without, the effect of which is to keep condition A in working order, and so to make $\frac{dH}{dt}$ generally negative. Otherwise we must regard our system as only part of an infinitely extended system, the parts of which, when not too distant, mutually influence each other. S. H. BURBURY.

Research in Education.

IT is quite unnecessary for Mr. D. S. T. Grant to suffer "dialectic annihilation" (see p. 5) in order to discover Prof. Armstrong's definite scheme of scientific education, inasmuch as in 1889-90 such a scheme was published by a Committee of the British Association, of which Dr. Armstrong was an active member.

As I believe many schools are still waiting for evidence as to the practicability of the scheme before adopting it, I venture to quote my own experience. I have been engaged for some time in practically applying this method to the teaching of girls of various ages, and am in a position to state that the scheme is perfectly workable.

It is not, of course, suggested that students should find out every known fact in chemistry or physics by a process of personal research—life is not long enough; but, if their early training be on these lines, they are in a much better position later on to accept, or if necessary reject, the work of others. A scientific method of thinking is of far more value than an accumulation of facts, and so it is extremely important that children should begin this kind of work before their logical perceptions have become obliterated by a continued application to irregular verbs. The problems set to young children are naturally of a very simple nature, and I do not leave the girls to themselves to "struggle to the truth by a process of trial and error." I state the problem to the class, and I usually find the girls have plenty of suggestions to offer as to its solution; these suggestions I criticise, and as soon as a practicable method has been found, the girls work it out for themselves. The early problems involve measurements of length, area, volume, and weight, and naturally the use of each new instrument is explained and illustrated. Simple physical problems follow these, such as experiments on relative density, and thus children are led to realise and appreciate the common properties of matter. After this training they are much more ready to solve elementary chemical problems. Certainly they could never work long enough to discover Dalton's laws for themselves, but they can quite appreciate classical experiments, and see how theory supplies an explanation of the facts. I am quite aware that if children are to work in this way they cannot be expected to sit still in their places with the look of passive receptivity on their faces, which is conventionally regarded as the proper appearance of well-disciplined scholars. They must move about, and should be encouraged to talk to each other about their work. I am convinced that a class of about eighteen is quite large enough if sound work is to be done; and if at any time their excitement becomes noisy, I find that a threat of numerical problems is quite sufficient to make them continue their practical work more peacefully.

It seems to me that physiology and hygiene, as usually taught in girls' schools, are absolutely pernicious and unscientific. Girls learn a list of the circulatory organs as they do the kings of England, and with less advantage. It would be considered criminal in them to doubt any of the facts in their books, although many are wrong, and yet, I take it, scientific training misses a great point if it does not engender a wholesome spirit of doubt. But the worst feature of all is the way in which girls are taught certain things in theory of the meaning of which they have not the faintest notion. They can tell one that water is H_2O, but the real significance of the symbol is perfectly unknown to them, and of course they are not able to understand it without some chemical training, in spite of the fact that some schools consider themselves very advanced and practical if the lessons are emphasised by the burning of hydrogen and the manufacture of oxygen. Numberless examples of similarly useless facts could be quoted, which are learnt under the name of hygiene—teachers, parents, and girls vainly believing that this is science. But all these facts are forgotten as soon as some examination is passed, and nothing is left behind; whereas a logical system of scientific training produces an effect on the mind which it is impossible to overrate. Surely the aim of education should be to produce not people who are full of facts, but those who can make the best use of the brains they possess, who are clear-headed, and able both to perceive and take advantage of opportunities that may be afforded them.

Central Foundation School for Girls, London. L. EDNA WALTER.

The Bibliography of Spectroscopy.

IT will be within the recollection of many of your readers that, in the year 1879, a committee was appointed by the British Association to report on the state of our knowledge of spectrum analysis, and I was asked to undertake the preparation of a bibliography of spectroscopy from the year 1870. It was not thought necessary to begin at an earlier date, for a bibliography of the subject is to be found in Roscoe's "Spectrum Analysis." With the help of several members of the committee, lists of spectroscopic papers were prepared, and appeared in the British Association Reports for 1881, 1884, and 1889. In that year Mr. H. J. Madan kindly consented to join the committee, and as he was then resident in Oxford he was able to afford valuable assistance in checking the references, and the section of the list that was published last year is almost entirely his work, as I had found it impossible to spare the time to go to London to look up the references in the libraries. Mr. Madan is now living in Gloucester, and therefore out of reach of scientific libraries; he has, notwithstanding, shown his interest in the subject by making frequent visits to Oxford and London to continue the work. He finds, however, that the work is hardly practicable for one so far removed from the great centres; and my object in writing this letter is to ask if any one will volunteer to relieve him from this duty—that is, on the supposition that the list is of real use to workers on spectroscopic subjects. Many of the readers of NATURE will be able to give valuable opinions on this matter, and probably to suggest improvements in the manner in which the list is drawn up.

It has been suggested that the four sections of the list should be rearranged and published as one continuous catalogue. The advantages of this for the purpose of reference are obvious; but from an estimate obtained last year, the cost of printing would not be less than £100. Dr. Tuckermann also very kindly proposed that the "Bibliography of Spectroscopy" drawn up by him and published by the Smithsonian Institution in 1888, should be incorporated with the British Association lists; this would very materially increase the expenditure.

Mr. Madan is quite willing to undertake gratuitously the literary work involved in the collection and rearrangement of the various sections. But the expense of publication is so great that the British Association can hardly be expected to bear the whole of it, although it is quite likely that a liberal grant might be made. Probably also grants might be obtained from other societies interested in the work, if it appears that the catalogue would be of special utility to those engaged in research. The balance might be met by a moderate charge for each copy sold.

Cooper's Hill, May 15. HERBERT MCLEOD.

An Aquatic Hymenopterous Insect.

NO doubt many of your readers are aware that, in 1863, Sir John Lubbock gave an account of an extraordinary hymenopterous insect which he had observed swimming in a basin of water taken from a pond at Chislehurst. Another observer (Mr. Duchess, of Stepney) had also found a single specimen about the same time; then, in 1881, Mr. Bostock found one in some pond water at Stone, Staffordshire, since which time it does not appear to have been recorded by any one. I have searched many ponds for it year after year, but without success.

On Saturday, May 4, the Quekett Microscopical Club held one

rs excursions in search of pond life, the neighbourhood visited being Totteridge and Mill Hill. Mr. W. Burton obtained two small phials of the water for examination, and the first pipette of water turned out into the trough contained a minute fly, which Mr. Burton kindly brought to me, when I immediately identified it as the *Polynema natans* (Lubbock, *Trans. Linn. Soc.*, vol. xxiv. 1864, p. 135, plate 23).

As this capture was, for the fourth time, the result of chance, Mr. Burton and I set out (May 6) to search for more specimens. After dipping our nets in and carefully examining the contents for over two hours, my patience was at last rewarded by seeing a beautiful female, struggling to free its wings from the mass of minute vegetation gathered in the dipping net. After a few hours more search, I found four males, which, together with the female, I transferred to an observation tank, where all soon disported themselves in the liveliest manner, swimming, or rather flying, under water for over four days, during which period they did not, to my knowledge, once leave the water. I have since obtained others, which are under close observation, and in course of time I hope to trace out their life-history.

Perhaps, owing to the microscopic dimensions of many of the *Mymaridæ* (Haliday), very few entomologists have paid any attention to this most interesting and fascinating family of beautiful "Fairy Flies," to whose industry we are no doubt largely indebted for our freedom from "blights" of many kinds. They are, indeed, mere specks, scarcely visible to the eyes of ordinary folk, and yet they have their place in nature.

I am inclined to think that when the type collection of the *Mymaridæ*, made by the late Mr. Haliday, has been thoroughly examined, this name *Polynema natans* will have to give place, so far as the genus is concerned. I hope that before very long we shall have figures of all the genera in this most interesting group.

FRED. ENOCK.

Halley's Chart.

I HAVE been much interested with the letter of Dr. L. A. Bauer in your last number, as I happen to possess a map, or chart, bound up with a number of Dutch, German, and French maps of the end of the seventeenth and the first years of the eighteenth centuries. The latest map with a date is 1704. This English map is evidently the same as 974 (4) mentioned by Dr. Bauer. It is entitled "A new and correct chart showing the Variations of the Compass in the Western and Southern Oceans, as observed in ye year 1700, by his Maties command by Edm. Halley." The dedication reads as follows, in Latin: "Majestati semper Augustæ Gulielmi III. D.G. Magnæ Britanniæ Fra. & Hib. Regis Invictissimi. Tabula hæc Hydrographica Variationum Magneticarum Index. Devotissime Consecratur a Subdito Humillimo Edm. Halley." At one side of the map is the following: "The curve lines which are drawn over the seas in this chart do show at one view all the places where the variation of the compass is the same: The numbers to them show how many degrees the needle declines either Eastwards or Westwards from the true North: and the double line passing near Bermudas and the Cape de Virde isles, is that where the needle stands true without variation."

The chart is in excellent condition, but has no name of printer on it. The only indication is "J. Harris, Sculp." The course of a vessel going from and returning to England is clearly marked.

THOS. WARD.

Northwich, May 27.

ON THE LINE SPECTRA OF THE ELEMENTS.

I THINK Lecoq de Boisbaudran was the first who called attention to the fact that the line spectra of the elements are by no means so irregular as they seem to be at first sight. He discovered the similarity in the spectra of the alkalies and alkaline earths, and pointed out how the lines in the spectra of these two families seem to be shifted towards the less refrangible side with increasing atomic weight. Mascart, in 1869, found two strong triplets of lines in the ultra-violet spectrum of magnesium, similar to the strong green triplet so prominent in the solar spectrum. He says: "Il semble difficile que la reproduction d'un pareil phénomène soit un effet du hasard; n'est-il pas plus naturel d'admettre que ces groupes des raies semblables sont des harmoniques qui tiennent à la constitution moléculaire du gaz lumineux? Il faudra sans doute un grand nombre d'observations analogues pour découvrir la loi qui régit ces harmoniques." But the wave-lengths corresponding to these rays were then not accurately known, and so the most interesting feature concerning the oscillation frequencies, or the number of waves which pass any fixed point in unit of time, remained unnoticed. It was later on shown by Hartley, that the differences between the wave-numbers of the three lines seem to be the same for all the triplets. This constant difference of wave-numbers repeated in a number of doublets, of triplets, and of more complicated groups of lines, has now been observed in the spectra of many elements. There are repetitions of doublets in the spectra of sodium, potassium, rubidium, cæsium, copper, silver, aluminium, iridium, thallium; of triplets in the spectra of magnesium, calcium, strontium, zinc, cadmium, mercury, manganese, and of more complicated groups of lines in the spectra of tin, lead, arsenic, antimony, bismuth. In all these cases the differences seem to be absolutely constant. For, notwithstanding the great accuracy with which Rowland has taught us to determine the wave-lengths, the law holds good. As an example, I give the list of doublets in the spectrum of thallium, according to Prof. Kayser's and my determinations. The number of waves passing a fixed point in unit of time, is equal to the distance the light travels in unit of time divided by the wave-length. If we measure the wave-lengths in vacuo, the distance the light travels is the same for all rays. We may then choose as unit of time, the time that light requires to travel one centimetre, so that the wave-number is simply equal to $1/\lambda$, λ being the wave-length in vacuo, measured in centimetres. In this manner, we get rid of the necessity of settling the velocity of light, which as yet has not been measured with anything like the accuracy with which the wave-lengths are known.

$1/\lambda$	Difference.	Estimated limit of error.
18684·2 26476·6	7792·4	0·32
28324·1 36117·1	7793·0	0·63
30952·1 38744·8	7792·7	0·74
33569·4 41365·1	7795·7	4·90
34217·7 42010·2	7792·5	0·90
34526·2 42321·4	7795·2	4·50
35372·1 43164·7	7792·6	1·20
36879·2 44671·0	7791·8	2·40
37503·0 45293·8	7790·8	2·70
38305·0 46096·8	7791·8	6·80
38663·3 46452·4	7789·1	7·30
39157·0 46947·3	7790·3	8·20

The mean of the twelve differences, assuming their weights to be inversely proportional to the square of the estimated limit of error, is 7792·5. When the wave-lengths are not reduced to vacuo, the differences are also very nearly constant, because the reduction alters them all nearly by the same amount. But it was a source of satisfaction to me, that the reduction brought all the deviations from the mean value well within the limits of error, whereas without the reduction the second difference had been just beyond the limit. These twelve doublets do not comprise half the number of wave-lengths that have been

observed in the spectrum of thallium. But, nevertheless, I think any one will agree that their numerical relation is no chance coincidence. Let us now make a drawing of these doublets to the scale of $1/\lambda$. Evidently the twelve first lines will give the same picture as the twelve second lines. Let us therefore, to simplify matters, only plot down the twelve first lines. At first glance this does not show any remarkable regularity; but if we drop the fourth and sixth line, we can arrange the rest in two series, as is shown in Fig. 1, both rows resembling the series of lines in the spectrum of hydrogen, which are so accurately represented by Balmer's formula. Recurring now to the general list of lines observed in the spectrum of thallium, we find that all five lines of the first series are accompanied on their more refrangible side by strong and easily reversed lines, while the lines of the second series are single. Thus not only does the symmetry of the drawing justify the separation of the lines into two series, but their that only four lines out of sixty do not show any signs of a system according to which they are grouped.

I have given this detailed account of the arc spectrum of thallium only as an example; for I might describe many more spectra that show a similar regularity in the distribution of many of their lines. But there is another interesting point. The distribution of lines in the spectra of chemically related elements shows evident signs of a common plan. I will, for instance, describe the series of triplets in the spectra of magnesium, calcium, and strontium.

The most prominent lines in the visible spectrum of magnesium are the three green lines 5184, 5173, 5168 10^{-8} cm. forming the group *b* in the solar spectrum. In the ultra-violet, at least ten repetitions of this group have been observed, two more being doubtful on account of their weakness and nebulosity. The differences of wave-numbers have been found to be the same in all the groups,

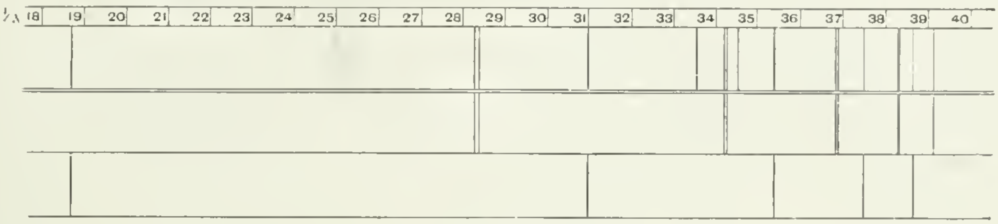


FIG. 1.

appearance teaches us the same. We may expect to find that a formula similar to that of Balmer connects the lines of each of these two series. Indeed, for suitable values of A, B, C the wave-numbers may be calculated from the formula,

$$A - Bn^{-2} - Cn^{-4}$$

A and B having nearly the same values for both series, and n assuming the values 4, 5, 6, 7, 8 for the first, and 3, 4, 5, 6, 7 for the second series. One may state the formula thus: if the wave-numbers be plotted as ordinates to the abscissæ $1/3^2$, $1/4^2$, $1/5^2$, &c., the points form a parabola. If we now go on substituting for n the subsequent whole numbers, we find that all these calculated wave-lengths really exist in the spectrum. But they are weaker and weaker for higher values of n. Prof. Kayser and I have been able to observe the wave-lengths calculated by the formula of the first series for $n = 9$, 10, 11, 12, 13, 14, 15, 16, and by the formula of the second for $n = 8$, 9, 10, 11, 12, 13, 14, 15. We searched for the second members corresponding to these lines, but could not detect them, owing to our plates not being sensitive enough for wave-lengths as small as 2100. However, they have nearly all been observed by Cornu. If we accept Cornu's wave-lengths, we now have two series of doublets of equal width in the scale of wave-numbers, and a drawing of them shows a remarkable symmetry (Fig. 2). The drawing comprises 47 out of 60 lines that constitute the arc spectrum of thallium, including Cornu's observations. Of the thirteen lines left, five are the strong lines, mentioned above, that accompany the five first lines of the first series on their more refrangible side. The distance between each line and its companion grows smaller as we advance to smaller wave-lengths, the last distance being not more than 0·45 10^{-8} cm. It seems probable that the next lines also have their companions, which, however, so closely coincide with them that it has not been possible to separate them. So there are only eight lines left, the positions of which do not enter into the general plan of the spectrum. Among these eight lines there are two doublets of the same difference of wave-numbers as all the other doublets. Both widen asymmetrically—one towards the more refrangible side, the other to the less refrangible side. Thus we may say

as may be seen from the following list. The wave-lengths have not been reduced to vacuo, because all three lines of one group are so near one another that they would all be changed by nearly the same amount, so that the differences of wave-numbers would practically remain the same.

λ		$1/\lambda$	Difference.
5183·84	... ...	19290·7	40·9
5172·87	... ...	19331·6	19·9
5167·55	... ...	19351·5	
3838·44	... ...	26052·2	40·7
3832·46	... ...	26092·9	20·1
3829·51	... ...	26113·0	
3336·83	... ...	29968·6	40·9
3332·28	... ...	30009·5	19·8
3330·08	... ...	30029·3	
3097·06	... ...	32288·7	40·9
3093·14	... ...	32329·6	20·5
3091·18	... ...	32350·1	
2942·21	... ...	33988·1	40·9
2938·67	... ...	34029·0	19·5
2936·99	... ...	34048·5	
———		———	
2848·53	... ...	35105·8	20·0
2846·91	... ...	35125·8	
2781·53	... ...	35951·4	41·1
2778·36	... ...	35992·5	20·2
2776·80	... ...	36012·7	
2736·84	... ...	36538·5	40·6
2733·80	... ...	36579·1	19·4
2732·35	... ...	36598·5	
2698·44	... ...	37058·4	40·1
2695·53	... ...	37098·5	21·4
2693·97	... ...	37119·9	
2672·90	... ...	37412·6	42·8
2669·84	... ...	37455·4	22·2
2668·26	... ...	37477·6	
2649·30	... ...	37745·8	38·4
2646·61	... ...	37784·2	19·8
2645·22	... ...	37804·0	

In the sixth triplet, the first line has not been observed. There is a very strong line 2852·22 not far from where the

first line of the triplet should be. But this one is out of the question on account of its enormous energy, which would be quite out of comparison with the other lines. So we must suppose that the first line of the triplet is concealed by the strong line. Indeed, on the plates Prof. Kayser and I have examined, it would be impossible to detect a line close to 2852. Again, as in the spectrum of thallium, these triplets form two series (see Fig. 3), and again we find that the wave-numbers of the first, second, and third lines in each series are very accurately represented by a formula,

$$A - Bn^{-2} - Cn^{-4},$$

n standing for the row of entire numbers. For each series there are three values of A, but only one value of B, and

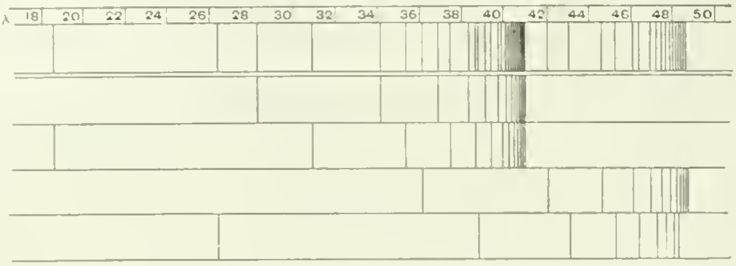


Fig. 2.

one value of C. The three values of A are very nearly the same in both series, indicating that the ends of both series coincide. The lowest number for which the formula gives a positive value is $n = 3$. To this value corresponds the strong green triplet. But in the other series the corresponding triplet ought to be found near 13000 10^{-8} cm. where photographic methods fail. It may be that it is identical with the lines that Becquerel has found near 12000 and 12120, the first of which, he says, is possibly double. The deviation between these and the calculated values is not so very great, considering the wide extrapolation of the formula. A small change in the value of C would alter the formula much more for $n = 3$ than for the higher values of n. Besides, we believe the formula only to be an approximation to the true function which may be developed into a series of descending powers of

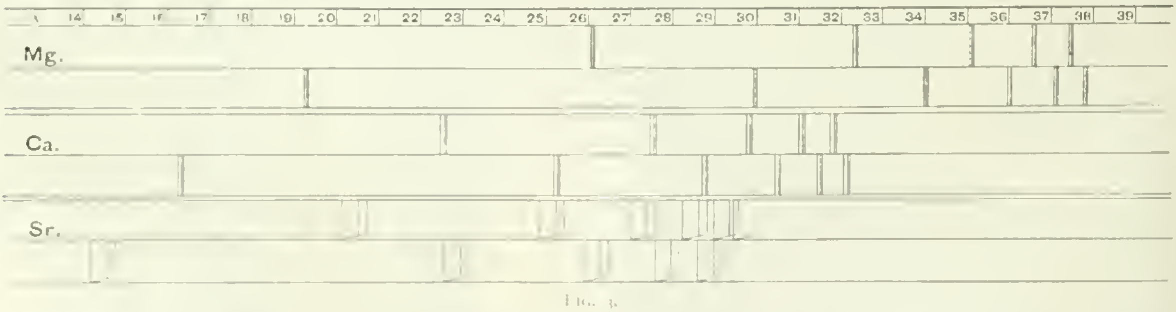


Fig. 3.

n^2. If this is so, the neglected terms world affect the values of the formula much more for the low values of n, than for the higher ones. The separation of the triplets into two series is not only suggested by the symmetry of the distribution, but also by the aspect of the lines.

In the spectra of calcium and strontium, we also find triplets with the same differences of wave-numbers, and their appearance teaches us in each spectrum to separate them into two series. We then see that the distribution of triplets shows a remarkable similarity to that in the spectrum of magnesium.

The dotted lines in the figure mean that the corresponding rays have not been identified with certainty. There are many lines beside those forming the triplets. For magnesium, the triplets contain 33 out of 56 lines, for calcium 33 out of 106, for strontium 29 out of 97. We have found that, as a rule, the higher the melting point of an element, the greater is the percentage of lines in the arc spectrum that do not belong to the series. From magnesium to calcium, and from calcium to strontium, the triplets widen and shift to the less refrangible side of the spectrum. The same thing happens in the spectra of other groups of chemically-related elements, the difference of wave-numbers of the doublets or triplets being somewhat proportional to the square of the atomic weight.

There is one more feature which seems interesting in regard to the connection of the spectra of different elements. In all the formulæ of series that have been observed, the coefficient of n^{-2} does not vary more than about 10 per cent. from its mean value, if we except one of the two series of doublets in the spectrum of aluminium where the variation is somewhat larger. I think, when in some time a satisfactory theoretical explanation of the symmetry in the spectra of the elements will be given, this co-efficient will prove to be an important physical constant.

C. Runge.

KARL VOGT.

THE life of Prof. Karl Vogt, who died on May 6, was no tranquil scientific career, for he was a fighting philosopher. He first comes into notice in 1839, working with Agassiz, then Professor at Neuchatel, on the "Freshwater Fishes of Central Europe." This great work, never completed, determined the direction of Vogt's best research during the rest of his long life. It was only in 1888–94 that the "Traité d'Anatomie Comparée," by Vogt and Jung, was published in Paris, taking high rank as a standard authority, and likely to retain it. He returned from Paris to his native town of Giessen, where he had been appointed Professor. But the revolution of 1848 soon burst forth, and we hear of him as an advanced Democratic Deputy contending for liberty and progress with the trenchant oratory he could use alike in politics and science. Political forces were too strong against him, and

he had to depart from his university and country, finding a home again in Switzerland, where he took up the double life of biologist and politician as a Professor at Geneva, and a prominent member of the National and Federal Council. His all-round knowledge is testified to by papers on Alpine geology, petrology, and prehistoric archæology. Those who were present at the Norwich meeting of the Congress of Prehistoric Archæology in 1868, remember his robust presence and slashing speech. To this subject, at the time rising into notice, belong Vogt's discourses, well known in the English translation, edited by Dr. James Hunt, and published by the Anthropological Society in 1864 under the title "Lectures on Man: his Place in Creation, and in the History of the Earth." There is so much forcible reasoning in this book, that it may still be read with profit thirty years after date. It is true that the thesis of the book which gained it favour with the polygenist school, whose desire was to trace the races of mankind to several locally and specifically distinct origins, is one which would nowadays hardly find supporters among anthropologists. Vogt held that the various branches of the human race trace their pedigrees to corresponding branches of the anthropomorpha. He cannot see "why American races of man may not be derived from American apes, Negroes from African apes, and Negritos, perhaps, from Asiatic apes." In these lectures Vogt shows a by no means admirable mode of controversy by unpleasant epithets, more or less like those which, in Germany as elsewhere, the orthodox world had poured on "infidels" and "materialists." But his sense of humour was blunt, and he evidently did not see that religion, which has swayed the universal human mind from untold ages, is a cosmic force which, by its very immensity, should be out of the reach of jokes like calling a low-type cranium an "apostle-skull." Even more remarkable in this respect is Vogt's "Köhlerglaube und Wissenschaft," an invective in the name of science on the credulous piety which, in countries where the trade of the charcoal-burner is plied, finds its best example among these isolated ignorant forest-folk. To the newer school of anthropologists, the term "charcoal-burner's belief" suggests quite a different sense. One would sit down by them and question them in order to find surviving in their minds ideas which are fossils from the most ancient times.

As a zoologist Vogt's reputation rests upon less equivocal grounds. The subject supplied him with fewer opportunities of displaying his anti-theological bias, and he brought his great powers to bear upon a number of problems, with the result that he added largely to the progress of zoology. His writings are numerous, and range over a wide variety of subjects; and he by no means confined himself to comparative anatomy, but made observations which entitled him to honourable rank among physiologists. In his "Traité d'Anatomie Comparée" he tells us, in the preface to the second volume, that he has studied and dealt monographically with no less than twenty-two types of animals, belonging to nearly every class of the animal kingdom. Much of this work was begun in the earlier part of his career, when he published many papers and several monographs upon the forms which he has afterwards chosen as types in his text-book. He was an active embryologist in earlier days, and wrote on the development of Filaria (1842), Batrachia (1844), Cephalophora (1856), and Crustacea (1873). In 1853 he published observations on the fertilisation of the ovum. He made a special study of the Siphonophora in 1852–54, and produced in 1868 an admirably illustrated monograph, entitled "Recherches sur les Animaux inférieurs de la Méditerranée," which deals with Siphonophora and pelagic Tunicata. His work on Branchipus and Artemia, published in 1872, is well known. Vogt's activity did not decrease with advancing years, as is testified by his contributions to current scientific literature and the publication of his text-book. His abilities were great, and he had a keen appreciation of the importance of the special problems of zoology to which he directed his attention. But his interests were too various, and his work ranged over too great a number of subjects, to admit of his rising to the position of a first-rate authority in any one of them. Had he applied himself solely to one course of study he would, by his powers of investigation and his vigorous method of exposition, have found a place among the foremost biologists of the century. As it was, he dissipated too much energy and thought in attempting to grasp too wide a range of knowledge.

E. B. T.
G. C. B.

At the meeting of the Paris Academy on May 6, M. Blanchard referred in the following terms to the part Vogt took in the study of the formation and movement of glaciers, under the direction of Louis Agassiz.

"At the beginning of August 1845, Agassiz arrived at the hospital of Grimsel, accompanied by Carl Vogt, Desor, Nicolet, and two students from Neuchatel. They brought their instruments with them, for they had come with the idea of determining the temperature of the glaciers, of studying the form of the snow, and of ascertaining in what manner the *névé* forms itself into ice.

"They had with them two very experienced men as guides; they resolved to take up their position on the smaller glacier of the Aar, which is of special interest; the surface is strewn with masses of rock, which produces an effect of a heap of ruins. On approaching the moraine, the investigators perceived that the glacier had advanced considerably since the previous year. A hut, left by Hügi, one of the first explorers, had disappeared.

"After a brief survey, they fixed the place of installation near a large block, and the guides set to work to build a small house large enough to hold six people. The walls were built of dry stones; large flagstones served as boards; beds were made of layers of grass, covered with oilcloth and other coverings, and were thought perfect.

"As a matter of fact, the opening which gave access to the house was very small, but still Carl Vogt could enter, and where Carl Vogt could pass every one could. Instead of a door, a curtain was put up. In the night, before going to bed, it was decided that the dwelling should be called the "Hôtel des Neufchâtelois": this name was, therefore, cut on the rock in big letters, and time has consecrated it.

"Does not this reunion of young savants in the solitude, in the middle of a nature both grand and sad, offer a curious spectacle to the imagination? The noises of the pleasures of this world and of public affairs does not ascend as far as the hut on the glacier of the Aar: aspirations and joys, unknown to most mortals, agitate the hearts there. These men, who without effort, without regret, renounce comfort for many a long day, dream of penetrating into the deepest secrets of nature; they discuss gravely most formidable questions, and laugh over many incidents. Agassiz never loses his good humour, and Desor abandons himself to joking. Carl Vogt, always sparkling with fun, and himself capable of enlivening an assembly of monks, effectually prevents the possibility of *ennui*.

"Amongst the investigators, who are stirred by the same thought, peace is never broken; on the sea of ice, with no other witnesses than the blocks of granite, and the peaks covered with eternal ice, there are no rivals. In proportion to the extent of his aptness every one sets himself with energy to the common work. Agassiz is the undisputed chief, the recognised master. To bring a stone to the monument he was building, was the only thing the zealous workers cared about.

"They rose early at the 'Hôtel des Neufchâtelois': on the stroke of four they had to be up. The time of dressing was rather trying, as the water was so cold and made them shiver; but that over, nothing more was thought of than continuing their research. Agassiz volunteers to bore holes; the ice can only be cut with great difficulty, for it resists the instruments. While this operation is being done, Carl Vogt examines the red snow, the strange hue of which is due to the presence of myriads of microscopic beings; he discovers many kinds of infusoria, and a pretty rotifer sowing the snow with its purple-coloured eggs.

"Carl Vogt was never inactive; in the last years he published, together with M. Jung, a treatise on zoology. Every one will acknowledge that a life so well spent is an honour to humanity." W.

NOTES.

SCIENCE is but poorly represented in the list of the Queen's birthday honours. Lord Playfair, previously a K.C.B., has accepted the honour of G.C.B. Rear-Admiral W. J. L. Wharton, Hydrographer to the Navy, has been made a Companion of the Order of the Bath. Mr. W. M. Conway, whose climbs in the Himalayas led to the publication of some interesting scientific results, has been knighted.

DR. E. FRANKLAND, F.R.S., Correspondent of the Paris Academy of Sciences, has been elected Foreign Associate, in the place of the late Prof. van Beneden.

DR. ESMARCH, of Kiel, has been elected a Correspondent of the Paris Academy of Medicine.

PROF. THOMSEN, who has been a Corresponding Member of the French Academy of Inscriptions since 1860, has been elected a Foreign Associate, in the place of the late Sir H. Rawlinson.

THE *Times* correspondent at Melbourne says that a meteorological observatory has been established on the summit of Mount Wellington, Tasmania.

A MILLION acres of forest land has been reserved by the Province of Ontario as a great natural park for the preservation of native animals and plants.

THE discourse at the Royal Institution to-morrow evening will be delivered by the Earl of Rosse, the subject being, "The Radiant Heat from the Moon during the progress of an Eclipse." That on June 7 will be by Prof. A. Cornu, F.R.S. This lecture will be delivered in French, and the title will be, "Phenomenes Physiques des Hautes Régions de l'Atmosphere."

THROUGH a gift of Mr. W. C. McDonald (says *Science*), McGill University has secured thirty-five acres of land for botanical gardens and an observatory. From the same source we learn that the residue of the estate of Mary D. Peabody has been left to the Catholic University of Washington, for the foundation of scholarships (probably three or four of the value of 5000 dollars each) in the chemical and physical sciences.

AMONG the appointments abroad, we notice that Dr. N. V. Ussing has accepted the Professorship of Mineralogy in the University of Copenhagen. Dr. F. Karsch has become Extraordinary Professor of Zoology in Berlin University, Prof. Emil Behring has become Ordinary Professor of Hygiene at Marburg, and Dr. Zaremba (privat-docent in mathematics at Krakau) has been promoted to an Extraordinary Professorship.

THE electrical power developed at the Niagara Falls will soon take the place of steam for several hundred miles distance from the Falls, including New York City. An important proposed application is to the Erie Canal, which has just opened for the season. Experiments will be made for applying the power by a trolley system, and the reduction of expense will probably drive out all other means of transportation for grain, &c., from Buffalo to tide water, during the season of navigation.

AT the International Horticultural Congress, opened at Paris on Saturday, resolutions were unanimously adopted to the effect: "(1) That the French Government should associate itself with the request addressed by the Italian Government to the Swiss Confederation, with a view of obtaining the revision of the Berne International Convention, and the free circulation between all countries signatory to the convention, of all vegetables and vines, accompanied by a certificate of origin; and (2) that the postal administration should return to the old reduced tariff, of which periodical publications on horticulture have hitherto had the advantage."

EFFORTS are being made (says the *American Naturalist*) to raise a fund of 12,000 dollars for the purpose of bringing Mr. Peary and his two assistants home from North-west Greenland early next autumn, and, in connection with this, to prosecute scientific investigations during the available summer season. It is hoped, by this means, to charter and fit out a staunch steamer, built for Arctic service and commanded by experienced Arctic navigators, which shall start from St. John's, Newfoundland, on or about July 5, 1895, for Inglefield Gulf, North-west Greenland, lat. 78 N., Mr. Peary's headquarters.

WE have received a notice concerning three "Priestley" Scholarships in Chemistry, two "Bowen" Scholarships in Engineering, and one in Metallurgy, which have been founded by the late Mr. T. Aubrey Bowen, of Melbourne. They are intended to encourage and afford facilities for the higher study of these subjects in Mason College, where they are tenable for one year, with the possibility of renewal at the discretion of the Council of the College. The annual value of each is £100. Although, naturally, good work done at Mason College will be regarded as a specially favourable qualification, the Council have generously thrown all the Scholarships open to general competition. The first award will be made in September next, and all particulars may be learned on application to the Secretary of the College.

THE gold medal of the Linnean Society has this year been awarded to Prof. Ferdinand Cohn, of Breslau, whose name is well known in connection with the *Botanic Journal*, which he has conducted, largely adorned with his own contributions, from 1870 to the present time. The work of Dr. Cohn extends over half a century. He was one of the earliest to investigate the life-history of the lower Algæ, and to demonstrate that they are not asexual. His important paper on *Protococcus pluvialis*, published so long ago as 1850, was translated by Busk for the Ray Society. Subsequent papers by him, on the mode of reproduction of *Sphaeroplea annulina*, and on the development of *Volvox*, mark a distinct advancement in botanical science. The medal referred to was awarded to him at the anniversary meeting of the 24th inst., and has been forwarded to Breslau, for his acceptance, through the German Embassy.

AT the anniversary meeting of the Royal Geographical Society, held on Monday, the Founder's Medal was presented to Dr. John Murray for his services to physical geography, and especially to oceanography during the last twenty-three years, also for his maps of the floor of the ocean, his calculations regarding the volume of continents and oceans, his study of the origin and formation of coral deposits, and for the stimulus he has given to researches in physical geography. The other awards were the Patrons' Medal, to the Hon. George N. Curzon, M.P., (1) for his work on the history, geography, archæology, and politics of Persia; (2) for his journeys in

French Indo-China, which have resulted in further publications of geographical as well as political and general value: and (3) for his journey to the Hindu Kush, the Pamirs, and the Oxus, together with a visit to the Amir of Afghanistan, in his capital of Kabul. The Murchison Grant, to Mr. Eivind Astrup, for his remarkable journey, with Lieut. Peary, across the interior glacier to the northern shores of Greenland: and for his independent journey along the shores of Melville Bay; the Back Grant, to Captain C. A. Larsen, for the geographical and meteorological observations made by him during his Antarctic voyage in 1894, and for his discovery of an active volcano on Christensen Island, of several other islands, and of part of the east coast of Graham Land; the Gill Memorial for 1895, to Captain J. W. Pringle, R.E.; and the Cuthbert Peek Grant for 1895, to Mr. G. F. Scott-Elliot, for his explorations of Mount Ruwenzori, and of the region to the west of the Victoria Nyanza.

WE wish the American Metrological Society success in its efforts to extend the use of the metric system in the United States, and to procure general agreement with regard to the constants of science. Its objects are ambitious, as the following statement of them, from *Science*, will show: (1) To improve existing systems of weights, measures and moneys, and to bring them into relations of simple commensurability with each other. (2) To secure universal adoption of common units of measure for quantities in physical observation or investigation, for which ordinary systems of metrology do not provide, such as divisions of barometer, thermometer, and densimeter; amount of work done by machines; amount of mechanical energy, active or potential, of bodies, as dependent on their motion or position; quantities of heat present in bodies of given temperatures, or generated by combustion or otherwise; quantity and intensity of electro-dynamic currents; aggregate and efficient power of prime movers; accelerative force of gravity; pressure of steam and atmosphere; and other matters analogous to these. (3) To secure uniform usage as to standard *points of reference*, or physical conditions to which observations must be reduced for purposes of comparison, especially temperature and pressure, to which are referred specific gravities of bodies, and the zero of longitude on the earth. (4) To secure the use of the decimal system for denominations of weight, measure, and money derived from unit-bases, not necessarily excluding for practical purposes binary or other convenient divisions, but maintained along with such other methods, on account of facilities for calculation, reductions, and comparison of values, afforded by a system conforming to our numerical notation.

ON January 18, the great seismometrograph at the Osservatorio del Collegio Romano at Rome registered five complete pulsations of slow period characteristic of earthquakes originating at a great distance. They commenced at 4h. 37m. 30s. p.m. (Greenwich mean time), and lasted 1m. 22s., giving an average duration of 16·4 seconds for each pulsation. On the same day a severe earthquake was felt along the east coast of Japan, and was recorded at Tokio at 3h. 48m. 24s. The distance between this place and Rome being about 9500 km., the pulsations must have travelled with an average velocity of 3·2 km. per second (see NATURE, vol. l. pp. 450-51; vol. li. p. 462). At Nicolaiew and Charkow, in the south of Russia, the horizontal pendulums were disturbed for nearly an hour, the epoch of maximum amplitude occurring a few minutes earlier than at Rome.

MR. MARSHALL HALL publishes in the *Alpine Journal* (vol. xvii. p. 438) a note on the progress made in the study of glaciers, for which purpose a Committee was appointed at the meeting of the International Congress of Geologists at Zürich. Good work appears to have been done, in exploring and mapping, among the glaciers of New Zealand, in the course of which Franz-Joseph Glacier, on the west coast, was found to end at a height of 692 feet above the sea, and a distance of four miles from it. The rate of movement is, of course, variable: an average of the observations (with certain omissions) gives 154·2 inches per diem. Valleys containing large glaciers give indications that the ice has been higher than it is at the present day, and has paused at four different levels. Work also has been done among the glaciers of the eastern side of New Zealand, and a few facts are recorded; among them, that in advancing the ice appears not to plough up the earth. In conclusion, Mr. Marshall Hall calls upon mountain climbers to help in the work of the Committee.

A PAPER on "The Brain of the Microcephalic Idiot," by Prof. D. J. Cunningham, F.R.S., and Dr. Telford-Smith, read before the Royal Dublin Society nearly a year ago, and noticed at the time in these columns (NATURE, vol. l. p. 287), has just been published in the Society's *Transactions*. The authors give the results of a thorough examination of the brains and skulls of two typical microcephales. Their study leads them to accept the view arrived at by Sir George Humphry, from an examination of microcephalic and macrocephalic skulls, viz.: "There is nothing in the specimens to suggest that the deficiency in the development of the skull was the leading feature in the deformity, and that the smallness of the bony cerebral envelope exerted a compressing or dwarfing influence on the brain, or anything to give encouragement to the practice lately adopted in some instances of removal of a part of the bony case, with the idea of affording more space and freedom for the growth of the brain. In these, as in other instances of man and the lower animals, the brain-growth is the determining factor, and the skull grows upon and accommodates itself to the brain, whether the latter be large or small."

DR. W. M. HAFFKINE has brought together his Indian experiences in anti-choleraic inoculations, and has published them in the *Indian Medical Gazette*. In spite of the very numerous difficulties which he had to encounter in carrying out his investigations, Dr. Haffkine has succeeded, with the assistance of others, in inoculating no less than 32,166 individuals with his cholera vaccine. Every pains was taken to obtain trustworthy records of the results derived from these inoculations, and, as far as can be judged from the data to hand, the balance appears to be decidedly in favour of the process. This is perhaps especially brought out by Dr. Haffkine's work in Calcutta, where the percentage of attacks and deaths amongst the inoculated was 1·18 per cent., whilst amongst the non-inoculated the percentage of cases amounted to 15·63 per cent., and of deaths 11·63 per cent. One fact has indisputably been established by these investigations, and that is the harmlessness of the operation; in view of this it is to be hoped that the inoculations may be more widely spread, and further facilities thus offered for the collection of observations on this very important subject.

A YEAR'S actinometric observations, made at the Konstantinow Observatory, Pawlowsk, are recorded by J. Schukewitch in the *Repertorium für Meteorologie*. They have led to some unexpected results regarding the intensity of the sun's radiation at different seasons of the year. This intensity, as measured on the surface of the earth, depends upon the altitude of the sun and upon the transmitting power or opacity of the atmosphere. The intensities were measured by a thermometer with blackened bulb, which was exposed to the sun side by side with a precisely similar one which was kept in the shade. To test whether the two thermometers were identical in their behaviour, two successive readings were taken, in which first the one and then the other was shaded. It was found necessary to take the mean of these two readings in each case. The tables embodying the results contain, besides the intensity, the state of the sky, the altitude of the sun, and other meteorological data. From these tables the yearly course of intensity of the unclouded sun at noon

is worked out. It shows a principal maximum in April, a secondary maximum in September, and chief minimum in November. The intensity of solar radiation for equal altitudes is greatest in winter and least in summer, a circumstance which tends to equalise the winter and summer temperature. A great diminution of transmissibility is brought about by that phenomenon so characteristic of the middle-European continent, called in Germany *Höhenrauch*, an elevated stratum of peat-smoke which gives a faint and rather pleasant odour, usually taken to indicate the continuance of fine weather. The author finds greater opacity in front of an atmospheric depression, and greater transmissibility after it. The clearest air is preceded by a heavy summer rain.

THE Meteorological Council have published a valuable set of monthly meteorological charts of the Red Sea, showing the prevalent winds and currents, with other information of use to seamen passing through the Suez Canal to India. The wind observations alone number nearly 75,000, and have been supplied from logs specially kept for the Meteorological Office, from ships belonging to the Royal Navy, and various other sources. Each chart, of which there are twenty-four, contains useful remarks referring to the leading features, which are shown graphically, and the introduction to the Atlas contains an interesting general summary by Lieutenant C. W. Baillie, R.N., Marine Superintendent. The wind charts show that from October to January northerly winds are prevalent over the northern half of the sea, and southerly over the southern portion. From February to May the northerly winds extend further south, while southerly winds prevail from near Perim to about the 16th parallel. From June to September, northerly winds blow over nearly the whole sea. Gales are most frequent between November and March; they generally blow from the southward, and are mostly met with in the southern part of the sea. The currents are somewhat erratic, and while occasional strong streams are experienced locally, their velocity is not usually great over large areas. The Gulf of Aden may be taken as an exception, as the currents often set there with considerable velocity. An interesting feature has been noticed in the range of sea-temperature in the Strait of Bab-el-Mandeb, near the Island of Perim, where it amounts to 20° at the period of the S.W. monsoon. The whole work shows evidence of the great care and labour bestowed upon it.

THE extent to which many of the American agricultural experiment stations are devoting attention to the culture of small fruits and other minor crops is perhaps significant of an impending change in the economic bearings of the management of the soil, and of the partial displacement of the *grande culture* which has hitherto almost monopolised the field of experimental inquiry. Bulletin No. 55 of the Purdue University Station, Lafayette, Indiana, opens with a description of experiments with small fruits, carried out in response to the numerous inquiries received from farmers and others concerning the different varieties of such fruits. Strawberries, raspberries, blackberries, currants, gooseberries, and grapes, form the subject of this section of the report. Field experiments with maize and oats are next dealt with, and amongst the results noted, it is stated that better yields have followed the sowing of two bushels or more of oats per acre than that of any smaller quantity. The bulletin concludes with a notice of experiments with sugar beet; but in view of the desperate condition to which the beet-growers of France are at present reduced, despite the artificial support which the sugar industry there receives under the bounty system, we cannot see any immediate hope for the American beet-sugar industry. This, indeed, is practically admitted in the bulletin, for it is said: "The condition of the sugar business throughout all sugar producing countries is such that there seems to be little probability of capital being invested in beet-sugar plants in this country at present." The points which are reported upon include comparison of varieties, time of harvest, the respective effects of bacterial disease and beet scab on the sugar content of beets, the effect of loosening beets some time before lifting them from the ground, special thinning, tests of foreign and American seed, and yield and cost of crop per acre. It is concluded that, under more favourable economic conditions, beet factories might advantageously be established in the State of Indiana.

A PAPER by Wilhelm von Bezold, on the lines of equal disturbance of the magnetic potential of the earth, appears in a recent number of the *Sitz. der Akad. der Wiss. zu Berlin*. The deviation of the potential at any place from the mean value of the potential corresponding to the parallel of latitude passing through this place being called the disturbance, the author gives the theory of the lines of equal disturbance. He shows that the westerly (or easterly) component of the earth's magnetism is given by the rate of change of the disturbance of the potential along the parallel of latitude or $W = \frac{\partial Va}{\partial y}$ where Va is the disturbance for the potential, and ∂y is an element of a latitude circle. Hence it follows that a knowledge of a westerly component of the earth's field for the whole surface of the earth suffices to everywhere determine the disturbance in the magnetic potential, and draw the lines of equal disturbance. Wherever the lines of equal disturbance are tangential to a circle of latitude $\frac{\partial Va}{\partial y} = 0$, and hence $W = 0$, or all such points will lie on the agonic lines, *i.e.* the lines along which the declination is zero. At all places where the lines of equal disturbance are tangential to the terrestrial meridian the northerly component of the earth's field has its normal value. The author has constructed a chart of these lines for the epoch 1880, using the data given in the magnetic charts published by G. von Quintus Icilius. The mean value of the magnetic potential for the latitude λ is found to be given with a high degree of accuracy by the expression $V_n = K \sin \lambda$, and the author considers that this simple expression must have some special significance, and not be merely an empirical formula.

A PAPER, by Mr. G. C. Whipple, entitled "Some Observations on the Growth of Diatoms in Surface Waters" (*Technological Quarterly*, vol. vii.), is a valuable contribution to the study of the periodic frequency of microscopic organisms in freshwater areas. The work is noteworthy as having been carried on in a biological laboratory attached to the Boston Water Works. The author's general results are here given: (1) That the growth of diatoms in ponds is directly connected with the phenomena of stagnation; that their development does not occur when the lower strata of water are quiescent, on account of greater density, but rather during those periods of the year when the water is in circulation from top to bottom. (2) That diatoms flourish best in ponds having muddy bottoms. (3) That in deep ponds there are two well-defined periods of growth—one in the spring and one in the autumn; that in shallow ponds there is usually a spring growth but no regular autumn growth, and that other growths may occur at irregular intervals as the wind happens to stir up the water. (4) That the two most important conditions for the growth of diatoms are a sufficient supply of nitrates and a free circulation of air, and that both these conditions are found at those periods of the year when the water is in circulation. (5) That while temperature has possibly a slight influence on the growth of diatoms, it is of so little importance that it does not affect their seasonal distribution. (6) That the increase of diatoms takes place substantially in accordance with the law of geo-

metrical progression, and that the cessation of their growth is caused by the diminution of their food supply.

Mr. H. G. Wells's scientific fantasy, the "Time Machine," which has been appearing as a serial in the *New Review*, will be published in volume form, by Messrs. Heinemann, in the course of a few days.

Mr. Gisbert Kapp has arranged with Messrs. Whittaker and Co. for a translation from the German of his new work on the "Alternate Current Transformer." The volume will be published in the "Specialists' Series" in the autumn.

The papers on the relation of diseases of the spinal cord to the distribution and lesions of the spinal blood-vessels, recently contributed by Dr. R. T. Williamson to the *Medical Chronicle*, have been reprinted and published in book form by Mr. H. K. Lewis.

This week's new editions include Prof. T. Preston's philosophical "Theory of Light," published by Messrs. Macmillan. More than one hundred pages of new matter have been added, a valuable addition being an account of Prof. Newcomb's experiments to determine the velocity of light. The second edition has appeared of the late Prof. Cayley's "Elementary Treatise on Elliptic Functions" (Macmillan), the first edition of which was published in 1876. Another second edition, received during the past week, is "A First Book of Electricity and Magnetism," by Mr. W. Perren Maycock. This book, now greatly enlarged, is published by Messrs. Whittaker and Co.

The Deutsche Seewarte, which, with the year 1894, has completed its twentieth year of useful activity, has just issued the seventeenth volume of *Aus dem Archiv*. This work, which has contained many elaborate and valuable discussions in meteorology, navigation, and nautical astronomy, is now devoted more especially to discussions of practical utility to seamen. Among the articles of more general scientific interest may be mentioned one by Dr. Grossmann, on the application of Bessel's formula in meteorology, and one by Dr. Maurer, on the application of graphical methods in meteorology and physics generally; the latter investigation may possibly lead to the substitution of this method for the use of tables in some of the problems of nautical astronomy.

The additions to the Zoological Society's Gardens during the past week include six Hairy-footed Jerboas (*Dipus hirtipes*), two Lesser Egyptian Gerbilles (*Gerbillus ægyptius*), two Lybian Zorillas (*Ictonyx lybica*), two Grey Monitors (*Varanus griseus*), two Egyptian Mastigures (*Uromastix spinipes*), three Egyptian Geckos (*Tarentola annularis*), a Common Chameleon (*Chamæleon vulgaris*), seven Common Skinks (*Scincus officinalis*), two Cerastes Vipers (*Vipera cerastes*), two Diademed Snakes (*Zamenis diadema*), from Egypt, presented by Dr. John Anderson, F.R.S.; a Grysbok (*Neotragus melanotis*, ♀), from South Africa, presented by Mr. J. E. Matcham; a Wapiti Deer (*Cervus canadensis*, ♀), a Japanese Deer (*Cervus sika*, ♀), a Burchell's Zebra (*Equus burchelli*, ♂), two Polar Hares (*Lepus glacialis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Mercury and Venus.—The planet Mercury is now an evening star, and will be favourably placed for observation until towards the end of June. The greatest elongation will occur on June 4 at 13h., when the planet will transit about 1h. 42m. after the sun; the declination will then be nearly 25° north, and the apparent diameter a little over 8". Jupiter will be in close proximity to Mercury during the present period of its visibility, so that observers not employing telescopes must be careful to discriminate between the two; at the elongation, Mercury will precede Jupiter by about 8m. in R.A., and will be about 1½ degrees farther north. The two planets will be in actual conjunction on June 8 at 4h., Mercury being 0° 47′ N. of Jupiter.

Venus, also, is most favourably situated for observation at the present time, and the great brilliance of the planet in the western sky after sunset cannot fail to attract the attention of the most indifferent. It will not, however, reach maximum brightness until August 13. The greatest eastern elongation will occur on July 11, and the apparent diameter will increase from 16" on June 1 to 59" at the inferior conjunction on September 18.

The Total Solar Eclipse of 1898 January 21–22. In addition to the eclipse of the sun which will take place on August 8, 1896, and for which we understand preparations are already well in hand, there will be another important solar eclipse before the end of the present century. This will occur on January 21–22, 1898, and the *Nautical Almanac Circular*, No. 16, gives local particulars of the same for that portion of the path of the shadow which lies across India. At Rajapur the duration will be 2m. 1·9s. and the altitude of the sun 53°; at Nagpur, 1m. 17·7s. with an altitude of 46°; and at a position south of Benares, 1m. 43·6s. with an altitude of 40°. Information as to the meteorological conditions prevailing at various points along the track of the eclipse during the latter part of January is being collected through the assistance of Mr. Eliot, Meteorological Reporter to the Government of India. It is proposed to publish this information early in 1897.

As the next sun-spot minimum is not due until the year 1900, observations of the phenomena of this eclipse will furnish information as to the solar conditions during the transition from maximum to minimum.

The Astro-photographic Chart.—The third part of the second volume of the *Bulletin* of the International Permanent Committee, gives an account of the present state of the great undertaking to prepare a photographic catalogue and chart of the heavens. The reports from the various participating observatories indicate in general a rapid advance towards the completion of the photographs which are intended to form the basis of the catalogue; four of the eighteen observatories have already completed the zones allotted to them, and it is expected that at least eight more will reach this stage by next spring. Systematic work at the South American observatories has been seriously interfered with by political events; but it is satisfactory to learn that the Australian and Cape of Good Hope astronomers are prepared to come to their assistance. An immense number of catalogue plates with short exposures has been taken with the various instruments, no less than 753 having been taken at Paris, and 1562 at the Cape. The measurement of the catalogue plates is also in a forward state at several of the observatories, but the reductions have scarcely been commenced.

For the chart itself, not one-third of the requisite photographs have yet been obtained, but the progress of this part of the work is necessarily slow, in consequence of the long exposures required.

Dr. Gill proposes that the Committee should meet in 1896, to reconsider the various questions left open at the former conference, among which one of the most important relates to the scale of magnitudes to be adopted.

Four important memoirs also form part of the present report. Prof. Turner and M. Prosper Henry discuss different methods of reducing the plates. M. Trépied gives his experience and views as to the determination of magnitudes, and M. Donner discusses the various corrections for instrumental errors.

Award of the Watson Medal.—On the recommendation of the Board of Trustees of the Watson Fund, the U.S. National Academy of Sciences last year unanimously awarded the Watson medal to Dr. S. C. Chandler, for his investigations relative to variable stars, his work in connection with the variation of terrestrial latitudes, and his researches on the laws of that variation. The recommendation was noted in these columns a year ago, and a description of the founding of the award was given (Nature, vol. l. p. 157). The medal was presented to Dr. Chandler at the recent meeting of the National Academy, and *Science* for May 3 contains the report of the Trustees, setting forth the grounds upon which the award was made, and briefly stating the history of the investigation of changes of latitude. Dr. Chandler's work upon the subject began with observations made by him in 1884–85. His observations, continued uninterruptedly for thirteen months, revealed a progressive change of a pronounced periodical character in the

instrumental values of the latitude. Circumstances prevented him from carrying on the work until six years later, when he took up the problem again. The results then obtained are published in a series of eighteen papers in the *Astronomical Journal* (1891–94), exclusive of a series of five papers upon a topic closely related thereto, namely, the aberration-constant. These papers have been noted from time to time in this column, so it is unnecessary to do more than refer to them now.

In connection with variable stars, besides the incidental work of observation and discovery which Dr. Chandler has contributed to it, his work has involved the collection of all the data in astronomical history, their discussion, and the formulation of the elements of their light-variations into numerical laws. His important researches upon cometary orbits are also well known to astronomers.

A LECTURE EXPERIMENT.

A FURTHER description of the use of the electric furnace recently exhibited at the Royal Society, for the purpose of lecture demonstrations, may be useful, as pictures, some six feet across, of the interior of the furnace may readily be projected on the screen. This is effected by the aid of the device which has already been given in NATURE (p. 17, Fig. 2). The result is really very beautiful, though it can only be rendered in dull tones by the accompanying illustration (Figs. A, B). It may be well, therefore, to state briefly what is seen when the furnace is arranged for the melting of metallic chromium. Directly the current is passed, the picture reflected by the mirror, E (Fig. 2, *loc. cit.*), shows the interior of the furnace (Fig. A) like a dark crater, the dull red poles revealing the metallic lustre and grey shadows of the metal beneath them. A little later these poles become tipped with dazzling white, and, in the course of a few minutes, the temperature rises to about 2500 C. Such a temperature will keep chromium well melted, though a thousand degrees more may readily be attained in a furnace of this kind. Each pole is soon surrounded with a

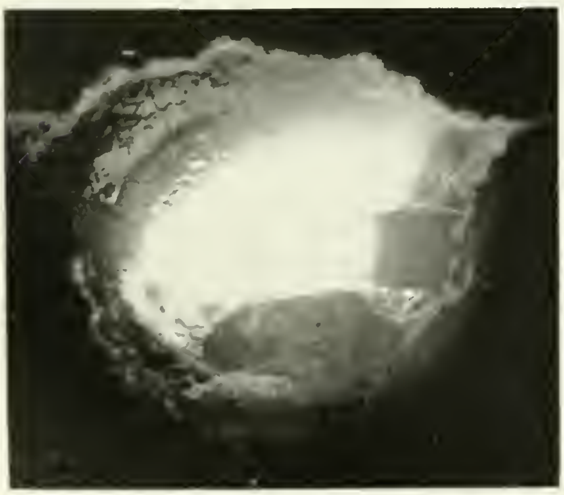


FIG. A.—This represents the interior of the furnace containing molten chromium as is seen either by reflection on a screen or by looking into the furnace from above, the eye being suitably protected by deeply tinted glasses.

lambent halo of the green-blue hue of the sunset, the central band of the arc changing rapidly from peach-blossom to lavender and purple. The arc can then be lengthened, and as the poles are drawn further and further asunder, the irregular masses of chromium fuse in silver droplets, below an intense blue field of light, passing into green of lustrous emerald; then the last fragments of chromium melt into a shining lake, which reflects the glowing poles in a glory of green and gold shot with orange hues. Still a few minutes later, as the chromium burns, a shower of brilliant sparks of metal are projected from the furnace, amid the clouds of russet or brown vapours which wreath the little crater; while if the current is broken, and the light dies out, you wish that Turner had painted the limpid tints, and that Ruskin might describe their loveliness.

The effect when either tungsten or silver replaces chromium is much the same, but, in the latter case, the glowing lake is more brilliant in its turbulent boiling, and blue vapours rise to be condensed in iridescent beads of distilled silver which stud the crater walls.

FIG. B.—In this case the arc was broken the instant before the photograph was taken. The furnace contained a bath of silver just at its boiling point. The reflection of the poles in the bath, the globules of distilled silver, and the drifting cloud of silver vapour, are well shown.

Such experiments will probably lend a new interest to the use of the arc in connection with astronomical metallurgy, for, as George Herbert said long ago—

> "Stars have their storms even in a high degree,
> As well as we";

and Lockyer has shown how important it is, in relation to such storms, to be able to study the disturbances in the various strata of the stellar or solar atmosphere. Layers of metallic vapour which differ widely in temperature can be more readily obtained by the use of the electrical furnace than when a fragment of metal is melted and volatilised by placing it in the arc, in a cavity of the lower carbon. W. C. ROBERTS-AUSTEN.

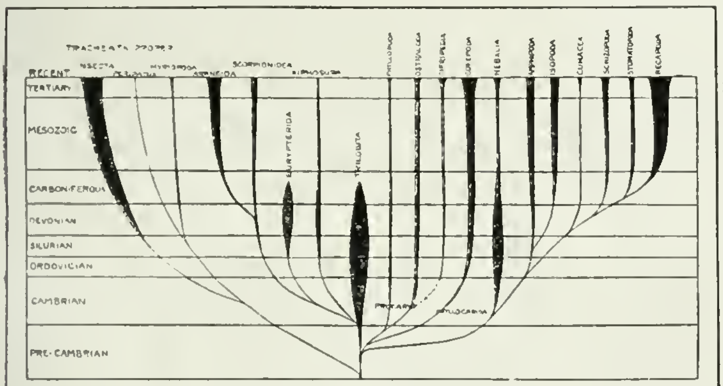
THE LIFE-HISTORY OF THE CRUSTACEA IN EARLY PALÆOZOIC TIMES.

IN his recent anniversary address to the Geological Society, the President, Dr. Henry Woodward, F.R.S., after the usual distribution of medals and awards, the reading of obituaries of deceased Fellows, and some preliminary matters relating to the affairs of the Society, including the moot question of the introduction of ladies as visitors to the evening meetings, devoted the remainder of his address to a brief discussion of "Some Points in the Life-history of the Crustacea in Early Palæozoic Times." Dr. Woodward continued as follows:—"Of the various groups of the Invertebrata whose ancestry extends into Palæozoic times, none possess a greater interest for the geologist than the Crustacea, whose existence is proved as far back as the Lower Cambrian rocks; while their near allies, the Arachnida, have been met with in strata as old as the Silurian.

"My earliest papers on the Eurypterida appeared in 1863 and 1864, and an account of *Stylonurus* and *Hemiaspis* was communicated to this Society in 1865, just thirty years ago. In that year (1865) I had the pleasure, with my friend and fellow-worker, the late J. W. Salter, F.G.S., of publishing a 'Chart of Fossil Crustacea,' in which an attempt was made to show the evolution in time of the various forms belonging to this class, graphically depicted on an engraved folding-sheet, with explanatory text. In it we pointed out that the main development of the crustacea in Palæozoic times consisted of the great groups of the Trilobita, the Eurypterida, the Xiphosura, the Phyllopoda, and the

Ostracoda. The faint beginnings of other great groups were also indicated, such as the Macrouran-decapods represented by *Anthrapalæmon* and other forms in the Coal Measures; the Stomatopods by *Pygocephalus Cooperi*, the Amphipods by *Gampsonyx*, both in the Coal Measures: and by *Prosoponiscus* in the Permian. Lastly, the Cirripedia, by the anomalous form *Turrilepas*, from the Wenlock Limestone.

"In November 1866, I laid before this Society the evidence upon which I based my arrangement of the *Pterygoti* and *Limuli* in one order, for which I adopted Dana's very appropriate name of Merostomata (or 'thigh-mouthed' animals)—expanded to include all those ancient crustaceans comprehended in the two sub-orders of Eurypterida and Xiphosura, and forming two groups of long-bodied and short-bodied forms, quite parallel to the Brachyoura and Macroura in the Decapoda; even the intermediate forms—corresponding to the Anomoura—being paralleled by the Hemiaspidæ (*Hemiaspis*, *Pseudoniscus*, &c.). This group formed the subject of a monograph published by the Palæontographical Society (1865–1878) comprising 17 genera and 84 species—69 of which are Palæozoic in age. The integrity of this group, founded on the researches of Huxley, Salter, Dana, Hall, and many others besides myself, has been firmly maintained, although many attempts have since been made to detach it from the Crustacea and place it with the Arachnida. For instance, it was proposed by Dr. Dohrn, in 1871, to include the Merostomata in a still larger division, under Haeckel's term Gigantostraca, which was made by expansion to embrace the Merostomata and the Trilobita, and to be placed *between* the Crustacea and the Arachnida.



THE EVOLUTION OF THE ARTHROPODA IN GEOLOGICAL TIME.

"In arguing for their retention before this Society in 1871 I wrote:—'Take away the trilobita from the pedigree of the crustacea, and I submit that one of the main arguments in favour of evolution to be derived from the class, so far from being strengthened, is destroyed. From what are the crustacea of to-day derived? Are we to assume that they all descended from the phyllopods and ostracods—the only two remaining orders whose life-history is conterminous with that of the trilobita? Or are we to assume that the arachnida are the older class?' 'If,' says Fritz Müller, 'the crustacea, insecta, myriapoda, and arachnida are indeed all branches of a common stock, it is evident that the water-inhabiting and water-breathing Crustacea must be regarded as the original stem from which the other terrestrial classes, with their tracheal respiration, have branched off.'

"In the above-quoted paper I pointed out that the young *Limulus*, when it quits the egg, has the hinder body as large as the head-shield, and the nine segments composing it are most clearly marked out, the abdominal spine being quite rudimentary and forming in fact the 9th segment. This is the so-called '*Trilobiten-stadium*' of Dohrn.

"'At this stage,' says Packard, 'the young swim briskly up and down, skimming about *on their backs* by flapping their gills, not bending their bodies.' This locomotion of the young *Limulus*, by swimming upon its back, near the surface of the water (by means of its gill feet), agrees very closely with the habit of *Apus*, of *Chirocephalus*, and *Artemia*, and is extremely suggestive of its affinity to the phyllopoda, with which, at this stage of its existence, it has many points in common, as well as with the trilobita.

"It is interesting to notice that the Xiphosura (king-crabs)—which form the surviving representatives of this ancient order of the Merostomata, and are so widely distributed in the Coal Measures of North America, Britain, &c.—have likewise been discovered as far back in time as the Upper Silurian of Lanarkshire, being represented by a small form which I named and described, in 1868, *Neolimulus falcatus*, having eight thoracic segments apparently free and movable, but wanting the tail-spine, which probably was developed later in life, or may have been represented by an extremely short terminal plate, as we see is the case in the young larval *Limulus*. Thus the earliest fossil king-crab known probably resembled closely the free-swimming larva of the living king-crab as it leaves the egg.

"As to whether the Eurypterida—with their evidently aquatic branchiated respiration, their jaw-feet provided with swimming- (not walking-) extremities—are in the direct line of ancestral relationship to the recent scorpions, I may refer again to my paper 'On some Points in the Structure of the Xiphosura,' &c.:—'This is one very strong argument, to my mind, in favour of the higher zoological position of *Pterygotus*—that, being extremely larval in its anatomy, it consequently possessed the capacity for further development, and so has been modified and disappeared'—its latest representatives being met with in the Coal Measures, where the then earliest known examples of fossil scorpions had also been found. But the discovery, almost simultaneously, by Thorell and Lindström in Gotland; by B. N. Peach in Scotland; and by Whitfield in North America (in 1885) of actual pulmonated land scorpions in rocks of Upper Silurian age (as far back, in fact, in geological time as the earliest known occurrences of *Pterygotus*, *Slimonia*, and *Eurypterus*) indicates that the air-breathing scorpions were derived from a *still earlier* and as yet undiscovered aquatic progenitor possibly in Cambrian or pre-Cambrian times.

"Simultaneously with the commencement of my own work on the Merostomata, J. W. Salter undertook a monograph on the British Trilobites for the Palæontographical Society in 1864. No one who takes up this fine work of our old friend can avoid a feeling of regret that Salter's valuable life and splendid palæontological knowledge should not have been longer spared to us to carry on to its completion this most important service.

"Following up the progress of our knowledge of the trilobites, I may note that Dr. Henry Hicks made his first communication to this Society in 1865 on the genus *Anopolenus*, and between 1871 (when he came to London from the happy hunting-grounds of St. David's and joined the Geological Society) and 1876, he communicated to this Society a series of papers on the faunas of the 'Menevian,' the Lingula Flags, Tremadoc Slates, and Arenig series, giving descriptions of no fewer than thirty-four species of trilobites, belonging to eighteen genera, from those ancient rocks.

"But numerous as are these additions to our knowledge of the trilobites of Wales, they only represent a part of Dr. Hicks's discoveries, many of which were announced by Salter; the most important being that of the finding of a large *Paradoxides* at St. David's, proving the existence of a Middle Cambrian or '*Paradoxides*-zone,' coextensive with the vast area over which these early rocks have been observed, and occupying a persistent horizon throughout Europe and America.

"A brief reference must here be made to the papers published by that excellent geologist and naturalist, the late Thomas Belt, F.G.S., in 1867 and 1868, on new trilobites from the Upper Cambrian rocks of North Wales, and on the Lingula Flags or Ffestiniog group of the Dolgelly District, with figures and descriptions of four species of *Olenus* (non-*Conocoryphe*) and four species of *Agnostus* from Dolgelly. In 1888 I was so fortunate as to be able to record the first discovery of trilobites (*Conocoryphe viola*) in the Longmynd Group, Penrhyn quarries, Bethesda, near Bangor, in North Wales.

"The remarkable fauna of the *Olenellus* or Lowest Cambrian zone, originally discovered in America by Dr. Emmons in 1844, was first recognised in Europe by the late Dr. Linnarsson in 1871, in the basal zones of the Cambrian near Lake Mjösen in Norway, but its typical genus *Olenellus* was then referred by him to the allied but more recent genus *Paradoxides*. This referenc

was corrected by Prof. Brogger in 1875; and the various brilliant papers on the Primordial formations by this author have given the *Olenellus*-fauna a marked and peculiar interest. In 1882 Linnarsson next made known the existence of the *Olenellus*-fauna in Scania, at the base of the Swedish Cambrian. In 1886 the same fauna was detected by Mickwitz in the Lower Cambrian of Russia (Esthonia), and this Russian fauna was figured and described in detail by Dr. F. Schmidt, of St. Petersburg. In 1887 Dr. Holm reported the existence of the *Olenellus*-fauna in the Cambrian of Lapland, where it was first detected by Morstell in 1885. Thus the existence of this remarkable fossil group, the oldest well-marked fauna recognised by geologists in the Lower Cambrian, had already been demonstrated, in 1888, in three main regions, namely: (1) in the region of the Rocky Mountains; (2) in the region of North-eastern America; (3) in the region drained by the Baltic Sea. Up to 1888 no recorded account of the discovery of *Olenellus* from the British Isles had been published, the oldest fauna described being the overlying *Paradoxides*-zones or Middle Cambrian formation.

"The first recognisable traces of *Olenellus* in Britain were discovered by Prof. Lapworth in 1885. Further collections were made in 1887 and 1888, on the flanks of Caer Caradoc, Shropshire, and the species was named, in honour of Dr. Charles Callaway, *Olenellus Callavei*. Later on it was figured and described in the *Geological Magazine* for 1891.

"In August 1891, Sir A. Geikie announced, at the British Association meeting in Cardiff, the discovery of *Olenellus* by Messrs. Peach and Horne, in blue-black shales, a few feet below the "Serpulite Grit" of the Cambrian rocks of North-west Scotland, in the Dundonnell Forest of Ross-shire. The description of "the *Olenellus*-zone of the North-west Highlands" formed the subject of a most valuable paper by Messrs. Peach and Horne, read before the Geological Society on February 10, 1892, and a new species of *Olenellus* is described and named *O. Lapworthi* by these authors. Mr. B. N. Peach, F.R.S., communicated a second paper, "Additions to the Fauna of the *Olenellus*-zone of the North-west Highlands," on June 20, 1894; in which, in addition to *O. Lapworthi*, he describes and figures *O. Lapworthi* var. *elongatus*, *O. reticulatus*, *O. gigas*, *O. intermedius*, and *Olenelloides armatus*.

"'The Fauna of the Lower Cambrian or *Olenellus*-zone' forms the subject and title of an admirable monograph by Mr. C. D. Walcott, F.G.S., which, with the exception of the subsequent discovery of an *Olenellus*-fauna in the Lower Cambrian of the Scotch Highlands (already referred to), gives us a very complete and up-to-date account of this interesting and oldest fauna. About eighteen widely distributed localities are shown on the map of North America from British Columbia to Labrador, and as far south as Texas; whilst in Europe we have Spain, North and South Wales, the Scottish Highlands, Norway, Sweden, Finland, Bohemia, Bavaria, Podolia, Sardinia, Petchoraland, and the Ural Mountains. Omitting trails, burrows, and tracks, the *Olenellus*-fauna has yielded fifty-five genera of organisms, fifteen of which are Trilobites.

"We may now add yet another locality in which this remarkable fauna occurs, as proved by the presence of the remains of *Olenellus* and the pteropod *Salterella*; namely, in Western Australia, where it was discovered by Mr. Hardman in 1886.

"I must here refer to the discoveries of the limbs of trilobites. In 1870 the late E. Billings, the Palæontologist of the Geological Survey of Canada, brought before the Geological Society and described a specimen of *Asaphus platycephalus*, from the Trenton Limestone of Ottawa, Canada, exhibiting remains of eight pairs of limbs, corresponding with the eight free and movable segments of the body, and showing the hypostome still attached to the doublure of the anterior border of the cephalic shield; traces of two appendages under the caudal shield were also visible. On that occasion I exhibited a specimen of *Asaphus* from the same locality and horizon, showing one or two small 7–8-jointed palpus lying at the side of the hypostome apparently in its original position. After some remarks on the superficial character of trilobites, I added: 'The prominence of the hypostome in the trilobita reminds one much more strongly of the genus *Apus* than of the isopods, and it seems reasonable to expect in the trilobita a more generalised type of structure than that which marks the modern representatives of the class.'

"In 1881, after many years of untiring labour, Charles D. Walcott furnished most conclusive proofs of the existence of appendages to the cephalic, thoracic, and abdominal divisions of *Calymene*, *Ceraurus*, and *Acidaspis*. His researches have been carried on by the method of making thin transverse and longitudinal sections of rolled-up specimens. He has shown that the ventral body-wall of the trilobites was bounded inferiorly by a thin chitinous membrane, which was attached to the lower margin of the dorsal exoskeleton all round. This ventral membrane was supported by calcified arches, which gave attachment to the appendages beneath. He further established the existence of a row of articulated cylindrical limbs, on each side of the middle line. Walcott described the thoracic appendages in *Calymene* as slender six-jointed walking-legs (endopodites) with a single pointed termination, the basal segment giving rise to a branch appendage (exopodite). On each side of the throracic cavity he also described a row of bifid spiral appendages, of the nature of gills, and he suggested that branchiæ were attached to the bases of the thoracic limbs as well. The abdominal or pygidial rings carried appendages, a pair to each segment, but they do not appear to have differed from the thoracic limbs, save in size. The mouth is situated behind the hypostome, and has four pairs of jointed manducatory organs, the bases of which are modified to serve as jaws; the hindmost pair being the largest, and expanded at the distal extremity into a swimming-organ.

"The correctness of Billings's views, as to the nature of the thoracic limbs of *Asaphus platycephalus*, was further confirmed by the finding of a specimen of *Asaphus megistos*, in the Ordovician rocks of Ohio, which shows the under surface with its appendages, described by Dr. I. Mickleborough. This specimen shows two pairs of maxillipeds or jaw-feet, eight pairs of walking-appendages, corresponding to the eight pairs of free thoracic segments, each limb having about six joints. The under side of the coalesced segments of the abdomen (pygidium) reveals a series of from twelve to sixteen similar paired appendages, diminishing rapidly in size from before backwards to the extremity. A broad median groove extends along the under side of the thorax and abdomen, and probably represents the space once occupied by the sternites or, possibly, the straight intestinal canal, observed by Barrande in some trilobites from Bohemia. Traces of supposed branchial filaments have also been observed in this specimen, apparently attached to the thoracic legs.

"No further addition had been made to our knowledge of the appendages of triobites until July 1893, when Mr. W. D. Matthew, a student of Columbia College (N.Y.), communicated the result of his examination of several specimens of *Triarthrus Beckii*, obtained by Mr. W. S. Valiant from the 'Hudson River Shales' (Ordovician), near Rome, New York. After recording the extent of our previous knowledge derived from the important researches of C. D. Walcott, he proceeds to describe the additions which the specimens from Rome have supplied. These trilobites are found in a soft, fine, black shale, and are perfectly well preserved. The most noticeable character is the presence of long, slender, many-jointed whip-like appendages attached to the front of the head, closely resembling the flagellate antennæ of other crustaceans. These originate beneath the anterior border of the head-shield, and are as long again nearly as the glabella itself. Mr. Matthew also was able to detect a series of walking or swimming-legs, one a narrow, jointed, cylindrical leg, the other thin, broad, fringed with a comb-like structure similar to the gills of many crustacea.

"The next communication is from Mr. C. E. Beecher, of New Haven, Conn., 'On the Mode of Occurrence and the Structure and Development of *Triarthrus Beckii*.' The material gathered for the Yale University (by the aid of Prof. Marsh), near Rome, New York, is probably some of the best which has been obtained, and has been carefully examined and described by Mr. Beecher.

"In their present condition the specimens from Rome contain very little calcite, nearly the entire calcareous and chitinous portions of the trilobites being replaced by a thin film of iron pyrite. To this cause is doubtless due the preservation of delicate organs and structures which would otherwise have been destroyed.

"The specimens thus preserved occupy an extremely restricted vertical distribution, but within this range they are nearly all complete, and preserve their appendages. They are of all ages, from larval forms up to full-grown individuals, whilst the adjacent strata contain a rather sparse fauna in which the trilobites are generally fragmentary and without appendages. The author believes that, in the majority of beds in which trilobites are

found, the remains met with represent the exuviæ of living animals that have cast their shell, rather than the tests of dead individuals. In this particular deposit the appendages are apparently in the position which they occupied during life, and not such as would be assumed in the cast-off shells of recent crustacea.

"Mr. Beecher mentions another interesting point, namely, that nearly all the specimens are found with the back down, which is explained by suggesting that, although they lived with the ventral side downwards, the gases in the viscera produced during decomposition were sufficient to overturn the animal and allow it to be buried by the accumulation of the fine sediments in the position in which it is now found.

"The appendages of *Triarthus* appear now to be very well made out. The antennæ, as seen in a number of specimens, were simple multiarticulate flagella, which Walcott has shown extend backwards to the lateral margin of the hypostome, so that they occupy exactly the same position as do the first antennæ in recent *Apus*.

"Two small appendages, like simple palpi, with broad basal joints, which may represent the maxilla, are seen in one of Walcott's specimens, and there were probably four pairs of similar cephalic appendages, besides the simple flagellate antennæ, more or less modified to serve as mouth-organs.

"Each segment bears a pair of biramous appendages originating at the sides of the axis, as in other trilobites. The anterior legs are the longest, and the others gradually become shorter towards the pygidium. Each limb consists of two nearly equal branches, the 'endopodite' and 'exopodite,' which may be correlated with the typical crustacean primitive limb, and are well displayed in the adult *Mysis*; in the biramose natatory-feet of the zoea of the common shore-crab (*Carcinus*); and retained in the appendages of the abdomen of the adult lobster (*Homarus*). Practically, these biramose limbs are reproduced along the entire series of free segments. The appendages belonging to the pygidium closely resemble the branchigerous feet of *Apus*, and may evidently be correlated with typical phyllopod limbs.

"The first point insisted upon by all systematic zoologists—long before the finding of appendages had thrown so much new light upon our investigations—was that the great variability in the number of the segments in trilobites was a feature which distinctly connected them with the phyllopoda. Bernard considers of greater importance still the gradual diminution of the size of the segments posteriorly, which remarkable feature the trilobites share with *Apus*. I would also call attention to the fact that those earlier trilobites which best exhibit this large number of segments, such as *Olenellus*, *Paradoxides*, &c., are likewise remarkable for the simplicity and exact similarity of their segments, being a serial repetition of one another, and even the coalesced segments forming the head-shield share the same resemblance with the free posterior thoracic and abdominal ones. Bernard has given expression to the idea most aptly when he writes (*op. cit.* p. 412): "The adult is but the grown, not metamorphosed, larva—grown by the continual development of segments from before backwards, until at a certain stage this process becomes fixed, and we have the adult *Apus* with a number of fixed rudimentary segments. This fixation of a number of undeveloped segments is visible also in many trilobites.

"In the earlier forms (as *Olenellus*) these rudimentary posterior segments still remain free; but, as a rule, they are coalesced to form the plate-like pygidium so characteristic of the trilobites.

"Turning to the appendages, the simple multisegmented flagellate antennæ are extremely characteristic of the crustacea, being met with in lowly copepods and highly-developed decapods.

"The biramose paired limbs are quite a primitive type, like the segments to which they are attached, exceedingly simple, yet characteristic, and with the exception of the antennæ and the four succeeding pairs of appendages, which are modified to serve as mouth-organs (maxillæ and maxillipeds), the whole series are simple biramose natatory or walking-feet, such as persist still in adult *Mysis* and many other recent crustacea.

"The eyes in trilobites closely resemble those of other anthropods, but vary somewhat in position, and also in development, in some genera the eyes being altogether absent, as in *Ampyx*, *Ceraurus*, &c., whilst in others, like *Æglina*, they are enormously exaggerated in size. In some genera the eyes are hyaline, the faceted surface being covered with a fine transparent layer, whilst in others the facets appear prominently on the surface. It is suggested by Bernard that the minute pore observed in the head, near the compound eye in several genera (*Trinucleus*, *Acidaspis*, *Calymene*, *Ampyx*, *Griffithides*, *Phillipsia*, &c.), may be analogous to the pore in the head-shield of *Apus*, and be the opening into the water-sac covering the eyes; and whilst in some genera of trilobites this water-sac may have existed, it may have degenerated in others, leaving the eye in contact with the outer cuticle, which covered it like a thin transparent membrane. In none of the trilobites have larval eye-spots been observed.

"Dr. Lang held the view (in 1891) that if a fifth pair of cephalic limbs were found comparable with the anterior antennæ, trilobites might then be regarded as primitive entomostraca, to be derived from the same racial form as the phyllopoda.

"Walcott is of opinion that the trilobita formed a distinct branch, which diverged at a very early date from the phyllopoda, and having expended its vital energy in Palæozoic times it disappeared. He adds: 'Probably two thousand species and one hundred or more genera are known from Palæozoic strata. With this great differentation the initial vital energy of the group became impaired, and the trilobita died out at the close of Palæozoic time.

"I willingly adopt the view that the trilobita are ancestrally connected with *Limulus*; that *Limulus* may be related through *Hemiaspis* with *Eurypterus*; but all the intermediate forms have not yet been met with. That some ancestral Eurypterid must have given rise to *Scorpio* cannot, I think, be doubted; but it must have been in pre-Silurian times, for Peach and Lindström's *Palæophonus* had already appeared in the Upper Silurian of Lanarkshire and Gotland as a terrestrial pulmonated form, while a similar land-scorpion had been discovered by Whitfield in the Silurian of America.

"The Phyllopoda deserve consideration from a geological standpoint, a representative of *Apus* (*Protocaris Marshii*) having been met with in the Lower Cambrian of Vermont, U.S. Some of the living genera are naked (*Branchipus* and *Artemia*), but in most the front portion of the body is protected by a shield-like carapace (*Apus*), or it may be enclosed, as in *Estheria*, in a bivalve shell. The fossil remains of bivalved phyllopods, *Estheria* and *Leaia* were described by Prof. T. Rupert Jones as far back as 1862 in the Palæontographical Society, where he defines nineteen species ranging from the Old Red and Carboniferous upwards.

"The most ancient of these shield-bearing crustaceans, originally placed with the phyllopoda and having a single modern analogue (*Nebalia*), have now, by general consent, been removed and placed under the order Phyllocarida, a name suggested by Dr. A. S. Packard in 1879. The fossil forms referred to this order were originally studied and noticed by M'Coy, Salter, Barrande, Clarke, and have subsequently been fully described by Prof. T. Rupert Jones and myself.

"Metschnikoff, who studied the embryology of *Nebalia*, considered it to be a 'phyllopodiform decapod.' Besides the resemblance to the decapods, there is also a combination of copepod and phyllopod characteristics. The type is an instance of a generalised form, and is of high antiquity, having made its appearance in Cambrian times, when there lived (if we regard the relative size of most crustacea, and especially that of the living *Nebalia*) gigantic forms. Such was the Silurian *Ceratiocaris ludensis*, which was probably more than two feet in length.

"The modern *Nebalia* is extremely small, about ¼ inch in length, but a newly-described species, *Nebaliopsis typica*, Sars, measures as much as $1\frac{3}{5}$ inch, with the body compressed, and the carapace bivalved, as in *Limnadia*, one of the genuine phyllopods. There is a large movable rostrum overhanging the head: stalked eyes; the cephalic portion carries two pairs of antennæ and three pairs of special mouth-organs (mandibles and maxillæ); the thoracic segments bear eight pairs of short, leaf-like respiratory-feet, which are followed by six pairs of (abdominal) simple swimming-feet, four being large and two rudimentary, while the last two segments (seventh and eighth) are destitute of appendages, the body terminating in an elongated phyllopod-like caudal fork. Compared with *Nebalia*, the fossil forms give evidence of an articulated rostrum; traces of antennæ; the presence of a pair of strong mandibles; of a large expanded shield in some, and of a folded or bivalved carapace in others; of the presence of seven or eight body-segments, sometimes carrying branchigerous appendages, the terminal segment carrying a central caudal spine and two lateral shorter ones. It seems highly probable that the old giant pod-shrimps (*Ceratiocaris*, *Pithyrocaris*, &c.), whose remains occur in the Palæozoic rocks from the Cambrian to the Carboniferous, are represented by the

minute living *Nebalia*, and that these early forms may have given rise to, and have been the forerunners of, the modern Malacostraca. 'In *Nebalia*,' says Claus, 'we probably have to do with an offshoot of the phyllopod-like ancestors of the Malacostraca, which has persisted on to the present time.'

"The genus *Estheria* existed in the fresh and brackish waters of the Devonian Period, in Livonia, Caithness, and Orkney, and also in Nova Scotia and Scotland. It flourished in the European area at several of the Upper Carboniferous stages, and was well represented in the Secondary and Tertiary rocks: it is also living, and has a world-wide distribution.

"The Phyllocarida seem in some cases to afford examples of persistency of type, and in others of local or temporary specialisation. One of the oldest known is the Cambrian *Hymenocaris*, a prototype of the recent *Nebalia*. *Caryocaris* of the Arenig series possibly belongs to the same group; and the Upper Silurian *Ceratiocaris* carries the form to a high degree of perfection; but until we meet with the *Nebalia* of to-day we have no tangible links in this series in intermediate geological times. Walcott's Cambrian *Protocaris* is quite susceptible of being regarded as a predecessor of the living *Apus*. The Carboniferous *Dithyrocaris* and its allies stand probably in the relation of genealogical links. But much more research among these interesting lower crustacean fossils is required before their phylogenetic relationship can be fully elucidated.

"The Ostracoda, which have the entire body enclosed in a shell or carapace composed of two valves united along the back by a membrane (represented by such forms as *Cypris*, *Cypridina*, *Candona*, *Beyrichia*, *Primitia*, &c.), are chiefly dwellers in shallows, and occur both in fresh and salt water: they are usually of minute size; but there are deep-sea types which attain comparatively large dimensions (an inch long). They are met with in rocks of almost all ages from the Cambrian upwards. To speak of them here is to recall the nearly life-long labours (from about 1840) devoted to their elucidation by Prof. T. Rupert Jones, who has described many hundreds of these primitive crustacea from rocks of every British formation as well as from very many foreign countries.

"Great as are the transformations which these organisms have witnessed in the long cycles of geological change from Lower Cambrian to modern time, they present, nevertheless, a general facies, and (like the genus *Lingula* amongst the brachiopoda) must be looked upon as one of those persistent types which possess enormous power of multiplication, so that entire beds of rock may be said to be composed of their microscopic tests. The living species also possess exceptional powers of endurance and provision for the preservation of their lives in periods of drought, often retaining their vitality in a dormant state perhaps for years; thus they have persisted through all the vicissitudes of geological time, represented by the entire succession of the stratified rocks; 'all things changing, but themselves unchanged.'

"None of the older Ostracod genera exist now; but some of the existing forms of the Cyprididæ, Cytheridæ, and Cytherellidæ are fully represented by predecessors in the Palæozoic rocks. The wonderfully well-preserved *Palæocypris Edwardsii*, discovered by Dr. C. Brongniart, enclosed in transparent silica, displaying the soft parts of the animal as perfect as in life, from the Coal Measures of St. Etienne, is evidence of the existence of Cyprids in that far-off time.

"I have endeavoured to depict in a diagram (p. 115) the evolution of the Arthropoda in geological time.

"In concluding this brief excursion over the abysses of Palæozoic time, I have only been able to bring under your notice a few isolated points of interest in the crustacean fauna which lie in the depths of these ancient deposits. They may, however, serve to show that this group of lowly existences is not destitute of interest for the biologist. There may also be a possibility of connecting these isolated observations so as to show their bearing upon the greater question of the development of life.

"In order, however, to do this effectively I must ask you to accompany me next year in a second excursion over the newer Palæozoic and Kainozoic seas, where, nearer land and in shallower waters, we shall find a still greater variety of life-forms to study.

"Two conclusions may be drawn from our observations, namely, (1) that the ancient faunas of the earth were far more widespread, more ample and more uniform than are our recent faunas; and (2) if, as the researches of geologists seem to indicate, other sedimentary rocks exist, *older* than the Lower Cambrian, then we may hope to gather evidence of still earlier and more simple forms of life than are met with in the '*Olenellus*-zone.' We are fully justified in concluding that such must actually have existed, because we find in the Lower Cambrian evidence of a quite considerable fauna belonging to several divisions which, although lowly in themselves, are nevertheless already so clearly differentiated one from the other as to prove to us that we are still, both biologically and chronologically, very far removed from the commencement of life on the earth."

SCIENTIFIC SERIALS.

American Journal of Science, May.—On the colour relations of atoms, ions, and molecules, by M. Carey Lea. Part 1. The colour or absence of colour of an element is a function of its atomic weight. No element having ions coloured at all valencies can belong to the same natural group with elements having colourless ions only. The entire class of elements with colourless ions is divided into nine great natural groups, as follows:—H, F, Cl, Br, I; Li, Na, K, Rb, Cs; Ca, Sr, Ba; Sc, Y, La; Be, Mg, Zn, Cd, Hg; B, Al, Ga, In; C, Si, Ge, Sn, Pb, Th; N, P, As, Sb; O, S, Se, Te. This first great division of the elements includes all those whose ions function as anions, and also part of the cathions. Intermediate between the two chief divisions are eleven transitional elements, viz. Ti, V, Cu, Nb, Mo, Ag, Ce, Ta, W, Th, Bi. These have ions which at some valencies are coloured and at others colourless. These are cathions only. With atomic weights ranging from 1 to 47 the atoms are colourless; 52 to 59 coloured; 65 to 90 colourless; 103 to 106 coloured; 112 to 139 colourless; 145 to 169 coloured; 192 to 196 coloured. Elements whose place in the numerical series falls between these periods have both coloured and colourless atoms. The six heaviest metals at the end of the series are alternately coloured and colourless. Argon, Prout's hypothesis, and the periodic law, by Edwin A. Hill. A very interesting question connected with the discovery of argon is what will be the effect of these researches upon Prout's hypothesis? It is possible that argon has been an unsuspected cause of error, which, when properly allowed for, will show the ratio of H to O to be almost exactly 1 to 16. This would make so many atomic weights even or half multiples of H as to render probable the generation of the elements from a common form of matter by the continued addition of some one or more constant increments of mass.—Relation of the plane of Jupiter's orbit to the mean plane of 401 minor planet orbits, by H. A. Newton. The secular perturbation of the orbit of a minor planet by Jupiter is such that the inclination of the orbit plane is not greatly changed, but the node has a constant motion. Whatever may be the distribution of the poles of these orbits at one epoch, the tendency of the secular perturbation by Jupiter is to finally distribute them symmetrically around the pole of Jupiter's plane. The present inclination of the mean plane to Jupiter's plane is 0° 43'.

American Meteorological Journal, May.—The cause of cyclones, by Prof. A. Woeikof. The article deals chiefly with two points mentioned in a former paper on this subject by Mr. Dines. Dr. Woeikof considers that the balloon ascent from Munich on December 11, 1890, showed that, while there is no cooling of the free air in calm anticyclonic weather, the radiation of the surface of the snow cools the surrounding air, even on an isolated mountain. With regard to the suggestion that the latent heat set free by condensation is sufficient to cause a storm, he points out that the heat set free by copious condensation in India does not produce storms.—Meteorological problems for physical laboratories, by Prof. C. Abbe. Few physical laboratories have conveniences for studying aero-dynamics, but the author, with the assistance of Prof. C. F. Marvin, gives a list of thirty-seven subjects for experimental investigation which demand attention from meteorological students. Long range weather forecasts, by Prof. H. A. Hazen. The author puts forward a series of crucial tests of weather forecasts, more particularly with the view of showing the fallacy of the predictions based on the positions of the moon, planets, &c.—There is also an article by F. B. White on topographic influence on the winds of the weather maps, which frequently show erratic winds, having no dependence on the barometric gradients charted with them.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, May 10.—Captain W. de W. Abney, President, in the chair.—Mr. Herroun read a paper on the iodine voltameter. After referring to the usual methods of determining the value of the small currents used in calibrating galvanometers and other apparatus for measuring small currents, and discussing the errors to which they are subject, the author gave his reasons for selecting iodine. He did this since, with the exception of mercury in the mercurous state, iodine has the largest electro-chemical equivalent, and in addition, by titration with sodium thiosulphate, it is possible to determine the quantity of iodine liberated with a greater accuracy than can be obtained by weighing a deposit of copper or silver with the balance. The solution employed in the voltameter contains 10 to 15 per cent. of zinc iodide. If care is taken to leave a small piece of metallic zinc in this solution, no free iodine is liberated on keeping, unless the solution is exposed to a strong light for some time. The anode consists of a plate of platinum at the bottom of a tall and fairly narrow beaker. The wire leading the current to the anode is encased in a glass tube, so that the iodine is only liberated at the bottom of the beaker, where, on account of its great density, it tends to collect. The kathode consists of an amalgamated zinc rod, which, to prevent loose particles of zinc falling down into the iodine, is surrounded by a piece of filter-paper or vegetable parchment. In an electrolysis lasting for as long as two hours, none of the iodine is found to diffuse up to the part of the solution near the zinc kathode. Where, on account of the extreme feebleness of the current employed, it is necessary to allow the electrolysis to continue for longer than two hours, a U-tube is used with two small plugs of asbestos at the bend, the anode being in one limb and the kathode in the other. With this form of voltameter, even after the current has flowed for several days, no signs of iodine have been found in the limb containing the kathode. On account of the production of electric convection currents, the iodine voltameter does not seem to be quite so suitable for the accurate measurements of strong currents. After the current is stopped the zinc electrode is immediately removed, the solution stirred, and the amount of iodine liberated determined by titration with sodium thiosulphate. The author finds that a convenient strength of the thiosulphate solution is one in which one c.c. corresponds to the amount of iodine liberated by five coulombs of electricity. This solution contains 12·8375 grms. of pure recrystallised sodium thiosulphate per litre. It is possible to perform the titration to within 0·1 c.c., which corresponds to 0·5 coulomb, or, if the electrolysis lasts one hour, to 1/7200 ampere. In a comparison made with a silver voltameter, the current as deduced from the silver was 0·0264 ampere, and that deduced from the iodine 0·0266. The author considers that part of the difference may be due to the effect of oxygen dissolved in the solution of silver nitrate. Prof. Carey Foster considered this process for measuring currents a most valuable one. The idea of using a volumetric method for measuring currents was to him new. He did not, however, see the advantage of using a substance with a high electro-chemical equivalent if a volumetric method was going to be employed to estimate the quantity of the substance liberated. It would be possible to use a chloride, though in this case the titration would probably be less accurate. Prof. Silvanus Thompson said he thought the method would be very valuable, but he would like to know if any error was likely to arise if too great a current density was employed. The number the author had assumed for the atomic weight of silver (108) was only approximate; if the more accurate value (107·7) were used, the agreement between the results obtained with the silver and iodine voltameters would be improved. Mr. Trotter asked what was the largest current that could be accurately measured. Mr. Enright said he had used porous diaphragms in iodine voltameters, and found that the iodine collected in the positive compartment, while the water was driven over into the negative compartment. With strong currents it was possible to get almost pure iodine left in one compartment. Mr. Watson thought that, since the value for the electro-chemical equivalent of iodine used by the author was deduced from Rayleigh's value of the electro-chemical equivalent of silver, and that Rayleigh's experiments were performed in air, the difference obtained with the silver and iodine voltameters could hardly be due to the cause suggested. Mr. Elder warned the members that volumetric measurements were not so accurate or easy as they seemed. He particularly mistrusted a solution of sodium thiosulphate, since he had found a solution of this substance to change even in twenty-four hours. The difficulty of accurately reading the burette might be overcome by weighing the burette and its contents before and after the titration. The author in his reply said that with the size of electrodes he used (about 9 sq. cm. surface) 0·1 ampere was the maximum current it was safe to use. The only substance likely to be produced by too great a current density was periodate, which, since it was insoluble, would immediately be noticed. The influence of the dissolved oxygen was only appreciable with small currents where the electrolysis lasts some time, while in Rayleigh's experiments large currents were employed. The chairman, while returning thanks to the author for his paper, mentioned that in his experience he had found zinc salts to be very untrustworthy.—Mr. A. Sharp read a paper entitled a new method in harmonic analysis. The author, in this paper, applies the principle of the form of harmonic analysis for giving direct readings of the amplitude and epoch of the various constituent harmonic terms, previously described by him, to the performance of harmonic analysis without the use of an instrument. The kinematic principle is as follows: Let the curve to be analysed be drawn with a scale of abscissa such that the period is 2π. Let a wheel w roll on the paper and be connected with a tracing-point P in such a manner that as P moves uniformly in the x direction the axis of the wheel w turns uniformly counter clockwise in a horizontal plane, and the distance rolled through during any short interval is equal to the corresponding displacement of the tracer P in the y direction. The curve traced out by w the author calls the roller curve, and from the vector joining the initial and final points of this curve the amplitude and epoch can be determined. Suppose the periodic curve consists of a portion of the curve $y = a_0 + a_1x + a_2x^2 + \quad + a_mx^m$ repeated over and over again. Then, if the tracer is taken round this periodic curve you get a rolled curve which may be called the first rolled curve. If now the curve whose ordinates are $\frac{dy}{dx}$ is traced out, the roller curve obtained is the evolute of the first, and so on for $\frac{d^2y}{dx^2}$, &c. The author gives two worked examples, and compares the values of the coefficient obtained with those given by the harmonic analyses of the Guilds Central Technical College. Prof. Henrici said he had not received the paper in time to thoroughly master it, but he thought that, at any rate for curves where no discontinuity occurred, the relation found by the author between the roller curves was always true, the last evolute being a point, and the one before that a circle. The interesting point was whether the method was capable of being used for practical purposes, for it occupied a place with respect to harmonic analysis similar to that occupied by Simpson's rule in planimetry. Prof. Silvanus Thompson asked if the author had devised a form of mechanism capable of fulfilling the kinematical conditions given at the commencement of the paper. The author in his reply said he had devised such a mechanism, and that it was described in his previous paper. In addition he had since invented a more practicable form which he had patented. The chairman said the Society ought to congratulate itself on the large number of important papers dealing with harmonic analysis and planimetry that had lately been communicated.

Malacological Society, May 10.—Prof. G. B. Howes, President, in the chair.—On behalf of Miss de Burgh specimens were shown illustrating the variation of *Columbella mercatoria*, Linn.—Mr. Da Costa exhibited a collection of univalve mollusca from Lakes Tanganyika and Victoria Nyanza, and pointed out the entirely different characters of the molluscan fauna of these two lakes.—On behalf of Mr. C. S. Cox were exhibited living specimens of *Glandina* from Italy.—Mr. E. A. Smith exhibited an almost complete collection of the land and freshwater mollusca of St. Vincent, W.I. Mr. E. R. Sykes exhibited specimens of *Achatinella variabilis*, Newc, and allied forms, from the Island of Lanai. The following communications were read:—Notes on *Trochonanina* and other genera of the land mollusca, with reference to the animals of *Martensia Mozambicensis*, Pfr., and other species, by Lieut.-Colonel H. H. Godwin-Austen.—Report on the land and freshwater shells collected by Mr. H. H. Smith at St. Vincent, W.I., by E. A. Smith.—Note on the larval oyster, by M. F. Woodward.

Victoria Institute, May 6.—Dr. Chaplin in the chair.—A paper on the so-called *Pithecanthropus* of Dr. E. Dubois was read by Prof. E. Hull, LL.D., F.R.S., after which a paper by

Sir J. W. Dawson, C.M.G., F.R.S., on the physical character and affinities of the Gaunches, or extinct people of the Canary Islands, illustrated by photographs, was read. In it the author reviewed the historical facts as to the Canary Islands and these inhabitants, the characters of the crania found, and the weapons, ornaments, &c., and described the conclusions he had arrived at with reference to the relationship of the Gaunches to ancient peoples of Western Europe and Africa, and their possible connection with the colonisation of Eastern America.

Royal Microscopical Society, May 15.—Mr. A. D. Michael, President, in the chair.—Mr. J. Swift exhibited an improved form of the Nelson microscope-lamp, fitted with mechanical movements; and also a Wales microscope which had been fitted with the new mechanical stage.—Mr. T. Comber read a paper on the development of the young valve of *Trachyneis aspera*. The subject was illustrated with lantern photographs exhibited upon the screen.—Miss Ethel Sargant's paper, "On the first nuclear division in the pollen mother cells of *Lilium martagon*, &c.," was communicated by Dr. D. H. Scott.

PARIS.

Academy of Sciences, May 20.—M. Marey in the chair.—The decease of M. C. Ludwig, correspondent of the Medicine and Surgery Section, was announced by the President. M. Ludwig will be chiefly remembered for his work on blood pressures and circulation, on artificial circulation, and on the physiology of the nervous system.—Reduction to sea-level of the values observed for gravity at the surface of the earth (Coast and Geodetical Survey), by M. G. R. Putnam. A translation of some passages of this work is given by M. H. Faye, in which it is shown that Faye's correction causes anomalies to more nearly disappear than Bouguer's correction. M. Faye then discusses the probable form of the earth's crust, and shows the bearing of his discussion on the theories of geologists.—New researches on the thermochemical relations between aldehydes, alcohols, and acids, by MM. Berthelot and Rivals. A *résumé* is given of the known thermochemical data connecting aldehydes with corresponding alcohols and acids.—Existence of phosphorus in notable proportion in oysters, by MM. A. Chatin and A. Müntz. Not only has phosphorus been found in the shells of different kinds of oysters in the form of tricalcic phosphate, but organic phosphorus has been found in oyster flesh in quantity, more in Portuguese oysters (*Gryphea angulata*) than in French natives (*Ostrea edulis*).—Classification of the chemical elements, by M. Lecoq de Boisbaudran. A theoretical paper discussing the author's system of classification and the genesis of elements from a primordial matter.—On the spectroscopic analysis of gases obtained from various minerals, by Mr. Norman Lockyer.—On the reducing properties of sodium alcoholates at a high temperature, by MM. A. Haller and J. Minguin. The results of heating together in sealed tubes at about 200° C. are given for: desoxybenzoin and sodium ethylate in absolute alcohol; benzophenone and sodium ethylate; anthraquinone and sodium ethylate, amylate, and butylate respectively.—On stereoscopic projections and the "stereojumelle," by M. Moessard.—Studies on the activity of the diastole of the ventricles, on its mechanism, and its physiological and pathological applications. An abstract of a memoir by the author, M. Leon Germe.—A comparison between the spectra of the gases from cleveite and the spectrum of the solar atmosphere, by M. H. Deslandres. A list of wave-lengths of lines observed in the spectra of gases from cleveite is compared with a similar list of lines observed in the solar chromosphere spectrum (see p. 56). Twenty lines in the former list are recorded and thirteen lines in the latter list are shown to have the same wave-length, extending through the luminous and ultra-violet portions of the spectra. Two *permanent* chromosphere lines, 587·60 and 447·18, correspond to two of the principal gas lines, 587·60 (D_3) and 447·175. There now remain but two such chromosphere lines always obtainable, which do not correspond to lines obtained in terrestrial spectra.—On the isomeric transformations of mercury salts, by M. Raoul Varet. It is shown that black amorphous HgS disengages +0·24 Cal. in changing to the red amorphous variety, and yields a further +0·06 Cal. in becoming red crystalline HgS.—Action of nitrogen peroxide on the halogen salts of antimony, by M. V. Thomas.—Heats of formation of benzoyl chloride and toluyl chloride, by M. Paul Rivals. The substitution of the group (COCl) for a hydrogen in benzene or toluene results in an increase in the heat of formation of +58 Cal. and +55·3 Cal. respectively.—Study of senecionine and senecine, by MM. A. Grandval and H. Lajoux. Two alkaloids have been prepared from *Senecio vulgaris*. Senecionine appears to have the composition $C_{18}H_{26}NO_6$, and does not possess very marked reactions. Senecine appears to possess much more definite reactions with the usual alkaloid reagents.—On phenyl-sulpho-orthotoluidine and some of its derivatives, by M. Ch. Rabaut. In conclusion, attention is drawn to the resistance of this substance to oxidation and to its great stability in presence of dilute acids and heat, notwithstanding its amide character.—Analysis of a mummy bone, by M. Thezard.—On a leucomaine extracted from urine in cases of Angina pectoris, by M. A. B. Griffiths and C. Massey. A new poisonous base, causing death in two hours, of which the composition is given as $C_{10}H_9NO_4$.—On some improvements in the preparation and study of thin plates of sedimentary calcareous rocks, by M. Bleicher.—On the anomalous divisions of ferns, by M. Adrien Guébhard.—The catastrophes of Titel in the Banat and of Mendoza (Argentine Republic), by M. Ch. V. Zenger. Arguments are adduced to show a connection between these seismic phenomena and sun-spot appearances on the sun.—The use of crude petroleum for prevention of incrustations in boilers is advocated by M. G. Liévin.

BOOKS AND SERIALS RECEIVED.

BOOKS.—The Telephone Systems of the Continent of Europe: A. R Bennett (Longmans).—The Theory of Light: Prof. T. Preston, 2nd edition (Macmillan). Dental Microscopy: A. H. Smith (Dental Manufacturing Company).—A Reader's Guide to Contemporary Literature: W. S. Sonnenschein (Sonnenschein).—A First Book of Electricity and Magnetism: W. P. Maycock, 2nd edition (Whittaker).—The Way about Middlesex (Iliffe).—The Way about Hertfordshire (Iliffe).—An Elementary Treatise on Elliptic Functions: Prof. A. Cayley, 2nd edition (Bell).—La Pluie en Belgique: A. Lancaster, Premier Fasc. (Bruxelles, Hayez).—Nature in Acadie: H. K. Swann (Bale).—The Linacre Reports, Vol. 2: edited by Prof. E. Ray Lankester (Adlard).

SERIALS.—Botaniscbe Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie, Zwanzigster Band, 4 Heft (Leipzig, Engelmann).—Zeitschrift für Wissenschaftliche Zoologie, lix. Band, 2 Heft (Leipzig, Engelmann).—The Evergreen, Spring (Unwin).—American Naturalist, May (Philadelphia).—Papers read before the Engineering Society of the School of Practical Science, Toronto, No. 8, 1894–5 (Toronto).—Good Words, June (Isbister).—Sunday Magazine, June (Isbister).—Longman's Magazine, June (Longmans).—Chambers's Journal, June (Chambers).—Century Magazine, June (Unwin).—Journal of the College of Science, Imperial University, Japan, Vol. vii. Part 4 (Tōkyō).—Journal of the Institution of Electrical Engineers, No. 117 (Spon).

CONTENTS.

THURSDAY, JUNE 6, 1895.

THE "CHALLENGER" EXPEDITION AND THE FUTURE OF OCEANOGRAPHY.

The Voyage of H.M.S. "Challenger." A Summary of the Scientific Results. (With Appendices). Two Parts. (London: Eyre and Spottiswoode, 1895.)

THE two new volumes of the *Challenger* Expedition have appeared, and with them this momentous enterprise has arrived at its final close. It is well worth our while to seize this occasion for a few words of reflection on a scientific drama, which is equally great in all its parts and dimensions, as in the effects it has produced and will go on to produce, on the progress of a group of sciences which every day grow more important in their influence on human intellect and thought.

It is nowadays a very common complaint, that specialisation in scientific pursuits threatens to do away with that character of universality that was attributed in former times to all those who busied their brains with the phenomena of nature. I can fully remember how, in my own childhood, the naturalist κατ᾽ἐξοχήν found his example in Alexander von Humboldt. He was credited with "knowing everything," and whoever followed some small pursuit as a naturalist, partook, in a certain degree, of the prestige the great "Naturforscher" enjoyed in all circles of the reading public. When I was studying zoology at Jena, a fellow-student of divinity asked me once, "Please tell me what is the name of those stars?" "I don't know, my dear friend, I am studying biology." "Oh, I thought you 'Naturforscher' study all the natural sciences."

I am afraid we are at present drifting far away in the opposite direction, and the general public is rather inclined to believe that each naturalist or natural philosopher lives on an island, of which he investigates only a small corner, without caring a bit for the rest of the island, and still less for other islands and whole continents. Whether we are quite as bad, I will not try to decide; certainly those happy times are far behind us when a professor of mathematics and astronomy taught also physics and medicine, or when botany, zoology, and chemistry were represented by the only professor of medicine, and all these things were taught merely by books and traditions. But even those modest cases of personal union between zoology and botany, or between geology and zoology, which not unfrequently occurred in the first half of our century, have passed away now at its close. Instead of such personal unions, we meet with, in a well-equipped university, distinct chairs for zoology, comparative anatomy, embryology, palaeontology, geology, mineralogy; round each of these chairs we see gathering numbers of privatdocents and other teachers, who deliver lectures on distinct specialities of these sciences, which threaten to grow themselves again to independent divisions craving a chair for themselves. "Division of labour" is all very well; but if we do not in time prepare for better mental digestion and assimilation, the next century will live to see a new Babylonian turret; dispersion of languages will grow to such a degree, that even the inhabitants of the same scientific island will find it hard to talk to each other.

It is a consolation, under these circumstances, to see, that, along with division of labour, combination of labour takes its firm hold in the organisation of modern scientific life, and Moltke's maxim, "march separately, attack jointly," proves also useful in the peaceful battles of thought and science.

A splendid proof of this combination of labour lies before me in the numerous volumes of the *Challenger* Expedition. Physics, chemistry, geology, zoology, and botany, and all those nautical and hydrographical attainments of modern date, have combined to produce results which close a past of unwarranted belief, and open a future of new research, boundless in fertility of problems and of unknown possible effect on the human intellect and understanding.

The imagination of human kind from the beginning of historical ages, and along all its phases of development and evolution, took hold of those unknown regions of the heights of mountains as well as of the depths of the ocean. Covered by ice and snow, hidden in thick masses of clouds, out of which thunder and lightning and endless floods of rain and hail came forth, the ranges of mountains gave birth to the grandest and most appalling visions of powers, upon which the poor human individual looked aghast, against whose mighty influences he felt helpless, and whom he dreaded and revered. Every human being becomes a poet under the influence of fear and reverence. Both magnify and intensify impressions, even of the most common kind, and create combinations where the acutest observer could not discover any connection. Thus the oldest forms of religious belief, as well as the numerous forms of still existing superstitions, have peopled the tops of mountains and the depths of the seas with images of supernatural powers; the Olymp of Hellas, and the old German myths, the Hebrew Jehovah, and the rudest Paganism, found their abodes beyond the clouds, and below the waters. And who can resist the temptation of such dreams, grand and awful at once, when standing on those solitary heights of the Alps, with ice and snow, and rock and cloud around him and below him, and looking over endless ranges of peaks and valleys? Who is not struck by the image of death and destruction, when he wanders on the volcanic deserts of Etna, where there is not one leaf of grass, not one smallest insect to keep him company? And in the midst of the raging ocean, with waves dashing against the poor ship, and clouds spreading darkness around, who will refrain from images of terror created by the imagination of the boundless depths to which he has trusted his life? Will there ever come a time when the human mind replaces such emotions by the cool reflection that the minimum or the maximum of atmospheric currents and pressure causes the disturbance of equilibrium on the floods of the ocean to such a degree as to shake the balance of the floating mass of wood or iron, on which he happens to find himself, and bring its metacentre to a position which enables the water to supplant the air-filled spaces until the greater specific gravity of iron carries all away, through the lamina of the hydrosphere, down to the lithosphere, which resists further gravitational concurrence? And will ever barometer and thermometer, or the observing eye of the geologist, caught by phenomena of denudation or glacial erosion on Mont Blanc, diminish the trembling of

emotion when the eye measures the enormous distances it commands from such a height? Whoever has experienced the thrilling delight of that other emotion caused by insight and discovery; whoever knows that intellectual powers can produce as much enthusiasm as artistic and æsthetical emotion, will not be haunted by the sickening dread that human imagination could become stripped naked by the impious hand of science. Whoever cares more for the Why, than for the How, will gather around the temple of science; but those gifted natures, who are impressed by colours, shapes, and situations, why shall they not go on to shake their kaleidoscope of beauty and appearance, just as much as these go on drawing invisible threads of cause and effect between old and new facts?

Let us therefore not quarrel with the natural growth of the human mind, but rather accept in delight all such actions as include a great increase of knowledge in regions where ignorance lent the hand to superstition; and so let us hail the work of those who lifted a piece of the thick veil that covered the abyssal depths of the ocean.

It will always be one of the greatest of the many merits of the late Prof. W. B. Carpenter to have given the first suggestion to the *Challenger* Expedition. Not content with asking the Council of the Royal Society to throw in its authority with the British Government to undertake a new and complete course of research for the exploration of the deep sea, he entered into direct correspondence with the First Lord of the Admiralty, and carried his point so far as to receive the answer that the Government would be prepared to give the requisite aid in furtherance of such an expedition on receipt of a formal application from the Royal Society—in consequence of which answer the Royal Society at once proceeded to take these necessary steps; and after exchanging some correspondence with the Secretary of the Admiralty, the proposal to defray the expense of such an expedition out of the public funds was brought before Parliament and "received the cordial assent of the House of Commons" in April 1872.

It is to be lamented that in the "Narrative of the Cruise," neither the proposition of the British Government nor the debate of the House of Commons are reproduced literally. It would have been of high historical interest to the general, as well as to the special, reader to know exactly the wording in which the proposition was formed, and the views and opinions with which it was received. It is, perhaps, not possible to the editor of NATURE to supply even now this omission, but yet many in the outer world would greatly desire a reprint of the day's discussion which produced results so momentous as that great and memorable expedition of the *Challenger*. In uttering this regret, I can assure the British reader that, though a foreigner, I feel deeply my share of gratitude to both Government and Parliament of Great Britain. I cannot omit this occasion to congratulate science for having her wants so well interpreted, understood, and satisfied by all those who have a share in the *Challenger* Expedition, be it the Government or Parliament, be it the officers and crew of the ship, or the scientific staff and the authors of the voluminous reports lying before me.

And I may be permitted to claim some personal license to proffer my thanks in the name of science, and especially of biological science; for at the time when Dr. Carpenter and the Royal Society asked the British Government to undertake the expedition, I was myself engaged in a collateral enterprise of similar tendency, and felt the same necessity to ask for help and assistance of the authorities of the German Government, and, in smaller degree, of the Governments of almost all civilised States and nations. A few years after the British House of Commons had "cordially assented" to the proposition of the Royal Society, and voted the funds demanded by the Admiralty, the German Reichstag passed a resolution, based on a petition of Helmholtz, Dubois-Reymond and Virchow, by which the Government of the empire was asked to grant an annual subvention of £1500 to the Zoological Station of Naples, a subvention not only continued up to this date, but four years since increased to £2000. These votes of the two great parliamentary bodies go far to disprove the old doctrine, that science and the promotion of research are to be abandoned to private enterprise, and to the favours they may meet with accidentally in raising money by public subscription, or falling in with wealthy private persons whose interest and generosity can be won over. I am afraid, if the House of Commons had not granted the necessary funds, the *Challenger* Expedition would never have taken place, and our ignorance about the many great and innumerable smaller questions connected with the deep-sea problems would be still the same as in 1872. Had not the German Reichstag voted in favour of the Zoological Station, all my personal efforts would have failed, and neither the Naples Station nor the Plymouth Laboratory, nor, perhaps, the many other imitations of "the big brother at Naples," would have had the chances with which they have met now. No; let science not be immodest and ask for all and everything from the State; but let it still less linger on and wait for the chances, growing always scarcer and scarcer, of being endowed by private source, be it public subscription or donation from wealthy men and amateurs. The number of persons combining great wealth with sufficient culture is unfortunately not on the increase; inherited wealth, which offers more chance for the acquirement of higher intellectual pursuits, is decidedly diminishing. The demand for funds for the endowment of research is doubtlessly augmenting, and the competition in the advancement of science is such, that the nation which is not ready to pay its share, will either be thrown in the background, or live like a parasite on the intellectual blood of its neighbours. How long such a parasitic existence could be protracted, remains to be seen; but certainly no great nation will deliberately accept such a disgraceful situation, the more since it cannot be doubted that each nation has its peculiar gifts and talents, which make its co-operation indispensable in the chorus of other nations and in the interest of humanity. It must be granted, that the weight of a nation in the scale of culture depends on the power and number of men of genius it has produced and goes on to produce; it may also be granted, that a genius has been known to open up his own ways and make his career through all the adversities of fate. Yet a genius needs to feed quite as much, or perhaps more than an ordinary mortal, and some think it would be economical

to give him at least average chance. Would any genius have been capable of diving, on his own account, to the great depths of the Pacific? or would a genius find it possible to replace, by his own work, the ant-like activity of the Naples Station? Hardly. But let him come now and handle the innumerable data of the *Challenger's* investigations, or use the opportunities offered by a modern laboratory to give us a solution of the problem of heredity, or decide whether natural selection suffices to account for the evolution of the organic world, or whether other principles must be sought. The genius of Pasteur and Lister and Koch have opened the enormous field of research regarding the nature and effect of bacteria, and I think the world has not been the worse for France and Germany spending public money for the equipment of large laboratories to enable those geniuses to continue, in the most effective way, their labours.

Certainly not every whim or fancy of a learned individual can be accepted as a sufficient reason for spending public funds; some sort of a controlling apparatus will always be necessary. But in the Royal Societies, National Academies, and other learned bodies of high standard, each nation has already what is wanted, and it is understood well enough that such bodies are often even more difficult to be won over by some new rising genius than a Minister of Public Instruction or the outside public. It is, therefore, not to be anticipated that from the Scylla of nihilism in officially supporting research, one must necessarily glide down into the Charybdis of supporting whatever scheme comes out of the fervid brain of a young discoverer. But this much can be said, or repeated over and over again—for it is certainly no new truth—that the mental and intellectual productions of a nation ought not to be the last, nor the least, in their claims on the public money; and it may be maintained with all confidence, that hardly any other expense will so amply repay the budget of a nation, both materially and ideally, as the funds handed over for the promotion of research, or, in the truer expression, for the *organisation* of research.

For it is in this, that the real future lies: in organisation. Being organised, the small Japanese empire was more than a match to the tenfold bigger Chinese mass: being organised, a few British regiments can keep populations in abeyance, which, if they were equally well organised, might crush them in a moment. And to be organised, even in the intellectual sphere, means to economise natural powers and not throw away chances, which if they cannot perhaps be brought about deliberately, nevertheless can be profited by when they occur—and they occur always and everywhere.

Organisation of research, will, I do not doubt, become the special feature of the coming century. It would be well worth to provoke discussion about schemes, ways and channels, into which organised research ought to grow. Each nation may adopt its own, according to its character, habits, and prejudices. But one feature ought to be observed with them all, for it will soon become uppermost; that is, *international organisation* of those interests and productions by which all the nations may be benefited together, without being forced to arrange separately, each for itself, what more effectually and with less material and intellectual effort can be provided for all of them at once. And there can be no doubt that foremost, in this regard, stands the question: *How to reorganise, or organise at all, scientific publication?*

It cannot be doubted that the way in which we deliver over to publicity at present the results of the work of hundreds and thousands of investigators, is all but destitute of any regulating principle. Publishing in the nineteenth century resembles very much the kind of warfare practised in bygone times, when regiments were the property of single individuals, who were responsible for their equipments, nourishment, efficiency, and who entered into contracts with their men and soldiers and with states and princes. Defection on the one side, plundering on the other, were concomitant features of such arrangements, which one only need compare with the present constitution of the Prussian army to feel at once what powerful element organisation has proved to be. Why shall the most subtle of human activities, the mental and intellectual functions, not be liable to profit in the same degree by organisation? Why shall prejudice and egoism be permitted to govern with almost absolute sovereignty in the lofty regions of thought and speculation, of experiment and observation—in one word, of research? Organisation is not pedantry, discipline not slavery, genius no direct contradiction to order and measure. Originality and individualism will neither be sacrificed nor diminished, if certain rules are observed in bringing the results of investigation to public knowledge, and a better, more economical, and more effective system of reporting and recording is adopted, with the intention to facilitate the communication of valuable scientific results over the greatest possible circles of competent readers. It is true that the all-powerful *vis inertiæ* will go far in opposing any serious attempt of reorganisation in this department; but, as I remarked at the commencement of this article, unless we put hands and shoulders to the work, we shall unavoidably arrive soon at a state of chaotic confusion, where the worse elements may be conspicuous, and valuable productions at times be choked among mediocrity.

It would lead me too far away from the direct subject of this article to develop here any scheme of better arrangement for scientific publication; and if I am not mistaken, the feeling that such arrangements ought to be found and to be universally introduced, is spreading rapidly among competent and conscientious men of science. Let these soon unite and form national and international centres for the organisation of scientific publication—a more wholesome influence on the progress of science and research can hardly be imagined nowadays.

The two new and last volumes of the "*Challenger* Report" are the work of Mr. Murray, the true soul of the expedition, to whom science owes a great debt of gratitude for his never-ceasing care and toil, and for his talent and amiability, with which he undertook the great burden of superintending the publications of the expedition, besides himself adding most remarkably to the vast amount of new knowledge regarding the deep sea.

In the "Editorial Notes" to these two volumes, Mr. Murray has some paragraphs on the whole expedition so characteristic that I think it right to repeat them here to every reader who does not happen to lay his hands on the volumes themselves. Mr. Murray, after having given

an account of how in general the collections and the reports on them were disposed, adds the following :

"From beginning to end the history of the *Challenger* Expedition is simply a record of continuous and diligent work. There were few opportunities for brilliant exploits during the voyage. The daily and hourly magnetic and meteorologic observations, the handling of the ship during the tedious deep-sea investigations, the work connected with the boat excursions and expeditions on land, in addition to the usual operations of the marine surveyor and navigator, all demanded from the naval officers and seamen an amount of care and attention far surpassing what is required during an ordinary commission of one of Her Majesty's ships. The labour connected with preserving, cataloguing, and packing the biological and other collections on board ship was enormous, so also was that involved in their subsequent examination on the return of the expedition and their distribution to specialists in many parts of the world. All this was, however, accomplished with success, and the typical collections have now been deposited without any mishap in the British Museum. The majority of the authors of the special memoirs have spent years in the examination of the collections and in the preparation of their manuscript and illustrations for the press, without other remuneration than either a copy of the *Challenger* publications or a small honorarium to cover the outlay necessitated by their researches. The payments of the civilian staff have been very moderate, and in my own case, at least, have not covered actual expenditure in connection with the work of the expedition.

"The great difficulty in carrying through an undertaking of this nature arises from considerations of time. The researches of the specialist tend ever to become more elaborate : in no case were the authors of the larger special reports able to terminate their work within the original estimates as to time and bulk. The limitations in reference to expenditure imposed on me by the Government often rendered it imperative to curtail the investigations, and to cut out matter from the memoirs when, in other circumstances, I would gladly have fallen in with the views of contributors and collaborators. The researches and publications connected with the expedition might have been extended in several directions with advantage to science had the allotted time and funds permitted ; as it is, a few collections have not been thoroughly examined, and some observations have not been fully discussed.

"In June, 1872, I was appointed one of the naturalists of the *Challenger* when the expedition was being fitted out. During the past twenty-three years my time has been wholly taken up with the work of the expedition and in the study of those subjects which the expedition was organised to investigate. The direction of the whole of the work connected with the publication of the scientific results passed unexpectedly into my hands, and I have done my best in the circumstances to place on permanent record a trustworthy account of the labours of this famous expedition. It has been my earnest endeavour to complete the publications in a manner worthy of the naval position and scientific reputation of this great empire. Notwithstanding the troubles, personal sacrifices and regrets necessarily connected with the work, it has been a pleasure and an honour to have taken part in explorations and researches which mark the greatest advance in the knowledge of our planet since the celebrated geographical discoveries of the fifteenth and sixteenth centuries."

It is hardly possible to speak in a more truthful, simple, and dignified manner of one's life's work than here Mr. Murray speaks of the work and the difficulties that beset the *Challenger* Expedition, "cujus pars magna fuit." He might have used quite other language, and have felt sure to meet the full acknowledgment of his contemporaries, and nobody will certainly dispute him the proud sentence with which he finishes the above account. There can hardly be any doubt about the epoch-making importance of the *Challenger* expedition, and if in the first letter of Dr. Carpenter to the Royal Society attention is drawn to an article in this journal (NATURE, vol. iv. p. 107, 1871), in which it was stated that the Governments of Germany, Sweden, and the United States were preparing to dispatch ships to various parts of the ocean, expressly fitted for deep-sea exploration, and the question put forward, whether Great Britain should not step in to do her share in such work, I think it might well be urged now, after Great Britain having done her work in the most unparalleled way, that other nations might continue and profit by the experience of the *Challenger*. Such expeditions may be undertaken by deliberately dividing the task of filling the gaps and lacunes left by the *Challenger*, one nation taking the Atlantic, the other the Indian, a third the Pacific, and a fourth especially the Antarctic Sea for its investigation and exploration. A large basis has been laid by the *Challenger*, capable of bearing any superstructure to be erected on it. Let France and Germany, the United States and Russia take up this work after a mutual understanding, let Sweden or Norway explore once more the North Polar Sea, Italy the Red Sea, and let international organisation add a second chapter to oceanography, after the first has been so well worked out by Great Britain.

Nevertheless, whatever important results may be arrived at by such repeated expeditions, embodying both principles—division of labour and combination of results—the future of oceanography requires still other means of research. Whenever a new domain of science is opened up, either by the isolated work of a discovering genius, such as Pasteur and Koch, or by combination of rarely found chances, such as the *Challenger* Expedition offered, the immediate consequence is that specialisation sets in to work out all the different chapters of the new doctrine, enlarging the basis, multiplying the parts, drawing new conclusions, correcting old ones—in short, bringing about a detailed colonisation of the newly-discovered intellectual areas. But no oceanic or African colony can live and prosper nowadays without well-established communication with its motherland ; no haphazard visits of travellers can supplant the permanent and systematic exploitation that alone provides those conditions of life which make a colony prosper. And the same holds good for intellectual colonising, and especially for problems of oceanography.

If we look over the fifty volumes of the "*Challenger* Reports," we see, at once, that the lion's share belongs to biology. More than nine-tenths of them are purely biological, and almost all the others include some important biological elements. It is therefore hardly wrong to suppose that the future of oceanography will lie with biology, and with its ways and means for increasing our knowledge. The problems of biology, of course, are extremely varied, and many of them may be studied in every inland university. Not so the problems of marine biology, for which the last twenty years have established

the utter necessity of laboratories near the sea-shore. Here we are only in the beginning of a movement, which will go far to increase our knowledge of the conditions of marine life.

If the establishment of marine laboratories on different parts of the Mediterranean and on both sides of the Atlantic—not to speak of the North Sea and the Baltic—have proved a necessity: if already, both in Japan and in California, the coasts of the Pacific have been provided with such scientific outposts, it cannot fail that, by-and-by, Africa, Australia, and the Polynesian Archipelago will also have their biological stations. It is a great pleasure to me to be able to state here, that a small beginning is being made at Ralum in Neu Pommern (alias New Britain), the neighbour island of New Guinea, from whence numerous specimens of *Nautilus pompilius* have lately been procured. An intelligent and enthusiastic German planter, Mr. Parkinson, living since many years on that island, visited me a year ago in Naples, and offered spontaneously his help and services to establish a small station on his own land. According to his views, locality and climate will favour such a plan, and as there is every six weeks a steamer of the North German Lloyd from Ralum to Singapore, and soon perhaps another one to Sydney, the possibilities of a tropical archipelago station are given. The Naples Station has undertaken to provide the scientific equipment of its infant brother at the Antipodes, and my friend Major Alex. Henry Davis, from Syracuse (New York), who, already helped so much to establish lasting and fruitful relations between the United States and the Naples Station, has again stepped forward to provide for the first pecuniary wants of the Papua Station. Let us hope that this small beginning will reap some fruits, and the more so, as Mr. Arthur Willey, well known by his work on the development and morphology of the Tunicates and Amphioxus, has gone there as first pioneer of biology to study the development of *Nautilus pompilius*. His impressions have been as yet very favourable, and he thinks that the fauna of New Britain will amply repay every sacrifice of Mr. Parkinson and Major Davis. If the local authorities of New South Wales, or Victoria, or New Zealand, would find it worth their while to help to a laboratory in Port Jackson, or somewhere else in Australia: if in the Cape Colony somebody would do as Mr. Parkinson has done—numerous problems thrown open by the work of the *Challenger* would make progress, and the threads of biological study would draw nearer and nearer to encircle the most distant parts of the oceans.

But the greatest stroke would come, if one nation or an international combination would present biology and oceanography with a steamer, expressly built for purposes of such research as the *Challenger* performed. In the year 1884, I attempted something of the kind by forming a committee of influential men in Germany for the purpose of collecting £15,000 to £20,000, with which to build a yacht large enough to go round the globe, and serving as a floating biological laboratory. Of course it was not the sum of money wanted for the construction of such a ship which constituted the main difficulty of the scheme, though I failed even in that from reasons which had nothing to do with the scheme itself. The true difficulties lie in the extraordinary great regular expenses in commissioning such a ship, as every owner of an ocean yacht understands. Of course I was also prepared for that, and have no doubt that my plans would have answered, at least to some extent, but I was compelled to recognise the truth of the old proverb, "qui trop embrasse mal étreint." I do not know whether I shall yet be able to return to the attack; it seems rather unlikely, but it is my firm conviction that this scheme is, if not the only one which will permit us to conquer the battlefield, at any rate the chief means to enlarge our knowledge in oceanography, and will and must therefore be executed in no distant future.

Such a ship ought not to be continuously crossing the oceans: on the contrary, its best services would be rendered by giving it the chance to thoroughly investigate distant areas for distinct problems. Give such a ship the commission to study in the greatest possible detail, and in a comparative way, life and formation of the coral reefs in the Indian Ocean; let it be stationary for months together on the most favourable spots for such a study; prepare a scientific staff of specialists for the work, land them where the best opportunities for a transient establishment of a small laboratory is to be got, assist them by as many hands of the crew as can be spared, help them by the steam-pinnace on board, use the diving dress as well as native divers, and study for hours under water the construction and the destruction of the reef, apply all kinds of dredging and surface-fishing at day and night, have well-trained laboratory servants for the preservation alive and in alcohol of such organisms as are required for further study,—in short, do as if a well-appointed laboratory were transported to Polynesia; and be sure that results will ensue which by no other contrivances can possibly be obtained, especially if the ship be under no restrictions, and can stay in any one spot as long as may be requisite.

For it is the great drawback of the usual men-of-war expeditions, that they are only allowed a few days or weeks to remain at the same locality. There are so many other objects, to which it is necessary to give full attention, that they are always driven away from the work when the preliminary difficulties are just overcome. Science must be sovereign on board, the scientific leader must be absolute for determining the course to take and the time to remain. Discipline on board the ship must, of course, be handled by the captain or his officers, but the general dispositions of the work must remain with the scientific leader. That alone already would make a great difference in such an expedition from all those antecedent, and though very often the naval captains of expeditions for scientific purposes might well enough be transformed also into scientific leaders, nevertheless they are dependent on orders from home, and cannot always understand the importance of embryological, physiological, or other specialist work, for which they have to stay a month or two longer in the same harbour.

Again, the scientific staff must be selected with greatest care in regard to technical and personal accomplishments. If the staff is not varied enough, and does not include men of different attainments, many opportunities for investigation must be lost for want of previous knowledge on the side of the naturalists on board. On the other hand, nothing is more difficult than to live together for months,

or even years, on board a ship, for men not well trained to such existence, except where the composition of the staff is made with a sharp eye for compatibility and incompatibility of character. Especially the scientific leader must be a man of imposing personality rather than of special scientific competence, for it will fall to his share to dictate in every case where conflicting tendencies threaten to do away with social harmony.

But though all this may be considered to offer considerable difficulties in the way of execution, nevertheless the future for oceanography will belong to such floating biological stations, and the time is perhaps not so far distant, when more than one of them will cross the oceans, and supersede completely the now adopted system of single-handed expeditions of young naturalists. The necessity for such expeditions is doubtless existing, in so far as it is better to try the solution of problems regarding the tropics by travelling alone than by staying at home. And no doubt very many geographical, ethnographical, geological problems have been greatly advanced by competent travellers, and will furthermore be advanced in the same way. Collections of animals and plants have been made, mostly terrestrial, and the systematic part of biology has had its due share. But all more complicated studies, such as require more technical appliances and preparations, remain in the background, for the same reason which has forced us already in Europe to establish well-organised morphological, physiological and chemical laboratories, both inland and on the sea-shore. And if we cannot go on without them in Europe, where the general conditions for biological research are so much more advantageous, we must certainly have them, if we wish to advance our knowledge of tropical, terrestrial and marine organisms.

Botany enjoys already some advantages through the botanical gardens in Ceylon and Java, and it is to be hoped that the British and the Dutch authorities will use their exceptional opportunities in both places to establish some sort of regulations for their use by the botanists of all nations. May it not be possible to enlarge these botanical gardens by adding also some facilities for research of animal morphology? The Zoological Station at Naples has a special part prepared and equipped for morphological and physiological botany; in the first place, of course, for marine algæ, but any other sort of botanical study, for which Naples offers opportunities, might be undertaken there, and already a valuable work on the cultivation of figs has been greatly assisted by the Zoological Station. No doubt every naturalist who travels in Ceylon or the Sunda Archipelago receives the most valuable advice and assistance by Messrs. Trimen and Treub, and perhaps these most competent gentlemen would be the first to advocate a larger endowment of their establishments in the sense just now indicated; science and research would be certainly greatly benefited by it.

All these dreams and perspectives are opened up before us when we are looking over the enormous mass of new facts and new material for study brought together by the *Challenger*. And to think that there were only four naturalists and one chemist on board all the years long, and one of the naturalists died during the expedition! It is, I think, only right to remember here that two others of the gentlemen of the civilian staff so heavily overtaxed their strength with the often surely very monotonous, and always very hard work, that their health broke down soon after their return, and they fell victims to their enthusiasm. If it is only right to pay the highest possible respect to Mr. Murray for his extraordinary power of work, talent for administration and competence in dealing with the special problems of deep-sea deposits, and if we gladly recognise the excellent work done by Mr. Buchanan, I think nobody will be so ready as these two gentlemen to join here in thankful remembrance of the share of work that fell to their late companions, Sir Wyville Thomson, Prof. Moseley, and Dr. von Willemöes-Suhm. And may it be once more permitted to the writer of these lines, who by right or wrong claims some special title for it as a sort of international official of biological science, to utter the thanks of science to the officers and men of the *Challenger*, and to the Admiralty, and to the British Government and Parliament, and to the whole British nation for having set the example to the world of one of the grandest and most successful scientific expeditions that ever has been, and most likely for considerable time to come will be, started.

ANTON DOHRN.

OUR BOOK SHELF.

Horses, Asses, Zebras, Mules, and Mule Breeding. By W. B. Tegetmeier, F.Z.S., and C. L. Sutherland, F.Z.S. (London: Horace Cox, 1895.)

THE first portion of the title of this interesting work is somewhat misleading, for with the exception of some half-dozen pages which deal mainly with the distinctions between the horse and the other species of the genus, and a description of the supposed new species known as Prejevalsky's Horse, the book entirely relates to asses, zebras, and mules. None of the varieties of the horse which have been produced during the period of its long domestication are referred to. We mention this fact in case the general reader should infer from the title of the work that it was a treatise on the multitudinous domestic varieties of the horse which exist in nearly every quarter of the globe.

The volume is conveniently divided into two parts. Part i. is chiefly of zoological interest, and contains very complete and accurate descriptions of the existing species of the genus known to modern zoologists under the name of Equus, including, in addition to Prejevalsky's Horse, an account of the still more recently discovered Grevy's Zebra. The engravings which illustrate the letterpress are particularly good, and will greatly assist the student in his endeavour to master the peculiarities of each species. It concludes with a chapter on the hybrids which may be produced by crossing the horse with the other species of the genus Equus.

Attention should be directed to an assertion on the part of the authors that a remarkable and noticeable difference exists in the period of gestation of the mare and ass. The duration of gestation in the mare is well known to be eleven months, and it has generally been assumed that it was similar in the ass and zebra. The authors, however, emphatically assert that in asses and zebras it usually exceeds twelve months; one of them, Mr. Sutherland, who is well known as an extensive breeder of mules, quotes from his stud-book eight instances of the period of gestation in the ass, the result in six cases of a single service, the period varying from 358 to 385 days. It seems strange that such a marked difference should have hitherto escaped mention in all previously published works.

Part ii. is devoted exclusively to mules and mule breeding, and is replete with valuable and exhaustive information on these subjects. The authors strenuously deny the existence of fertility in either the male or female mule, affirming that abnormal lactation not unfrequently occurs in female mules, when milk is secreted in great abundance, and that the foals which they are observed to be suckling are in reality the foals of other animals which the mules have adopted. With regard to the oft-quoted instance of a mule in the Acclimitisation Gardens in Paris, which has produced foals when mated both with the horse and ass, the writers doubt whether the animal is a mule, and assume that she is an ordinary mare, whose female parent was influenced by a first alliance, as is so often the case in dogs and other animals. If their contention is correct, the mule may still aptly be described as "an animal of no ancestry and with no hope of posterity."

The writers are enthusiastic, nay even fulsome in their praise of this hybrid, and bitterly lament the lack of appreciation in which it is held in Great Britain as compared with America and some European States. "In endurance," say the authors, "capability of hard labour, economy in keep, longevity, and freedom from disease, mules far surpass horses." Into so controversial a matter this is not the place to enter, and we must content ourselves with the belief that so plain and oftentimes so ugly an animal as the mule will never supplant to any great extent, in this country at least, the beautiful and graceful varieties of the horse of which Englishmen are naturally so proud.

To any of our readers who are interested in the subject of mule breeding, this work may be heartily recommended; and, in conclusion, we feel bound to compliment in the highest terms all who have been instrumental in its production. W. F. G.

The Moon. By T. Gwyn Elger, F.R.A.S. Pp. 174. London: George Philip and Son, 1895.

IN this latest work on the moon, from the pen of one of the foremost of British selenographers, the most noteworthy feature is the excellent chart, eighteen inches in diameter; this is given in four quadrants, but it can also be obtained complete and separately. All the named formations are distinctly shown, and the names of the more important are very clearly printed on the map itself. The greater part of the text resolves itself into a descriptive index to the map; but though this appears in rather stereotyped fashion, it embodies a good deal of information which has been gleaned by the author during many years of observation. An introduction of forty pages deals with lunar phenomena generally, and includes numerous hints which will be of use to the observer. Mr. Elger objects most emphatically to our satellite being spoken of as a changeless world, and justifies his position by stating that volcanic outbursts, producing mountains as large as the Monte Nuovo, might occur in many parts of the moon without the world being any the wiser. Though possessing little of novelty, and not appealing to the general reader, the book and map together constitute a handy work of reference which observers of experience, as well as beginners, will be glad to have by them. A few details as to the phenomena to be observed during eclipses of the moon, might have been included with advantage.

Algebra. Part i. By M. H. Senior. Oldham: D. W. Bardsley.

KINDERGARTEN methods of teaching are now applied to most subjects. In this small book of fifty pages, the author endeavours to make algebra interesting to young students by associating the abstract symbols with concrete objects. The novel features of the book are the explanation of brackets, the exercises on factors, short methods of multiplication and division, the elucidation of signs, and the numerous practical examples.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Argon and Dissociation.

THE discovery of the new substance argon, by Lord Rayleigh, has given rise to a difficulty which, it is thought by some, shows that the periodic law of Mendelejeff has not that generality which has been attached to it by chemists during the last few years.

According to Lord Rayleigh's determination, the density of argon is 19·9 ($H=1$), making the atomic weight 39·8, as the molecules are shown to have no internal energy of the same order as their energy of translation, and hence to be monatomic. Argon with this atomic weight cannot possibly find a place in the natural classification. If its atomic weight were less than 39·1 (the atomic weight of potassium), argon would fall in the VIIIth or interperiodic group in Lothar Meyer's table; and its properties, so far as they have been investigated, would harmonise with this position.

The determination of the vapour density of iodine by V. Meyer, Crafts and Meier, and others, has shown that at temperatures below 1000° C. the gas consists of diatomic molecules, while above this dissociation takes place, and above 1500° C. we have the dissociation complete, and the molecules are monatomic.

Why, then, cannot we have a similar behaviour in the case of argon?

If argon at low temperatures (somewhere near its critical point) consisted of diatomic molecules, which dissociate as the temperature rises, the difficulty of the position of argon would be removed. Thus, suppose at the temperature at which 19·9 was determined as the density of argon, the dissociation has proceeded so far that 5 per cent. of the molecules remain diatomic; the average molecular weight would be 39·8, but we should have two kinds of molecules, monatomic and diatomic, and the atomic weight under these supposed conditions would be

$$\frac{39{\cdot}8 \times 100}{105} = 37{\cdot}9.$$

The ratio of the specific heats, at constant pressure and constant volume, taking 1·4 for this ratio for a gas with diatomic molecules, and $\frac{5}{3}$ for a gas with monatomic molecules, would be for argon, on the above supposition,

$$\frac{95 \times \frac{5}{3} + 5 \times 1{\cdot}4}{100} = 1{\cdot}65.$$

This value agrees very well with the values (1–16 1–65) determined for argon.

This explanation reconciles argon with the natural classification; and as yet no facts have been published in opposition to it.

If this hypothesis be true it could be easily verified, for at temperatures, not much higher than that at which the vapour density determinations were made, the dissociation would be complete; and hence the vapour density in agreement with a molecular weight about 38; and also at lower temperatures than that at which the vapour density has been determined the gas would not obey Charles' law: for the recombination of the single atoms to form diatomic molecules, and possibly molecules containing a greater number of atoms, would cause a contraction greater than that due merely to the cooling of the gas according to the ordinary law. PENRY VAUGHAN BEVAN.

Melbourne University, April 18.

PROF. BEVAN ascribes to me work done conjointly with Prof. Ramsay. An addendum to our paper (see *Proc. Roy. Soc.*) contains our account of experiments by Prof. Ramsay, especially directed to examine the question raised.

It has turned out that the gas possesses the same value of $\frac{pv}{T}$ as hydrogen, and that the value of this expression is not altered between − 90 and + 250°. The most trustworthy determination of the ratio of specific heats gives the number 1·65; but much dependence is not to be placed on the accurate value

the second decimal. Very concordant determinations of density gave as a mean number 10·00.

Argon, therefore, shows no sign of association on cooling, nor dissociation on heating, as Prof. Bevan thinks it might.

RAYLEIGH.

Terrestrial Helium (?).

PROF. PASCHEN and I have lately made a careful determination of the wave-length of the strong yellow line emitted by cleveite when heated in a Plucker tube. We owe the mineral to the kindness of Prof. Rinne. My large Rowland concave grating of 0·5 metre radius, clearly shows the yellow line to be double. Its less refrangible component is much weaker, but comes out quite bright, when the stronger one is brilliant. We photographed the two lines together with the second order of the spark spectrum of iron. There are a number of iron lines on each side that are included in Rowland's list of standard wave-lengths (*Phil. Mag.*, July 1893). From these we interpolated the wave-lengths of the yellow lines by micrometric measurement. Three different plates taken on different days gave us:

Strong component.		Weak component.
5875·894	... .	5876·210
5875·874	... .	5876·206
5875·880	... ...	5876·196
Mean 5875·883		Mean 5876·205

We think an error of more than 0·025 very improbable.

Now Rowland's determination of D_3 (*Phil. Mag.*, July 1893) is

5875·982

the result of three series of measurements which he believes to be accurate to 0·02.

The difference between this value and the wave-length of the strong component is much too large to be accounted for by an error of observation.

We do not therefore agree with the conclusion, drawn by Mr. Crookes, that the unknown element helium causing the line D_3 to appear in the solar spectrum is identical with the gas in cleveite, unless D_3 is shown to be double. Perhaps Prof. Rowland will tell us if this might have escaped his notice. From his note on D_3 in *Phil. Mag.*, July 1893, it appears that D_3 cannot have been so wide as to include both lines, because he would then not have considered his determination accurate to 0·02. As for dispersion, we may see in his table of solar spectrum wave lengths that he has frequently measured three and even four lines in an interval as large as the one between the components.

Hannover Techn. Hochschule, May 10. C. RUNGE.

The Origin of the Cultivated Cineraria.

I HAD hoped that it would not be necessary for me to say anything more upon this subject. But Mr. Bateson's last letter seems to require a few remarks on my part.

I confess that I find it very difficult to follow his train of arguments. All I can do is to restate once more my original position, and endeavour to see how far Mr. Bateson has been successful in impugning it. I am sorry that Mr. Bateson thinks I have "treated" him "to some hard words," though I confess he seems to me, in that matter, quite able to take care of himself.

I asserted then (*a*) that the cultivated Cineraria only differs from the wild form, putting colour changes aside, in dimensional differences. I believe that in saying this I am expressing the deliberate opinion of the Kew staff, the members of which [illegible] would have no hesitation in dis[illegible] their chief, if they thought otherwise. To this [illegible] I [illegible] that Mr. Bateson advances any serious [illegible]

[illegible] (*b*) I assert [illegible] that these dimensional differences had [illegible]. To this I understand Mr. Bateson [illegible], [illegible] I [illegible] that he has brought forward a [illegible] of [illegible] the contrary.

[illegible] Mr. [illegible] position. He asserts, in common [illegible] that the modern Cineraria is of hybrid [illegible] conclusion. And here I [illegible] Dr. Masters, F.R.S., the well-known [illegible] of the *Gardeners' Chronicle*, who in that paper for [illegible] 21, 1881, [illegible] "Cinerarias and Pelargoniums,

again, which originate from one species, vary from seed but not from buds; and the same may be said of the Cineraria, the offspring of one species."

Mr. Bateson complains that I do not give "any specific answer" to the historical evidence. I thought I had made it sufficiently clear in my last letter that: (*a*) I doubted its value for scientific purposes: (*b*) I set it aside as irrelevant on account of the impossibility of proving the descent of the modern Cineraria from its supposed ancestors. Both Prof. Weldon and I have shown that the historical evidence can be handled both ways. But I prefer to set it aside altogether in the face of objective facts.

Mr. Bateson's next step is one to which I most seriously demur. He transforms a proposition of mine into terms to which I could not assent, and then proceeds to attack it. He makes me say that "to improve a plant the only safe way is by selecting," &c. I absolutely never said anything of the kind. "Improve" in horticulture is a word of large connotation. I confined myself to the production of dimensional changes, and I believe that what I said was in accordance with horticultural experience.

To demolish my position, Mr. Bateson has to get over the fact, which seems to me incontestable, that there is no essential morphological difference between the cultivated Cineraria and the wild *C. cruenta*. To do this he trots out the Himalayan rabbit. I cannot but admire his courage. What possible analogy can there be in the two cases? Two "breeds" of rabbits are crossed and produce a third *different from either*. If the modern Cineraria is of hybrid origin, then it has eliminated traces of all but one of its parents. The principle of economy of hypothesis makes me slow to believe this. Anyhow the Cineraria has clearly not produced anything analogous to a Himalayan rabbit which differs from both its parents.

As to Mr. Darwin's account of the origin of the Cineraria, I must frankly take the responsibility. I have no doubt he worked with ordinary garden kinds. He wrote to me for information as to their origin. At the time I was entirely ignorant of the subject. I wrote to Mr. Thomas Moore, who was considered the best authority on such matters, and he sent me the traditional account. I passed it on to Mr. Darwin, with the opinion, no doubt, that I thought the information trustworthy. So I am afraid Mr. Bateson is only appealing in this case from Philip sober to Philip drunk; *i.e.* from my own considered opinion to my unconsidered one.

I will now wind up all I have to say on the subject with a few miscellaneous remarks.

There can be no two opinions as to the importance of the study, from the point of view of organic evolution, of the changes which can be brought about in plants under cultivation. But it must be conducted with scientific precision. This discussion will not have been fruitless if it directs attention to the subject. A beginning has already been made. M. Bornet has worked on the genus *Cistus* at Antibes, and has reconstructed some of the forms, as to the origin of which there was only "historical evidence," described and figured by Sweet. My friend Count Solms-Laubach is engaged on the cultivated forms of *Fuchsia*, and I am quite sure that any results he arrives at may be accepted with implicit confidence. As he has asked me for species of *Cineraria*, I hope he may look into this matter also.

I must repeat my caution as to the danger of accepting horticultural evidence as to hybridity. I will give a few recent instances. I could easily give a long list with chapter and verse for each.

(*a*) *Thuya filiformis* was long considered to be a hybrid between *Juniperus virginiana* and a *Thuya*. It is now known to be a "growth-stage" of *Thuya orientalis*. The history is discussed by Sir Joseph Hooker in the *Gardeners' Chronicle* for June 22, 1861, pp. 575, 576. It affords a delightful commentary on the hybridisation fallacy and the value of "historical evidence."

(*b*) Some years ago we received at Kew bulbs of what professed to be a hybrid between *Amaryllis Belladonna* and *Brunsvigia Josephinae*. When it flowered, it was evident that it was no hybrid at all, but only a very fine form of the former species. This is rarely propagated from seed. In this particular case seminal variation had come into play with corresponding dimensional change. The hybrid origin is recorded in the *Gardeners' Chronicle* for September 4, 1875, p. 302. It will, no doubt, be dug out hereafter as "historical evidence."

(*c*) The last number of the *Gardeners' Chronicle* (June 1, 1895,

p. 692) affords a striking instance. Hybrid Cypripediums are of considerable pecuniary value. One recently exhibited at the Royal Horticultural Society was at once denounced as no hybrid at all, but a merely seminal variation. The possessor has fears that it will "add one more to the long list of doubtful crosses by which auctioneer and purchaser are alike misled."

Notwithstanding the Himalayan rabbit, I am afraid botanists will continue to refuse to accept hybrid origin on historical evidence unless there is palpable objective proof of the fact.

There are two additional bits of evidence, to which, however, I do not attach great weight, but which may be recorded to complete the story. It is, at any rate, agreed that the Cineraria originated from the Canaries. I have already pointed out that De Candolle divided the wild Canarian species into shrubby and herbaceous. I do not believe that they are mixed in the modern Cineraria, which remains entirely herbaceous. Now, Schultz-Bipontinus, who described the Canarian species for Webb and Berthelot, relegates the shrubby species to *Senecio*, and the herbaceous to *Doronicum*. Though this is not now sustained, it shows that the two groups are not very closely related, and diminishes the probability of their freely intercrossing.

On the other hand, *Cineraria cruenta* and the modern *Cineraria* cross with the greatest facility. In fact, if you grow the two together it is almost impossible to keep the wild species true. I have no doubt that in a short time we shall be able to combine the pleasing habit of the wild plant with the fine colour of the modern strains. All this does not surprise one, as to me they are all essentially the same thing.

I must add one word more. I cannot but think that there is a growing danger nowadays of a pseudo-biology growing up for the especial use of evolutionists. This is not the first time by many that I have been so unlucky as to come into collision with it. Long ago I pointed out in these pages that biology is not a deductive science, and for the present, at any rate, theory must be adjusted to facts, not facts to theory.

W. T. THISELTON-DYER.

Royal Gardens, Kew, June 1.

MR. BATESON now admits that *some* named varieties of *Cineraria* may have arisen from pure-bred *C. cruenta*, or from plants believed to be pure-bred. He holds that these have become extinct, while Mr. Dyer believes the hybrids to have disappeared. I have never attempted to discuss this question, and shall not do so now. I wish only to justify my interpretation of the passages I quoted against Mr. Bateson :—

(1) Mrs. Loudon begins the article quoted by both of us with these words : "Most of the purple *Cinerarias* are varieties, or hybrids, of *C. cruenta*." She then goes on to say that in or about 1827 (the year in which he recommended the growth of pure-bred *C. cruenta* "for the production of fine double and single varieties"), Drummond, of Cork, produced certain hybrids ; while since his time other hybrids had been made. She then, in a new paragraph, says : "Some of the most beautiful *Cinerarias* now in our greenhouses have been raised by Messrs. Henderson . . . particularly *C. Hendersonii* and the King, both raised from seeds of *C. cruenta*" ; and a line or two further : "Two new ones have lately been raised, of remarkably clear and brilliant colours, apparently from *C. cruenta*, named Queen Victoria and Prince Albert," &c.

It will be seen that the general statement, with which the article begins, declares "*most purple Cinerarias*" to be "*either varieties or hybrids*" of *C. cruenta*. Of others, and of those Cinerarias (such as "the King") which are not purple, nothing is said. This general statement is illustrated by examples, first of hybrids, next of pure-bred varieties.

In discussing the examples of pure-bred forms, Mr. Bateson omits to notice "Queen Victoria" and "Prince Albert," and discusses only *Hendersonii* and "the King." He believes Mrs. Loudon, in saying that these were "raised from seeds of *C. cruenta*," to mean simply that *C. cruenta* was the female parent, the male being unknown, or unnamed. I do not know what degree of inaccuracy Mr. Bateson is willing to attribute to Mrs. Loudon ; but in the writings of serious botanists a "seed" means the fertilised product of two elements, the ovule and the pollen grain ; and therefore the "seed" of *C. cruenta* means the product of two parents, both of which belonged to this species.

Mr. Bateson says that six or seven years after writing the passage in question, Mrs. Loudon speaks of *C. Hendersonii* and the King as "hybrids." This simply shows that she changed her mind ; and although it may affect the value of her opinion as evidence, it does not alter the plain meaning of her words in 1842.

(2) The only author whom I quoted as asserting the pure-bred origin of *C. Hendersonii* and the King was Mrs. Loudon. It is true that in two other articles quoted by Mr. Bateson these plants are called hybrids. I did not allude to this matter in my first letter, because I hoped Mr. Bateson would himself see the folly of attributing to these articles any definite meaning whatever. It will suffice to consider one of them.

In the earlier article, describing *C. Waterhousiana* (*Paxton's Mag. Bot.* iv. p. 219), that plant itself is called a "variety," although it is said to be the offspring of specifically distinct parents. On p. 221, *C. Hendersonii* is alluded to in these words: "The following are the names of some of the hybrids raised and cultivated by Messrs. Henderson . . . *C. cruenta var. Hendersonii*, *formosa*, &c." Both these passages are meaningless, if the words "hybrid" and "variety" are construed strictly. If they are not to be so construed, and they evidently cannot be, then I was justified in ignoring the passages, for they prove nothing but the incompetence of their author.

On the other hand, the passage which I did quote from this article is at least intelligible ; and it asserts that *C. cruenta* "may be regarded as *the parent*"—which means, if it means anything, the *only* parent "of many of those beautiful varieties so successfuly cultivated by Messrs. Henderson," &c. This passage Mr. Bateson does not consider in his reply to me.

The second article (*Paxton's Mag.*, 1842, p. 125) in which the King is called a hybrid, uses the word in the same loose fashion, and it would be as easy as unprofitable to quote other passages in which the same plants are called now "varieties" and now "hybrids."

Enough has been said to show that Mr. Bateson's original evidence does in fact bear the interpretation I put upon it ; and further, that the words "variety" and "hybrid" are so loosely employed by early writers that their records are often of little value. Stories of hybridism and sporting are frequently brought forward on such evidence ; so that I have thought it worth while to examine the case for one such story, as stated by its advocate. Having done this, my interest in the matter ends, and I do not propose to speak further upon it.

W. F. R. WELDON.

University College, London, May 31.

Some Bibliological Discoveries in Terrestrial Magnetism.

IN a letter on the above subject, by Dr. L. G. Bauer, published in NATURE of May 23 last, I read as follows : "I find it asserted that the Frenchman, L. J. Duperrey, was the first (1836) to construct 'magnetic meridians' for the whole earth, *i.e.* those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle." The writer then remarks that the honour of first introducing this method is due to Thomas Yeates, an Englishman, in 1817.

This is hardly correct, as I possess a coloured map of the Northern Hemisphere with the "magnetic meridians" as described shown upon it of an earlier date. The title of the map is :

"To George Washington,
"President of the United States of America,
"This Magnetic Atlas or Variation Chart is humbly inscribed by John Churchman."

As Washington died in December 1799, it is evident that John Churchman has a prior claim to being the first to construct "magnetic meridians." ETTRICK W. CREAK.

London, May 31.

Effects of Earthquake in Sumatra.

ON May 17, 1892, an unusually severe earthquake was felt through nearly the whole of North Sumatra ; most severely shaken was the district between the Dolok Lubuk Raja and the Ganung Talamau (Ophir). Serious landslips occurred in many parts of the mountains, especially near the summit and along the slopes of the Gunung Merapi, a volcano 2145 metres high in the residency Tapanuli. On inspection it was found that the safety of a brick pillar, erected on its most elevated point by the triangulation service, was endangered by part of the crater having been

destroyed. At three metres distant from the original pillar, as much as the narrow ridge would allow, a new pillar was built, the top of which was made level with that of the original one. The measurements made in order to fix the position of this new pillar showed such differences with the original measurements, that these could only be explained by a displacement of the original pillar. As, however, neither fissures nor local disturbances of the ground could be observed, new measurements were

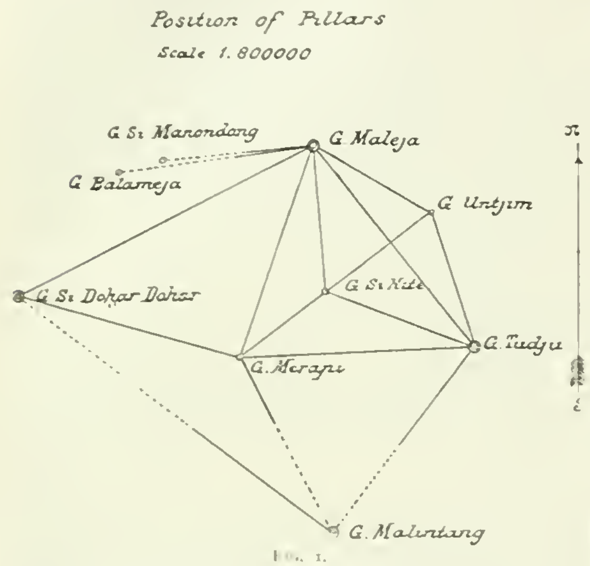


FIG. 1.

made from all the surrounding positions, and it was proved that a displacement of several more pillars had taken place.

Fig. 1 shows the position of the pillars before the earthquake: Fig. 2, their displacement by the earthquake. A detailed description of these measurements was published in the *Natuurkundig Tijdschrift*, vol. iv. part 3, by Captain Muller, the chief of the triangulation party. The longest distance over

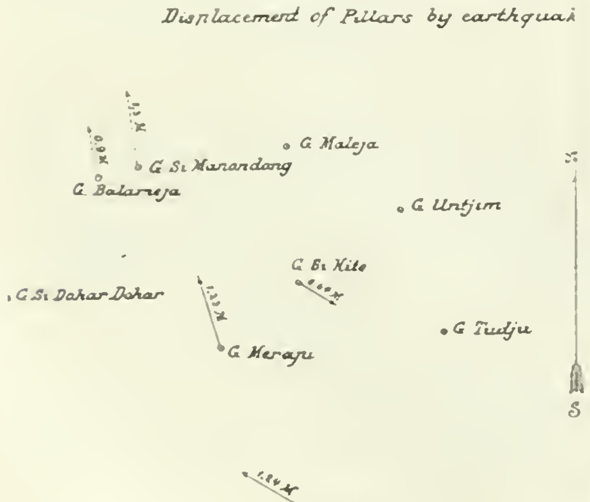


FIG. 2.

which a displacement was proved to have taken place was between the Gunong Malintang and the Dolok Balameja, or 53 kilometres. Captain Muller, however, has no doubt that if a new survey were carried on more southward, a displacement of more pillars—that is, a contortion of the surface over a larger area—would be found to have taken place.

Malang, April 14. TH. DELPRAT.

Instinct-Impulse.

THE note published in NATURE under date of April 18, in reference to my article in the April number of *Mind*, leads me to think that it may be well to explain my reasons for adopting the terminology there and elsewhere used by me, and which the writer of the note calls in question. I do so with the hope that this explanation may lead towards that "consensus of opinion on psychological nomenclature" that the writer of the note thinks is at present impossible.

The word "instinct," as my critic states, is generally applied "to the manifestation of particular activities." In other words, it is used by the biologist in an *objective* study of activities in animals, when he is not dealing with the nature of the conscious states coincident with these activities. It is thus, too, that I employ the word; but I have extended its use to cover certain manifestations of activities that do not take a large place in the considerations of the biologist, but that, nevertheless, appear to me to be of the same general nature as those "manifestations of particular activities" to which the word "instinct" is by current agreement applied.

What I claim is that the actions of one who is carried away by imitation, and the work of the philanthropist and of the artist, when objectively viewed, appear as "manifestations of particular activities," just as much as do the actions that go with self-defence and tribal protection, with care of the young, with nest-building, with migration, &c., and that therefore the term instinct, if applied to one set of such activities, may be applied to all.

If it be held that the objection to the extension of the use of the term lies in the fact that the activities that I speak of as due to the "imitation instinct," the "benevolent instincts" and the "art instincts" are not sufficiently *particular*, then I must answer that the fixedness of the actions involved is in all cases of instinct only relative; that this relative fixedness varies with the different instincts. In the self-preservative reactions, for example, we are able to predict the blow at the enemy, whilst the very varied actions by the animal mother in securing the safety of her young are unpredictable; but who hesitates to speak of the maternal "instincts"?

The word "instinct" then, in my view, should be used to indicate the manifestations of those animal activities which, when we consider them objectively, we see to have become emphasised because of racial values; of these values the acting animal (even if he be a man) may have no cognisance whatever. This is the usual use of the word, and there seems to me to be no scientific demand for any change in this usage.

On the other hand, I have suggested that we use the term "instinct feelings" to indicate the conscious coincidents of the animal activities that we call instinctive; and I have endeavoured to show that where these instinct actions are relatively fixed and forceful, then their coincident "instinct feelings" gain names, and form the class of psychic states known as the "emotions."

Furthermore, I object to the use of the word "impulse" in the description of these activities, as my critic suggests its employment, especially when they are objectively considered; for the word "impulse" is in general used to indicate those phases of consciousness which are produced by the *inhibition* of instinctive activities that have been stimulated by the presence of the objective condition that usually calls them out, but which for one reason or another are not at once realised. This, indeed, is the way in which the word is usually employed, not only by the psychologist, but in common speech as well. We speak of having an impulse to strike an enemy, not when we do strike him, but when the instinct to strike is held in check. What is more, I think this word "impulse" should be employed in this sense only; for the requirements of science do not demand its use with any other signification. I have discussed this matter of the nature of impulse rather fully at pp. 272, &c., in my book, "Pain, Pleasure, and Æsthetics," to which the writer of the above-mentioned note refers. HENRY RUTGERS MARSHALL.

New York, May 2.

THE term "instinctive" should, in my judgment, be applied to those activities which are congenital and which are also relatively definite; the term "instinct" being reserved for the subjective and affective condition of the performance of instinctive activities. Where the definiteness is the result of individual acquisition the term "instinctive" should not be applied, though it is so used by Prof. Wundt and others. The modern

controversy as to the inheritance of acquired characters seems to render insistence on the congenital element advisable. Undoubtedly there is an inherited tendency to imitation; but from the nature of the case, the activity performed through imitation is not congenitally definite.

With Mr. Marshall's statements concerning impulse I cannot agree. If we say in common speech that "the instinct to strike is held in check," we also say that the impulse to strike is held in check. The control of our lower impulses is an important part of our moral life; but the contention that the impulses are "produced by the inhibition," is open to serious criticism.

THE WRITER OF THE NOTE.

RECENT EXCAVATIONS AT THE PYRAMIDS OF DAHSHÛR.

FEW sources have supplied more facts for the study of anthropology than the Egyptian tombs, and the most important necropolis of Egypt is situated south-east of Cairo, close to the remains of ancient Memphis.

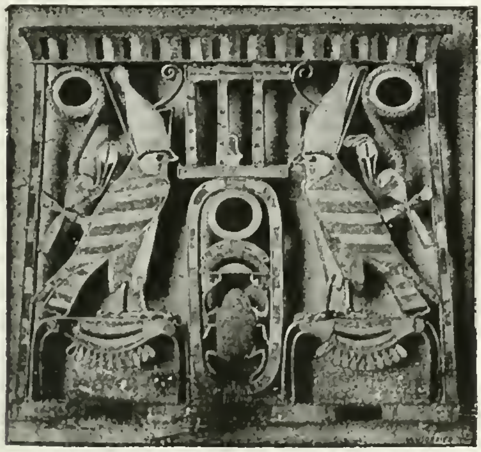


FIG. 1. Pectoral belonging to Usertsen II. (Found March 7, 1894.)

This stretches from the village of Abou-Roash on the north to that of Médûm on the south, about a distance of twenty-five miles.

To the south, and at the end of the great chain of pyramids, are those of Dahshûr, of which four are of stone and two of brick. Up till 1892 the history of two of these still remained to be unravelled, but in that year a large party of excavators, headed by M. de Morgan, set out, and succeeded in opening up both these pyramids. It is to this interesting work we wish to draw attention, for it marks an important step in Egyptology, indicating some of the earliest applications of science in one direction known to us, while ancient art is at the same time illustrated. M. de Morgan has recently given an account of his explorations in *Le Monde Moderne*, and we are indebted to the courtesy of the Editor of that magazine for the illustrations of the finds.

The two pyramids are of brick, and covered with a layer of limestone; each one was surrounded by a brick wall, which showed the limits of land reserved for the use of the royal family. Round this was an avenue, left out of respect to the descendants of the gods; then came the tombs of the great people connected with the court. From inscriptions found, there is every reason to believe that these two pyramids belonged to Usertsen III. and Amen-em-hât III., both of the Twelfth Dynasty. On the north side of the more northerly one are the tombs of some princesses, four among them more important than the rest.

These tombs have been plundered, for, owing to the Egyptian custom of burying jewels with their dead, the pyramids have ever been a favourite resort of robbers; and thus it is that some of the tombs are in great disorder, which causes much hindrance to the scientific research now being carried on, more especially as many documents have been carried away. Still, the plunderers have not stripped them entirely, and the remaining documents and treasures have been a most important clue to finding out the dates of the pyramids and the history of the people they entomb.

This spoliation of the tombs, continued by each successive generation, was not stopped till the celebrated Mariette founded the "Service for the Conservation of Monuments in Egypt."

Amongst the most interesting and perfect pieces of jewellery found are three pectorals. They were found in the princesses' tomb, and had been hidden in the soil in order, no doubt, to deceive the plunderers.

Fig. 1, the first one unearthed, has in the centre the cartouch of Usertsen II., held by two hawks, which bear the crown of Lower and Upper Egypt. The signs of the cartouch are made of cornelian, lapis-lazuli, and turquoise, set in gold; the other figures are likewise set with precious stones. The other two pectorals are similarly executed. The first (Fig. 2) represents two men, each in the act of striking with a club an Asiatic captive who they are holding by the hair. In the centre is the double cartouch of the king, and on each side the emblem of life, out of which protrude two arms holding a flabellum. Above them all is an eagle with outspread

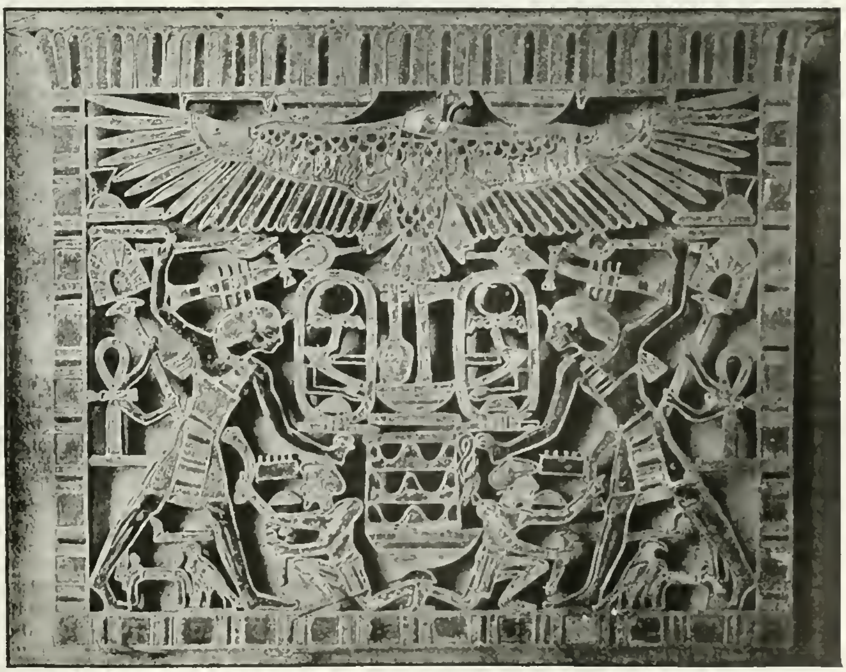


FIG. 2. Pectoral belonging to Amen-em-hât III. (Found March 8, 1894.)

wings, having in its claws the symbols of eternal life and stability. The second one (Fig. 3) has similarly an eagle with outspread wings, and beneath it is the cartouch of Usertsen III. To the right and left is a sphinx with the head of a hawk, on which are the feathers of Ammon; each is standing on a captive, whilst in front of each kneels an interceding Asiatic prisoner.

The workmanship of these jewels is wonderful. The perfection with which the precious stones are set, and, moreover, the delicacy and freshness of the whole, makes it hard to believe them five thousand years old. The work shows how far science dates back, and is evidence that in the case of the Egyptians, the further we look back, the higher we find their culture.

It is a curious fact that when we compare these jewels with those of a later period, we should find them far superior in workmanship; but so it is, for those of the time of the Ramessids are but an imperfect edition of the more ancient ones, not nearly so artistic, nor yet so well finished off.

When the excavations were continued, five large barges were brought to light; it was not till the work had continued some time that the royal apartments were found, so cleverly were they hidden.

The pyramid of the south is the most southerly royal monument of the Memphite necropolis. Traces are still to be found of a wall round it, and similarly situated as

Fig. 3.—Pectoral belonging to Usertsen III.

the princesses' tombs at the pyramid of the north; here, too, we find a gallery of twelve vaults or tombs, of which only two contain mummies, one being King Ra-Fou-Ab, and the other a princess, Queen Noub-Hotep.

Near the king's sarcophagus is a small chamber, in which were a quantity of broken vases and chests, and in a great wooden tabernacle was a statue of the *double* of the deceased (Fig. 4), painted grey, representing a young man of fifteen or sixteen. It is made of hard wood, almost black, and is admirably done; every muscle and bone are perfectly placed, and specialists have certified it [illegible]. It is a fine piece of Egyptian sculpture, of which only four good specimens have descended to [illegible]. Some savants have endeavoured to classify what has been found into certain schools, but this is scarcely advantageous till more has been collected.

The well leading to the princesses' tomb is about 13 ft. deep. At the bottom is a vaulted brick passage, which formerly ended in a wall. As was suspected, the wall being removed revealed a vault containing a flagstone, on which were water jars, pieces of embalmed meat, and other offerings, also two cases containing many things pertaining to the toilet. No inscriptions were found until the flagstone was removed, and a coffin brought to light on which were many texts relating to the name and title of the princess. As this tomb is so similar to that of King Ra-Fou-Ab, and is so closely situated, it is supposed that the princess was his wife; but nothing has been found to confirm her marriage with him.

Fig. 4.—Statue of double of the King Ra-Fou-Ab. (Found April 16, 1894.)

Although a great deal has been done, it will require many years of hard work to open up all the tombs in the Dahshûr necropolis; but general interest has now been awakened, thanks to those who have been the means of making us acquainted with the preceding facts; the results of future action will be followed by many.

NOTES.

Prof. Cornu, the Vice-President of the Paris Academy of Sciences, is now in England, and will deliver the discourse at the Royal Institution to-morrow evening. On Tuesday evening he was entertained by the members of the Athenæum Club who are members of the Institut de France, either as Associates or Correspondants. There were present, representing the Académie des Sciences, Lord Kelvin (Associate), Sir H. Gilbert, Mr. Huggins, Mr. Lockyer, Admiral Sir G. H. Richards, and Mr. Sylvester (Correspondants); representing the Académie des Inscriptions, Sir J. Evans and Sir E. Maunde Thompson; representing the Académie des Beaux Arts, Mr. Herkomer. Letters of regret for unavoidable absence were read from Mr. Frankland and Sir Joseph Lister, Associates of the Académie des Sciences; and Sir J. Hooker, Lord Rayleigh, Sir A. Geikie, Dr. Williamson, and Sir H. Roscoe, Correspondants; Académie des Beaux Arts, Sir J. Millais, Mr. Alma-Tadema, Sir E. Burne-Jones; Académie des Sciences Morales et Politiques, Mr. Goschen, Mr. Bryce, Mr. Lecky, and Sir F. Pollock.

Mr. Herbert Spencer has been created by the German Emperor a foreign Knight of the Order Pour le Mérite. Another mark of the esteem in which he is held is his election as an Honorary Member of the Vienna Academy of Sciences.

Sir Archibald Geikie has just been elected a Corresponding Member of the same Academy.

Dr. Backlund has been appointed Director of the Pulkova Observatory, and Dr. Hermann Struve will succeed the late Dr. C. F. W. Peters as Director of the Königsberg Observatory.

It is noted in *Science* that Deputy Surgeon-General J. S. Billings will shortly leave the Army Medical Museum, of which he is curator, and the Library of the Surgeon-General's Office, of which he is librarian, having accepted the chair of Hygiene in the University of Pennsylvania. Dr. Billings hopes to complete his work on the final volume of the great Index Catalogue before his retirement.

Dr. John Anthony, whose name is familiar to many workers in microscopy, died at Birmingham on Monday, at eighty-one years of age.

The death is announced of Prof. Franz Ernst Neumann, Honorary President of the Physikalisch-Okonomische Gesellschaft at Königsberg. Prof. Neumann died on May 23 at the advanced age of ninety-seven, having been born September 11, 1798. He was eminent in the department of mathematical physics, and was elected a foreign member of the Royal Society of London in 1862.

Among other deaths of scientific men abroad, we notice that of Dr. John Byron, well-known for his bacteriological researches. He was bacteriologist in the Loomis Laboratory, and lecturer on bacteriology in the University Medical School of New York. Dr. Byron is believed to have contracted the disease of which he died, by inhaling tubercle bacilli while carrying out some experiments. The deaths are also announced of Dr. O. Ke[illegible]h., at Berlin; Dr. F. Müller, the zoologist, at Basel; and Brigadier-General Charles Sutherland, formerly Surgeon-General of the United States Army, at Washington.

The Harveian Oration will be delivered at Edinburgh on June 28, by Dr. Yellowlees.

The Secretary of State for the Home Department has requested the following gentlemen to inquire into and report on the manufacture, filling, and use of gas cylinders:—Prof. C. V. Boys, Prof. H. B. Dixon, Dr. A. Dupré, the Rev. F. J. Smith, and Prof. W. C. Unwin. Mr. Robert F. Reynard, of the Home Office, will act as secretary.

Acting under the Wild Birds Protection Act, 1894, notice has been given by the Home Secretary, that the taking or destroying of the eggs of the "barn owl, brown or wood owl, long-eared owl, short-eared owl, common buzzard, merlin, kestrel, goldfinch, black-headed gull, peregrine falcon, kingfisher, dotterel, raven, heron, bittern, woodcock, dipper or water ouzel, and golden plover," is prohibited in any part of the county of Westmorland.

The preliminary programme for the sixty-third annual meeting of the British Medical Association, to be held in London from July 30 to August 2, is given in the *British Medical Journal*. The President, Sir J. Russell Reynolds, will deliver his address on July 30. The Address in Medicine will be delivered by Sir William Broadbent on the following day. Mr. Jonathan Hutchinson, F.R.S., will give the Address in Surgery on Thursday, August 1, and the Address in Physiology will be given by Prof. E. A. Schafer at the concluding meeting on August 2.

At the annual general meeting of the Institution of Civil Engineers, held last week, Sir B. Baker was elected President, and Mr. J. Wolfe Barry, C.B., Mr. W. H. Preece, C.B., Sir Douglas Fox, and Mr. James Mansergh Vice-Presidents. The members of the Council are Dr. W. Anderson, Mr. Alex. R. Binnie, Mr. W. R. Galbraith, Mr. J. H. Greathead, Mr. J. C. Hawkshaw, Mr. C. Hawksley, Dr. John Hopkinson, Dr. Alex. B. W. Kennedy, Sir G. L. Molesworth, Sir Andrew Noble, Sir E. J. Reed, Mr. W. Shelford, Mr. F. W. Webb, Sir W. H. White, and Sir E. Leader Williams.

We have received from Dr. P. Bergholz, Director of the Meteorological Observatory at Bremen, the results of the hourly observations made during the year 1894, with rainfall values obtained from four stations in the suburbs. This observatory forms part of the regular German meteorological service, and the results are therefore given in the form recommended by recent congresses; but in addition to the prescribed observations the work contains other valuable information, *e.g.* phenological observations, and the dates of freezing and clearing of the Weser since 1818. This table shows that the most prolonged frosts during that period were in 1844-5, 1846-7, 1857-8, and 1870-1. In each case the Weser was frozen over for two months or upwards. We observe, however, that the publication of the data is to be discontinued, as that river is now kept free for navigation by artificial means. A graphical representation of the principal meteorological results gives a ready means of comparing the characteristics of the different months.

The Egyptian Government have published an important paper on the climate of Cairo and Alexandria, based on observations taken between 1886 and 1890, and discussed by Dr. Engel, chief of the Statistical Service. The work contains a number of tables and diagrams, together with introductory text, from which we extract a few of the results obtained. At Cairo, the mean annual temperature for the five years was 70°·3, the absolute maximum being 118°·2 on June 13, 1886, and the lowest 33°·8 on January 1, 1890. The average yearly number of rainy days was twenty-four, and the amount 1·2 inch only. At Alexandria the mean temperature was 68°·5, the absolute maximum being 100°·6, on May 10, 1889, and the minimum 43°·9, on January 22, 1880. The average number of rainy days was forty, and the amount 8·2 inches. The principal difference in the climate of the two places consists in the diurnal and seasonal variations of temperature. Cairo is much the hotter of the two places in summer, but cooler than Alexandria in the winter; and the differences in the extreme temperatures are much greater at

Cairo, both as regards days and seasons. Relative humidity varies much more at Cairo than at Alexandria, but it is much lower at Cairo in summer, and a little higher in winter than at Alexandria; while, on the contrary, the absolute humidity varies much more at Alexandria, being very high in summer and considerably greater than at Cairo. Both places enjoy a large amount of sunshine, but fog occurs occasionally, more particularly at Cairo in the early morning.

A MOST important contribution to the study of the formation of dolomite is made by M. C. Klement, in the *Bull. Soc. Belge Géol. Paléontol. et Hydrol.* After describing the history of theories of dolomite, the author calls attention to the frequent occurrence of dolomite in the form of coral-reefs, as observed by Dupont in the Devonian, by Richthofen and Mojsisovics in the Trias, and by Dana in the recent raised reefs of Metia in the Pacific. He points out that while in the chemical experiments that have been made with a view of dolomitising carbonate of lime, *calcite* has always been operated on, the substance of coral has been shown by Sorby to be probably *aragonite*. The author has therefore carried out a large series of experiments on the action of the constituents of sea-water (particularly magnesium sulphate) on aragonite, the results of which are given at full length. From these he finds (1) that a solution of magnesium sulphate, in the presence of sodium chloride, and at a temperature of 60 C. or more, decomposes aragonite with formation of a magnesium carbonate the exact composition of which is difficult to determine, owing to the impossibility of isolating it from the residual aragonite; (2) that this action increases with the *rise of temperature*, and with the *concentration* of the solution, and is greatly diminished by the absence of sodium chloride; (3) that recent coral is attacked by magnesium sulphate just as mineral aragonite is; and (4) that the lagoons of modern coral-reefs afford all the conditions of temperature, saturation, &c., necessary for the production of magnesium carbonate in the manner of his experiments. While recognising, therefore, that dolomites may have been formed in more ways than one, M. Klement concludes that one of the most usual ways of formation of dolomite in nature has been the action of heated and concentrated sea-water in coral-lagoons on the aragonite of coral and other skeletons, with formation of carbonate of magnesium, which is subsequently, perhaps after solidification of the rock, with the remaining carbonate of calcium, converted into massive dolomite.

THE last number of *Modern Medicine and Bacteriological Review* is of exceptional interest, inasmuch as it contains an original article by Prof. Metchnikoff, of the Pasteur Institute, on "the extra-cellular destruction of bacteria in the organism." This article is really a critical comment upon some of the conclusions deduced by Dr. Pfeiffer from his experiments on the destruction of cholera vibrios in the peritoneal cavity of guinea-pigs. Dr. Pfeiffer observed this destruction of cholera vibrios when the latter were introduced into animals previously vaccinated against this germ, and also in the case of unprotected animals when the vibrios were injected together with a small quantity of serum from vaccinated animals. In both cases Dr. Pfeiffer found that they were destroyed *outside* the cells in the peritoneal fluid, and he believes that this bacteria-killing fluid is secreted by the cellular elements in consequence of a special excitation produced by the injection of cholera vibrios, and that the immunity acquired by guinea-pigs is independent of phagocytosis. Prof. Metchnikoff, however, regards this as an episode in the battle between bacteria and phagocytes, and maintains, on evidence supported by experiments, that the leucocytes secrete this bacteria-killing fluid whilst undergoing a process of degeneration due to the injection of Pfeiffer's mixture of vibrios, serum, and broth. That although unable to engulf the vibrios, they are able still to destroy them by their secretions. Metchnikoff points out that if before introducing the vibrio-mixture, a few cubic centimetres of broth be injected into the peritoneal cavity, the leucocytes will gather together in great force after a few hours, and if the vibrio-mixture be then introduced, phagocytosis does take place, and the cholera bacteria are more rapidly destroyed by this process of intra-phagocytosis than by the extra-cellular destruction produced by the conditions of Pfeiffer's experiments. The mechanism of immunity is surrounded with so many complicated problems that the search for its solution, whilst one of the most interesting tasks afforded by the developments of bacteriology, must still remain one of the most puzzling and difficult.

THE current number of the *Journal de Physique* contains an important paper by M. P. Curie on the magnetic properties of bodies at different temperatures. The author has examined the magnetic properties of a number of substances in fields of from 25 to 1350 C.G.S. units, and in some cases for temperatures from 15° to 1370° C. The body under observation was generally in the form of a coarse powder, and was enclosed in a glass bulb, which was placed in a non-uniform magnetic field produced by two electro-magnets. The force acting on the body was measured by means of the torsion of a wire. For the purposes of heating the glass bulb was surrounded by a fine clay jacket, and this latter was heated by a wire in which an electric current was passed, the temperature being measured by means of a thermo-electric junction. In the case of diamagnetic bodies, with the exception of bismuth and antimony, the author finds that temperature has practically no effect on their magnetic properties. Fusion and allotropic modification also seem to produce no effect, so that the magnetic properties of a body seem to depend not on the arrangement, but rather on the nature of the molecules of the body. Selenium, however, is an exception, for in this case the susceptibility is about 3 or 4 per cent. smaller in absolute value in the liquid than in the solid state. Phosphorus is another exception, for the susceptibility of the different allotropic modifications are slightly different. The susceptibility of bismuth increases with rise of temperature, according to a straight line law, up to the melting-point, where there is a sudden rise. The susceptibility of melted bismuth is independent of temperature, and is very nearly 0. Observations made on oxygen show that the coefficient (K), which, when multiplied into the strength of the magnetic field, gives the magnetic moment of the body per unit mass (the author calls this the coefficient of specific magnetisation), is independent of the pressure, and is between 20° and 450 inversely proportional to the absolute temperature. In the case of solutions of paramagnetic salts, K is also found to vary inversely as the absolute temperature; thus supporting the observations of Wiedemann and Plessner on this subject. Glass when cold is generally feebly diamagnetic; when heated, however, it becomes much more strongly diamagnetic. The rate of increase of the diamagnetism decreases as the temperature rises; above 300° C. no further change takes place. The author considers these changes to be due to the fact that glass consists chiefly of a diamagnetic substance, the properties of which remain unaltered when the temperature rises, and of a small quantity of a relatively strongly paramagnetic substance, the para-magnetism of which decreases as the temperature rises.

MESSRS. GEORGE PHILIP AND SON will shortly publish "The Exploration of Australia," by Mr. Albert F. Calvert. This book is designed to form a companion volume to Mr. Calvert's work, "The Discovery of Australia," and will trace the progress of maritime and land exploration from the period of Captain Cook, up to recent times.

A TRANSLATION, by Mr. W. E. Baxter, is announced of Van Heurck's important treatise on the Diatomaceæ. It will contain

about 2000 figures, illustrating every known genus of diatoms, and every species found in the North Sea and countries bordering it, including Great Britain.

THE second edition of "Elements of Marine Surveying," by the Rev. J. L. Robinson, lately published by Messrs. Macmillan and Co., contains several very useful additions and improvements. Young marine surveyors will find the volume an excellent aid to the study of the theoretical side of their profession, and would do well to include it in their outfit.

PARTICULARS of editions of Gilbert White's "Natural History and Antiquities of Selborne" have been compiled by Mr. Edward A. Martin, for the Selborne Society. Since the original edition was published in 1789, twenty-three other editions have appeared. The list compiled by Mr. Martin, gives the dates of the various editions, publishers, printers, editors, number of pages, and general description.

THE annual report of the Royal Botanic Gardens, Trinidad, for the year 1894, compiled by the Superintendent, Mr. J. H. Hart, furnishes evidence of the practical value of these colonial botanic gardens, and of their relation with the central institution at Kew. Under the Economic Section, information is given of the growth in the island of the sugar-cane, cacao, coffee, yam, gambier, vanilla, the Brazil nut, and cola, and of the principal enemies of these crops, and the best mode of combating them.

WE have received Part i. of "The Flowering Plants and Ferns of New South Wales," with especial reference to their economic value, by Mr. J. H. Maiden, assisted by Mr. W. S. Campbell, and issued under the authority of the Department of Mines and Agriculture for New South Wales. The present part contains descriptions and coloured drawings of four species—*Telopea speciosissima*, *Eucalyptus corymbosa*, *Actinatus helianthi*, and *Acacia glaucescens*. It is intended in this way to illustrate the principal flowering plants and ferns of the colony.

THE additions to the Zoological Society's Gardens during the past week include a Panolia Deer (*Cervus eldi*, ♂) from Hainan, presented by Mr. Julius Neumann; a Ruddy Ichneumon; (*Herpestes smithii*) from India, presented by the Earl of Hopetoun: a Spotted Ichneumon (*Herpestes nepalensis*) from India, presented by Mrs. Thompson; a Rosy-faced Love-Bird (*Agapornis pullaria* from West Africa, presented by Mr. Cecil M. Bevan; a Rufescent Snake (*Leptodira rufescens*) from South Africa, presented by Mr. J. E. Matcham; a Spiny Tree Porcupine (*Sphingurus spinosus*) from Peru, a Blossom-headed Parrakeet (*Palæornis cyanocephala*) from India, two Tuberculated Iguanas (*Iguana tuberculata*) from South America, deposited; two Guira Cuckoos (*Guira pirirgua*) from Para, purchased; a Japanese Deer (*Cervus sika*, ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MOTION OF THE SOLAR SYSTEM.—The methods elaborated by Argelander and Airy for the numerical solution of this problem have been followed with more or less variation by a host of investigators. As a rule the deviations in method have involved matters of detail rather than any fresh departure. Various suppositions have been made as to the motions of the stars themselves (*motus peculiares*): that the magnitude and direction of these motions have no connection with position, or that, in general, all these motions take place with the same angular velocity parallel to the galactic circle. Stars may be grouped according to their brilliancy, or the amount of their proper motion, or they may be arranged with more or less ingenuity according to their apparent position; but when the final equations are solved, the results are found to be fairly accordant. This fact has been recently demonstrated by M. Pannekoek, who, to vary the problem as much as possible, has based his investigations on the type of spectrum presented by the star. The zone from which the stars are selected is somewhat limited, being restricted to 0°—20° of declination, the spectra of which have been observed at Potsdam. The stars have been divided into four groups, according to the amount of the proper motion, with the following results:—

Stars of the First Type.

	No. of stars.	Centennial proper motion.	Position of apex. R.A. (α).	Declination (δ).
		"	°	°
I.	203	2·11	322·8 ± 19·2	+ 14·7 ± 7·0
II.	93	5·58	304·7 ± 4·6	+ 12·1 ± 3·4
III.	58	9·84	275·8 ± 6·1	+ 18·3 ± 3·6
IV.	48	34·36	251·6 ± 12·1	+ 33·0 ± 7·3

Stars of the Second Type.

I.	77	2·07	274·6 ± 9·6	− 2·6 ± 6·3
II.	52	5·93	280·1 ± 9·9	+ 35·8 ± 6·5
III.	65	20·85	268·6 ± 7·1	+ 31·4 ± 4·6

The result derived from stars of small proper motion of either the first or second type of spectra is scarcely accordant with previous investigations. The Right Ascension of the one and the Declination of the other are sensibly different from results involving larger numbers of stars. The author remarks, however, that all the values in R.A. can be rendered less discordant by an increase in the constant of precession of +0″·01, and in Declination by assuming a constant negative error in the proper motions themselves. Here we have again evidence that no rearrangement of groups materially alters the position assigned for the apex of the sun's way; but when processes sensibly different in their conception are employed, the accordance in the results is not so gratifying. For instance, the attempt to determine the position of the apex from Vogel's measurements of the motion of stars in the line of sight led to either of the two results, according to the method of "weighting" employed.

	I.	II.
α	206°·1 ± 12°·0	159°·7 ± 20·2
δ	+ 45·9 ± 9·2	+ 50·0 ± 14·3

Here, if the Declination be fairly satisfactory, the Right Ascension is hopelessly discordant. On the other hand, Dr. Kobold's treatment of the problem according to the graphical method suggested by Bessel, a method which does not easily lend itself to numerical treatment, gives a fairly satisfactory result in R.A., but the Declination will scarcely be accepted. The position assigned to the apex by this method is $\alpha = 266°·5$; $\delta - 3°·1$. This result is based on 1425 stars, and ought to be entitled to considerable weight if it could be satisfactorily demonstrated that all ambiguity, which arises from the definition of the poles of the great circles in which the proper motions take place, had been satisfactorily removed. This question is still *sub judice*, and while distinct methods give conflicting results, it is not wise to insist too strictly on the direction of the motion of the solar system.

THE ROTATION OF MARS. Among numerous observations of the planet Mars during the last opposition, Mr. Percival Lowell gave his attention to the measurement of the longitudes of some of the more conspicuous markings. The observations covered 36 points in all, and were made with a power of 440 on the 18-inch refractor of the Lowell Observatory. The first fact that emerged from the observations was that all the longitudes as given in Marth's ephemeris were affected by a systematic error of about 5°; or, in other words, the Martian features were retarded by about twenty minutes as compared with the computed times. The cause suggested for the discrepancy between the calculated and observed positions is that the received time of rotation of the planet is a trifle too small, and that the longitudes are consequently falling slowly behind their predicted times of meridian passage.

A somewhat similar discrepancy appears to have been noted by Prof. Keeler in 1892, who ascribed it partially to the constant error in estimating the position of the diameter of a large disc (*Astrophysical Journal*, May).

THE SUN'S STELLAR MAGNITUDE.—A new method of computing this important constant, being the number representing the sun's brightness on the scale in which the magnitudes of stars are represented, has been employed by Mr. Gore (*Knowledge*, June). Taking one of the outer planets, the known size and distance

enable us to determine the fraction of the sun's light which it receives, and correcting for the albedo, it is easy to calculate the brightness of the sun in terms of that of the planet, the exact stellar magnitude of which can be found by direct measurement. Thus, Mr. Gore finds that the apparent diameter of Mars in opposition, as seen from the sun, is 0″·17, so that the area of the disc is 20·9 square seconds. Dividing the number of square seconds in a hemisphere by the latter, it is found that if the surface of Mars were a perfect reflector, the sun as seen from Mars would be 8,940,450,000 times brighter than Mars appears to us when in opposition.

According to Zollner, the reflecting power of Mars is only 0·2672, so that the previous number must be raised to 33,459,768,000. This, however, is for mean distance 1·5237, so that when reduced to the earth's distance (by multiplying by the square of 0·5237), we get the light of the sun as seen from the earth to be 9,174,008,385 times the light of Mars when in opposition; this number, on the basis of a light ratio of 2·512 corresponding to a difference of 1 magnitude, represents 24·9 magnitudes. Prof. Pickering's photometric measurements show that the stellar magnitude of Mars at mean opposition is 2·25, so that the deduced stellar magnitude of the sun is −27·15. Similar calculations from the data relating to Jupiter give a value of −27·17, and from Saturn −27·11. Though agreeing so remarkably among themselves, these new values differ very considerably from the value hitherto adopted, namely −25·5. The new value, however, receives confirmation in the fact that it is very nearly equal to the magnitude which α Centauri would assume if it were brought to the sun's distance from the earth, assuming the parallax to be 0″·76, the spectrum of this star resembling the spectrum of the sun.

THE GREENWICH OBSERVATORY.

THE Report of the Astronomer Royal to the Board of Visitors of the Royal Observatory, Greenwich, was read at the annual visitation on Saturday. A few of the developments made during the year covered by the report, and some observations of interest, are referred to in the subjoined extracts.

Provision has been made in the Navy Estimates for the erection in Greenwich Park of a magnetic pavilion for absolute determinations of the magnetic elements, and the plans are now being prepared in the Director of Works' Department. It is proposed to establish this station in the immediate neighbourhood of the Observatory, and at such a distance that there would be no suspicion of disturbance from the iron in the buildings.

WORK WITH EQUATORIALS.

The flint and crown discs for the new photographic telescope of 26 inches aperture, the gift of Sir Henry Thompson, have been received at the Observatory. The details of the design for the mounting have been carefully worked out, and good progress has been made with the mechanical work.

The 28 inch refractor has been in use throughout the year, and is quite satisfactory. It moves easily in R.A. and Declination, the new slow motion screws work successfully, the water clock in general drives it with great precision, and the performance of the object glass under good atmospheric conditions is admirable. Various improvements in the accessories of the instrument have been carried out in the past year. A spectroscope specially adapted to photography, for use with this refractor, is being made.

Micrometer measures of sixty three double stars have been made; in 27 of these the distance of the components was under 1″ and in 13 it was 0″·5 or under. The most remarkable of these measures are those of κ Pegasi (β 989). The components of this star, though only 0″·14 apart, were distinctly separated with a power of 1030.

Measures of the positions of satellites of Mars near elongation were made on two nights. Several attempts were also made to measure Jupiter's fifth satellite, but the results obtained were [illegible]. A series of measures of the polar and equatorial diameters of Jupiter and his satellites was made. Measures of the diameters of Saturn and his rings and the positions of the satellites have also been made, and are being continued.

With the astrographic equatorial 595 plates, with a total of 1450 exposures, have been taken. Of these, 162 have been rejected for various reasons, such as: partially fogged plates; because the réseau lines were not clearly printed; because the images were too faint to show 9th magnitude stars with a twenty-seconds exposure; for faults in development; for mistakes of setting; and for miscellaneous defects. It is hoped that a much smaller number of plates will need to be rejected in future for these causes.

The total number of celestial fields photographed since the commencement of work for the chart is 422, and the total number of fields photographed for the catalogue is 617. Only half as many fields for the chart and catalogue have been photographed this year as during last year. This is due partly to the unfavourable weather, and partly to the telescope being out of use for two months while the shutter of the dome was being repaired.

SPECTROSCOPIC AND HELIOGRAPHIC OBSERVATIONS.

Since 1894 December 19, when the spectroscope was brought into adjustment, 98 measures have been made of the displacement of the F line in the spectra of 13 stars, and 16 of the *b* line in the spectra of four stars. Some experiments have also been made in photographing stellar spectra, to give data as to the work to be done with the new photographic spectroscope.

Photographs of the sun were taken with the Dallmeyer photoheliograph on 199 days, and of these 375 have been selected for preservation, besides 18 photographs with double images of the sun for determination of zero of position-angle.

The 9-inch photographic telescope presented by Sir Henry Thompson, which has been mounted on the Lassell equatorial, was also in regular use as a photoheliograph up to October 15, when the progress of the building operations prevented its further use. Photographs of the sun had been obtained with it by that time on 80 days, of which 121 have been selected for preservation. In all, with one photoheliograph or the other, a record of the state of the solar surface has been secured on 213 days during the year.

The mean daily spotted area of the sun was only slightly smaller in 1894 than in 1893, the marked falling off in the spring of 1894 noted in the last report being followed by an increase during the summer months. The number of sun-spots was greater than in 1893. The spring of this year has shown a decline both in the number and area of spots.

MAGNETIC OBSERVATIONS.

The variations of magnetic declination, horizontal force and vertical force, and of earth currents have been registered photographically, and accompanying eye observations of absolute declination, horizontal force, and dip, have been made as in former years. Increased magnetic activity was shown in the year 1894, and great disturbances occurred on July 20 and August 20; the spot of light of the vertical force magnet, on the former date, and the spots of light of the horizontal force and vertical force magnets, on the latter, having passed beyond the range of the registering sheets for some hours. In July and August the disturbances in the earth-current registers caused by the South London Electric Railway showed a great increase, which is presumably due to the experiments then being made in the use of motors on the carriages of the railway instead of separate locomotives.

The following are the principal results for the magnetic elements for 1894:

Mean declination	17° 4′·6 West.
Mean horizontal force	3·9661 (in British units).
	1·8287 (in metric units).
Mean dip	67° 16′ 5″ (by 9-inch needles).
	67 17 8 (by 6-inch needles).
	67 18 43 (by 3-inch needles).

In the year 1894 there were ten days of great magnetic disturbance and thirteen other days of lesser disturbance. Tracings of the photographic curves for all of these days are being made, and will be published in the annual volume according to the arrangements made with M. Mascart. The calculation of diurnal inequalities from five typical quiet days in each month has been continued.

METEOROLOGICAL OBSERVATIONS.

The registration of atmospheric pressure, temperature of the air and of evaporation, pressure and velocity of the wind, rainfall, sunshine, and atmospheric electricity has been continuously maintained, except that during the winter the register of atmospheric electricity was interrupted during the greater part of February by freezing of the water in the exit pipe.

The mean temperature of the year 1894 was 49°·9, being 0°·5 above the average for the fifty years 1841-1890. The severe frost which set in on December 30, and continued with slight intermission until March 9, was the most remarkable meteorological feature of the year. The cold wave, defined as the period during which the mean daily temperature was below the average, extended from 1894 December 30 to 1895 March 9, with a break from January 14 to 20, and on March 1, a period extending over seventy days in all. The total defect of mean daily temperature below the fifty years' average during this period was 489°, or 7°·0 per day.

A comparison with some of the coldest winters since 1841 is given in the following table:—

Period of cold wave.	Number of days.	Total defect of mean daily temperature.
1845 Jan. 27—March 21	54	443°
1855 Jan. 10—Feb. 24	46	467°
1870 Dec. 21—1871 Feb. 3	45	320°
1886 Jan. 5—March 18	73	408°
1890 Nov. 25—1891 Jan. 22	59	560°
1894 Dec. 30—1895 March 9	70	489°

The cold on the four days February 6, 7, 8 and 9 was particularly severe, the mean temperature being 18°·6 or 20°·5 below the average of the 50 years from 1841-1890, and there is no other instance of four consecutive days since 1841 with so low a temperature.

The lowest temperature recorded during the winter was 6°·9 on February 8, the lowest temperature in February since 1841, the next lowest being 7°·7 on 1845 February 12. Lower temperatures have been registered twice since 1841, viz. 4° on 1841 January 9 and 6°·6 on 1867 January 5. The mean temperature throughout the whole of February was 28°·9, or 10°·5 below the 50 years' average. The mean in February 1855 was 29°·2.

The mean daily horizontal movement of the air in the twelve months ending 1895 April 30 was 283 miles, which is slightly above the average. The greatest movement was 867 miles on December 22, and the least 50 miles on August 30. The greatest pressure of the wind was 36 lbs. on the square foot on March 24, with a velocity of 56 miles in the hour. During the gale of December 22, the greatest pressure recorded was 30 lbs., with a velocity of 50 miles in each of two hours.

The number of hours of bright sunshine recorded during the twelve months ending 1895 April 30 by the Campbell-Stokes instrument was 928 out of the 4454 hours during which the sun was above the horizon, so that the mean proportion of sunshine for the year was 0·208, constant sunshine being represented by 1. In the corresponding period for 1893-4, the number of hours of sunshine was 1364, and the mean proportion of sunshine was 0·306.

The rainfall in the year ending 1895 April 30 was 24·56 inches, which is very nearly the same as the average amount for the 50 years 1841-1890. The number of days on which rain fell was 187.

THE FIELD COLUMBIAN MUSEUM.

THE museum founded to commemorate the World's Columbian Exposition at Chicago has reached a stage which enables it to commence a series of publications designed to present to the world the results of research conducted under its auspices. The first of this series is before us, and is devoted to an historical account of the movement that resulted in the establishment of the museum. From this description we extract the following sketch of the early history of the museum, and of the general character of the contents.

The formation of a museum at Chicago, after the Columbian Exposition, was suggested by Prof. Putnam in 1890, and received the support of Prof. Goode, Director of the U.S. National Museum, Prof. Wilson, of the Smithsonian Institution, and other representative men. In the summer of 1893, a number of the prominent citizens of Chicago resolved "to establish in Chicago a great museum that shall be a fitting memorial of the World's Columbian Exposition, and a permanent advantage and honour to the city." The delicate and important task of securing the funds necessary to carry the resolution into effect was at once begun, but the appeal at first met with little response. A munificent gift from Mr. Field gave confidence in the assured prominence and success of the museum. Mr. G. M. Pullman followed with a subscription of 100,000 dollars, and a like sum was contributed by Mr. H. N. Higinbotham. Mrs. M. D. Sturgis gave 50,000 dollars, and a number of other donations for various amounts were made, as well as Exposition stock having the approximate par value of 1,500,000 dollars. With these funds in hand, the museum committee felt justified in making extensive purchases, including the exhibits from Paraguay, Peru, Java, Samoa, the Hagenbeck collection, and the Ward collection of natural history, for which a sum of 95,000 dollars was paid. The new President of the museum, Mr. E. E. Ayer, presented the Ayer anthropological collection, valued at 100,000 dollars, to the museum, and other donations of material followed. Many exhibits were purchased at the close of the Exposition, and these furnished the broad foundation upon which the present collections have been built. Great gaps in the continuity of separate subjects have thus been, to a large degree, obviated, until

The Field Columbian Museum.

to-day, from one end of the museum to the other, can be traced the story of nature and of man and his works.

The collections illustrating geology in the museum are grouped into Systematic Geology and Economic Geology. In the former division there are about five thousand palæontological specimens, many of them especially instructive and valuable, and as many specimens of minerals, classified according to the chemical constitution of each species. The collection of meteorites in the same division includes several very large specimens, notably the meteoric stone from Phillips County, Kansas, weighing 1184 lbs.; two masses weighing respectively 465 and 344 lbs., with several smaller ones from the meteorite of the Kiowa County, Kansas; two masses weighing 1013 and 265 lbs. respectively, and several smaller ones of the Cañon Diablo, Arizona, meteorite; about 650 individual aerolites of the Winnebago County, Iowa, fall, and many other specimens. Physical geography, structural and dynamical geology, and lithology are also well represented in the division of systematic geology.

The collections of the division of Economic Geology were obtained through the Chief of the Department of Mines, Mining and Metallurgy of the World's Columbian Exposition, from exhibits made in that exposition. Being designed to illustrate the practical bearings of the science of geology, they consist chiefly of specimens which show modes of occurrence in nature of minerals having economic importance, and the localities where they may be obtained. In addition to these, however, are

many illustrations of the processes employed in the extraction and treatment of minerals or ores, and of the application of resulting products to human arts and industries.

An immense amount of material, illustrative of the botany and forestry of all parts of the world, came into the possession of the museum at the close of the Exposition. These exhibits are gradually being arranged in geographical sequence, but some time must elapse before all the specimens can be fully identified and labelled.

The Department of Zoology includes all the classes of animals except birds, and six large halls of the museum building are set apart for the specimens belonging to it. The mounted collection of birds in the Department of Ornithology is essentially one of comparative ornithology, in which the bird fauna of the world is represented by some 650 species. North American bird-life is at present only represented by some 150 species out of a possible 825. Among the treasures of which the museum can boast, however, is a pair of the now (probably) extinct Labrador Duck (*Camptolaimus Labradorius*).

The extensive exhibits illustrating the archæology and ethnology of America, brought together by Prof. F. W. Putnam, were transferred to the museum at the close of the Exposition. A number of other very important collections, representing primitive culture in many widely separated regions of the world, were also obtained. Belonging to the Department of Anthropology are psychological and physical laboratories, and collections of cranic casts, &c., illustrating the physical characteristics of man.

During the Exposition a great group of exhibits had been brought together within the Department of Transportation, to illustrate the evolution of the carrying industry, beginning with its inception in remote times, and extending down to the present day. These exhibits were transferred to the museum building, and largely augmented by collections from other departments. All of this material, together with a number of exhibits illustrating other industries of especial importance to civilised man, including ceramics, the textile art, the leather industry, jewellery, &c., have been brought together in a Department of Industries. The collections in this department have been arranged to show, as far as possible, the more important steps which have led to improvement in handiwork, or progress in the invention of those implements, machines, and processes which have proved to be important factors in the world's material development.

Although but a few months have elapsed since the doors of the museum were publicly thrown open, a course of popular lectures have been inaugurated, a publication series established, and several scientific expeditions sent into the field for augmenting its collections. In these and other directions, the Field Columbian Museum appears to be advancing along the path marked out for it, and performing its part in adding to the wealth of Western civilisation and culture.

PRIZE SUBJECTS OF THE FRENCH SOCIÉTÉ D'ENCOURAGEMENT.

THE prizes and prize subjects of the French Société d'Encouragement pour l'industrie nationale, for 1896 and 1897, are described in the *Bulletin* of the Society. The Society's Grand Prize of 12,000 francs will be given this year to the author of the discovery most useful to French industry. The following list shows the arrangements with regard to the prizes of the two succeeding years:—

1896.

Grand medal to the author, of any nationality, of works that have exercised the greatest influence on the progress of French mechanical arts during the preceding six years.

The Henri Giffard prize of 6000 francs for signal services to French industry. The Parmentier prize of 1000 francs for researches tending to improve the material or processes of agriculture and alimentary industries. The Meslens prize of 500 francs for the author of an application of physics or chemistry to electricity, ballistics, or hygiene.

In the section of Mechanical Arts, a prize of 3000 francs is offered for the best motor fed with some commercial oil. Other prizes are: 3000 francs for an engine of from 25 to 100 horse-power, using as a maximum, when working, $7\frac{1}{2}$ kilogrammes of steam per hour and per indicated horse-power; 2000 francs to the manufacturer who first produces, mechanically, linen threads of which at least 100,000 metres go to one kilogramme, or, in the case of hemp, 15,000 metres per kilogramme; 2000 francs for an investigation, or a method tending to prevent, or at least reduce in amount, the leakages, known as "fuites aux tubes," in marine boilers; 1000 francs for the best memoir on the cost price of the motive power of steam; 2000 francs for a small motor suitable for a home workshop, and which will work by the use of some simple power available in the house, or by energy transmitted from a central station; 3000 francs for improvements in the processes of retting linen and hemp in industrial use.

The prizes offered in the section of Chemical Arts are: 1000 francs for the utilisation of waste products; 2000 francs for a work or memoir of use to chemical or metallurgical industry; 2000 francs for an experimental study of the physical or mechanical properties of one or more metals or alloys, selected from those which are in current use; 2000 francs for a new process for the production of fuming sulphuric acid, or sulphuric anhydride; 2000 francs for an improvement in the manufacture of chlorine; 1000 francs for the discovery of a new alloy useful to the arts; 2000 francs for a scientific study of combustion in the furnaces used for the production of gas; 2000 francs for an investigation of the expansion, elasticity, and tenacity of ceramic clays and coverings; 1000 francs for the substitution of sulphuric acid in dyeing, and especially in silk dyeing, by another compound which will give to the fibres the desired stiffness, without exercising any destructive action; 2000 francs for an investigation of the physical and mechanical properties of glass; 2000 francs for the discovery of processes capable of yielding, by certain chemical changes, useful organic products, such as quinine, cane-sugar, &c.; 2000 francs for an investigation on an industrial process of which the theory is but imperfectly known; 2000 francs for the production of cast steel or iron having useful properties, by the incorporation of a foreign substance.

In Economic Arts the following are the prizes and subjects. A prize of 2000 francs for the invention of a new process in which at least 0·800 kilogrammes of petroleum can be used without danger, as a source of light or heat, either in industry or in domestic economy; 2000 francs for the discovery of methods to diminish the number of chimney fires, and reduce the damage which results from them; 2000 francs for an incandescent electric lamp of one-tenth candle power when a current of 0·05 ampere is passing through it at a potential of 100 volts.

In Agriculture the prizes and subjects are as follows:—2000 francs for the best investigation of the comparative physical and chemical constitution of the soils of one of the natural or agricultural regions of France; 1500 francs for the best varieties of barley for brewing; 3000 francs for the re-establishment of vineyards on chalk soils; 1500 francs for the introduction and culture, on a large scale, of a new forage plant; 2000 francs for the best study of the culture of the vine in various regions of France, and of the influence of various processes of vinification on the quality of wine.

A prize of 1000 francs is offered for the discovery of a plastic material, similar in appearance to some stone, marble, or brick, and hard enough to be used either for the insides or the outsides of houses; 1000 francs for the discovery of a process to prevent woods used by carpenters and cabinet-makers from deformations by atmospheric influences; 1000 francs for the author of the best memoir on some practical process other than a chemical process, and capable of being applied in the workshop, for the detection of adulterated Portland cement.

1897.

A prize of 2000 francs is offered for improvements in the methods of grinding grain; and a prize of 2000 francs for a motor weighing less than fifty kilogrammes per horse-power. This prize is offered with the idea of furthering the problem of aerial navigation. A second prize, having the same object, is for a study of the coefficients necessary to the mechanical calculation of an aerial machine. There is also a prize of 3000 francs for improvements in the manufacture of permanent magnets; and prizes of 3000 francs for an investigation of alcoholic ferments, and 2000 francs for the best investigation of the deterioration of cider, and the means to prevent the changes to which the loss of vivacity is due.

The prizes are open to investigators of any nationality, but the memoirs, and descriptions of inventions, should be written in French. Models, memoirs, descriptions, and specimens intended to compete for prizes must be sent to the Secrétariat de la Société d'Encouragement pour l'industrie nationale, 44 rue de Rennes, Paris. Competitors for the prizes of 1896 must send in before the end of the present year; the latest time for entering memoirs, &c., for the 1897 competition is the end of 1896.

RECENT GLACIAL STUDIES IN GREENLAND.[1]

DURING the summer of 1894, Mr. Chamberlin was enabled to devote some time to a personal study of the glaciation of Greenland, and the results of his observations are so interesting, that all geologists who seek to interpret the records of the "Great Ice Age," will gladly make acquaintance with them. Seldom has a geologist so experienced in the study of glacial drifts and of the problems connected with them, had the advantage of examining the behaviour of ice in the Arctic regions.

His observations were specially directed to the way in which a glacier gathers up detritus along its course, to the way in which it carries it forward and finally puts it down. The main problem he sought to solve, was connected with the basal material of glaciers, débris which, of course, is largely concealed.

In comparing the glaciation of Greenland with that of the mainland of North America, he had to bear in mind, that for the most part the continental drift is spread over a vast plain. In Greenland the ice-fields rest mainly on plateaus fringed by rugged mountains, and he sought for a tract free from such bordering elevations. This was found at Inglefield Gulf, where the borderland is a plateau about 2000 feet above sea-level, and where the margin of the great ice-sheet may be studied on relatively smooth ground, on undulating ground, and in lobes or tongues that descend the valleys. Of the thirty or forty glacial tongues which descend towards Inglefield Gulf, less than one-third reach the shore, and scarcely one-half of these discharge notable icebergs. The majority terminate in valleys whose bottoms are formed of glacial débris, and whose lower gradients are moderate.

The fact that great part of Greenland appears to consist of ancient gneissic rocks, renders the débris more or less stony and arenaceous; clayey material is rare. About Inglefield Gulf, however, the older rocks are covered by thick layers of sandstone and shale, traversed by basic igneous dykes. Hence it is possible there to tell how late the erratics from this sedimentary series were introduced into the ice, to ascertain what courses they pursued, and the actions they suffered.

The margins of the Inglefield glaciers rise abruptly like escarpments of rock, 100 or 150 feet or more. The layers of ice are cut sharply across, exposing their edges; and the formation of these scarps is attributed to the lower inclination of the sun's rays, which strike vertically and effectively against the edges of the glacier, whereas its back is affected only by rays of low slant.

The stratification of the glaciers attracted particular attention. The ice was found to be almost as distinctly bedded and laminated as a sedimentary rock. The vertical face was seen usually to present two great divisions an upper tract of thick, obscurely laminated layers of nearly white ice, and a lower laminated tract discoloured by débris. At the base there is usually a talus-slope, and sometimes there is a moraine. In the lower portion of the ice there are, here and there, interstratified layers of sand and silt, rubble and boulders. These vary from a mere film of silt to a heterogeneous mixture of débris and ice several feet thick. The detritus is usually arranged in definite and limited horizons, the ice above and below being firm, clean, and pure. Often a fragment of rock, or a boulder of considerable dimensions, will be several times thicker than the silt layer, and it projects above and below into the clean ice. The débris-layers, though often regular and persistent, frequently thin out and disappear. Lenses of débris also appear, and the layers are sometimes doubled back upon themselves.

The laminæ of the ice are sometimes very symmetric, straight, and parallel, but often wavy and undulating. In many instances they are greatly curved or contorted. Thus, as Dr. E. von Drygalski has remarked, they closely simulate the foliation and contortion of gneiss.

The débris-belts, which are essentially parallel to the base of the glacier, are confined chiefly to the lower 50 or 75 feet, but they occur up to 100 feet and, perhaps, to 150 feet. They are more abundant at the sides of the lobes than in the centre; a notable portion of the débris having evidently been introduced after the lobes were formed. Thus the detritus appears most abundant in glacier-lobes which descend as cataracts, or crowd between closely hugging cliffs.

In meeting obstacles the basal beds of the glacier sometimes simply curve upwards, carrying their débris with them over the obstacle; at other times, the laminæ of ice are much crumpled. Not only are the foliations of the ice twisted, but they are at times fractured and faulted, and along the fault-plane the laminæ are affected by "drag," as in faulted rocks.

The general stratification of the ice had its initial stages in the original snow-falls; and the seasons doubtless developed annual subdivisions. The more definite partings and the introduction of the layers of débris, arose through a shearing movement between the layers of ice.

The actual process of intrusion of detritus was observed in proximity to a large boss of rock which, protruding through the margin of the ice, had been partially cut away. Trains of débris, apparently rubbed from the surface of the rocky dome, were carried out almost horizontally into the ice in its lee. Some of these were short, while others extended several rods into the ice, passing into the body of it instead of following its base. At one point the overthrust of the ice reached such a degree as to carry the earthy layers obliquely across the thickness of the glacier, producing a marked unconformity.

In another instance similar features were observed below an ice-cataract. Tongues of débris, having their origin in the boulder-clay below the glacier, were seen to reach out into the basal portion of the ice as though they were being introduced into it by the differential movement of the layers upon each other. Thus when the ice is forced over a prominence it settles down a little in its lee, and is then protected somewhat from the thrust of the ice behind. The next ice that passes over, being prevented by the former portion from settling down at once, is thrust forward over it. This is accomplished by the bending and doubling of the layers, and also by distinct shearing. At length, however, the first layer is compelled by the general friction to move somewhat forward, and in time to join the common moving mass, carrying the overthrust layer of débris between it and the ice-layer above.

It appears obvious that the ice in the lee of a rocky prominence moves more slowly than that above; hence the doubling of the laminæ upon themselves. Moreover, there is a gradation from laminæ that simply suffered doubling up, to layers that obviously sheared upon each other and produced manifest unconformity by overthrust.

Evidence showed that the more solid (blue) bands in the ice are produced by exceptional pressure in moving over rugosities, and that their position in the ice is parallel to the ice-movement; while at the same time blue bands may be developed nearly at right-angles, after the manner of slaty cleavage.

Summarising the above conclusions, it appears that stratification originated in the inequalities of deposition, emphasised by intercurrent winds, rains, and surface meltings; that the incipient stratification may have been intensified by the ordinary processes of consolidation: that the shearing of the strata upon each other still further emphasised the stratification, and developed new horizons under favourable conditions: that basal inequalities introduced new planes of stratification, accompanied by earthy debris, and that this process extended itself so far as even to form very minute laminæ.

There is involved in the foregoing conceptions the idea of an ice-layer acting as a unit of movement: at any rate, there is recognised individuality of movement in the layer. This view involves the idea of rigidity rather than viscosity. The introduction of earthy material into the ice-layers involves the idea of thrust rather than pull. The picture is not that of gravitation pulling a thick, stiff liquid down the lee side of a rocky prominence, but of a rigid body thrusting itself over the crest by means of a force in the rear.

The extreme fragility of the ice is difficult to harmonise with the idea of viscosity. Wherever the ice passed over an undulation of even moderate dimensions, it was abundantly crevassed. There was no indication that boulders descend through the ice as heavy substances descend through viscous bodies. The rigidity did not prevent contortions and foldings of the laminations, such as take place in crystalline rocks, but faulting and vein-structures also occur; and there seems no more occasion to assume viscosity in the one case than in the other. Even if a certain measure of viscosity be admitted, it does not follow that viscosity was an essential agency of motion. The crystalline body may readily be made to change its form by the removal of particles from one portion by melting, and their attachment at other points by congelation; but not, apparently, by the flowing of crystallised particles over each other in their crystalline conditions.

It has been already pointed out that much basal material is

[1] Abridged from a paper by T. C. Chamberlin, in the *Bulletin* of the Geological Society of America, February 1895.

carried in the lower layers of ice. It was also a matter of frequent observation that débris lies under the ice. Apparently the ice sometimes pushes this along, and sometimes slides over it. At the end of the glacier the debris within the ice is freed by melting, and accumulates as a talus-slope. This sometimes protects the basal layers from melting, and they become at length incorporated in the growing accumulation.

It appeared, from the stages presented by the several glaciers, that where the ice is slowly advancing, the talus-slope gradually grows forward and constitutes an embankment, upon which the glacier advances. It thereby grades up its own pathway in advance. On seeing this process, one is at no loss to understand how ice can advance over fields of sand or soil without in any way disrupting them. It buries them before it advances upon them.

Where the frontal material accumulates in a large mass, it opposes such a degree of resistance to the ice that its layers are curved upward on the inner slope; and if the glacier subsequently advances, the ice rides up over the moraine. Several such instances were observed, but none was seen where the ice showed any competency to push even its own débris, in notable quantity, in front of it. The ice is weaker than the moraine as a whole.

Great quantities of snow are carried by winds from the region of the great ice-cap, and this snow may be lodged in immense heaps in the lee of the terminal moraines. Such a border-drift may have a breadth of from 1000 to 3000 feet. It becomes solidified after the fashion of a glacier, and may serve to arrest or deflect the main ice; for it was observed that the basal layers of the ice in places curved upwards on encountering the resistance of this wind-drifted accumulation.

The rate of movement of the majority of the glaciers was found to be exceedingly slow, though a few which produce large icebergs are notable exceptions.

The amount of drift on the territory once occupied, but now free from ice, was scanty. At some points there are considerable accumulations of drift within a mile or two of the present ice-front, but over much of the area no great moraines, nor any thick mantles of drift, were to be seen. There was but moderate evidence of glacial action; the land was gently rounded, but not greatly moulded. In this area of Southern Greenland tracts of angular, unsubdued topography alternate with rounded, flowing contours. The inference was drawn that the ice formerly so extended itself as to reach the present coast for about half its extent, while in the remaining portion the ice fell short. Thus the conclusion seems unavoidable that the ice of Greenland, on its western side, at least, has never advanced very greatly beyond its present border in recent geologic times. This carries with it the dismissal of the hypothesis that the glaciation of the mainland of North America had its source in Greenland.

There is no ground to question the former elevation of Greenland, but it would appear that this was not coincident with conditions favouring glaciation. H. B. W.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD. Among the distinguished men upon whom it is proposed to confer the honorary degree of D.C.L., on June 26 are Sir W. H. Flower and Prof. Michael Foster.

In a Convocation held on Tuesday, the statute appointing Dr. E. B. Tylor professor of Anthropology during the tenure of his office as Reader in Anthropology was finally approved. In a Congregation, held on the same date, the Statute on Research Degrees received the final approval of the house, and it now remains for it to be passed by Convocation. The proposed Final Honour School of Anthropology was again brought before Congregation, and excited some opposition. On a division the proposed statute was carried by a considerable majority, the numbers being: Placet, 47; Non placet, 28. The statute has now to run the gauntlet of Convocation before it finally passes into law. In the same Congregation, the dates of the preliminary examinations in the Honour Schools of Natural Science were fixed for the Monday after the eighth week of Full Term in Hilary Term in each year, instead of in the last week or last week but one, as has hitherto been the custom; and the grant of £300 per annum to the Chemical department of the University Museum was renewed for a period of five years. The published lists of the candidates for the final and preliminary examinations in Natural Science show that there are 44 candidates in the final school and 64 candidates in the preliminary school. These figures do not include women students.

CAMBRIDGE.—The following is the speech delivered by the Public Orator, Dr. Sandys, on May 30, in presenting for the honorary degree of Doctor in Science, Dr. John Murray, editor of the *Challenger* publications.

Meministis omnes poetae nostri maximi locum insignem, ubi Northumbriae Ducis filius acerrimus non recusavit gloriam aut ex ipsa luna audacter deducere, aut maris in profundo demersam extrahere, modo solus sine rivali laudem omnem sibi vindicaret. Quanto pulchrius autem rerum naturae penetralia intima assidue perscrutari, eque oceani altitudine immensa laudem cum sociis optimis participatam reportare. Adest unus ex illis qui, plusquam tribus annis in oceano explorando fortiter toleratis, ut poetae antiqui verbis sensu novo utar,

"referebant navibus altis
occulta spolia, et plures de pace triumphos."

Una saltem nominis bene ominati navis velut ipsam rerum naturam ad certamen provocavit, ipsamque veritatem in profundo abstrusam orbi terrarum patefecit. Tanti autem itineris monumenta, quinquaginta voluminum in serie ingenti a collegis plurimis parata, viri huiusce praesertim industria infinita non modo adaucta et summatim descripta sed etiam ad terminum felicem perducta et diei in lucem prolata sunt. Quid non potuit rerum naturae, quid non potuit veritatis amor?

"Merses profundo; pulchrior evenit."

Duco ad vos Universitatis Edinensis alumnum, oceani indagatorem indefessum, virum etiam in posterum sine dubio laudem indies maiorem meriturum, IOANNEM MURRAY.

The Master of Downing (Dr. Hill) and Dr. Barclay-Smith will give a course of instruction in Practical Histology during the Long Vacation, beginning on July 6.

The State Medicine Syndicate propose to make a grant of £50 to the Department of Pathology, in aid of the course of laboratory instruction in Bacteriology therein provided for candidates for the diploma in Public Health.

Prof. Ewing's serious illness has made it necessary to appoint Mr. Dalby, Demonstrator in the Engineering Laboratory, to act as Examiner for him in the Mechanical Sciences Tripos.

The Smith's Prizes in Natural Philosophy have been awarded (1) to G. T. Manley, of Christ's College, for his essay on "The Conformal Representation of a Quadrilateral on a Half Plane," and (2) to G. H. J. Hurst, of King's College, for his essay on "Electro-magnetism and Magneto-optic Rotation." Mr. Manley and Mr. Hurst were respectively Senior and Second Wrangler in 1893. The essays of H. E. Atkins, of Peterhouse, and P. E. Bateman, of Jesus College, are declared worthy of honourable mention. Mr. Atkins was bracketed Tenth Wrangler, and Mr. Bateman bracketed Fifteenth Wrangler in the same Tripos.

Mr. S. S. Hough, of St. John's College, has been elected Isaac Newton Student in Astronomy for the three years ending June 15, 1898.

Mr. Charles Chree, Director of the Kew Observatory, has been approved for the degree of Doctor of Science.

Mr. W. N. Shaw has been appointed Chairman of the Examiners for the Mechanical Sciences Tripos, in the room of Prof. Ewing, who has resigned on the ground of illness.

Mr. Charles Smith, Master of Sidney Sussex College, has been elected Vice-Chancellor for the ensuing academical year.

Classes in Osteology, in General Chemistry, in Geology, and in Experimental Physics, are announced to be held in the Long Vacation.

Mr. A. E. Shipley, University Lecturer in Invertebrate Morphology, has been appointed a member of the University Press Syndicate.

PROF. W. T. A. EMTAGE, of University College, Nottingham, has been elected Principal of the Technical Institute, Wandsworth.

HONORARY degrees were conferred, by the Chancellor of Victoria University, last week, upon Lord Kelvin and Sir Henry Roscoe, among others, for distinguished services rendered to the University.

THE twelfth annual report of the Mitchell Library, Glasgow, is before us. The library is open to the public, and is adminis-

tered by a committee of the Glasgow Town Council, from which it obtains a grant of £2000 a year, from the moneys received under the Local Taxation (Customs and Excise) Act; it is also fortunate in being the recipient of several bequests from persons interested in its work. A noteworthy point is that, out of a total of 112,447 volumes contained in the library, no less than 20,812 are classified under "Arts, Sciences, Natural History." This is two thousand volumes more than are included under any other head. The most important accession to the library during the three years covered by the report (1892-94) consists of a complete set of the *Transactions* of the Royal Society, in 183 volumes. A very valuable addition to the scientific resources of the library has resulted from agreements entered into with the Glasgow Natural History Society, and with the Glasgow Geological Society. These societies have transferred to the library their sets of the *Transactions* and *Memoirs* of foreign scientific societies, the Library Committee undertaking on their part to continue to the members their privilege of borrowing the books, to bind such as required it, and to bear the expenses attending the printing extra copies of the *Transactions* of the Glasgow societies, and forwarding the same to the foreign societies as an exchange. During last year, 115,788 scientific works were issued, the daily average being 386. It would be well if there were more public libraries conducted on the enlightened plan of the Mitchell Library.

ANOTHER library of which we have received the report (in this case the first report) is that of St. George, Hanover Square. Though on a much smaller scale than the Mitchell Library, the Commissioners appear to aim at making the library a means of education as well as of recreation. There are 11,860 volumes in the lending library, of which twenty per cent. are fiction, and 6206 in the reference library, none of which are novels. To obtain a satisfactory conclusion as to the work of a library, the use made of the library as a whole, and not of any particular department, ought to be taken into account. The records of the institution show that out of 416,760 visitors during the year, only thirteen per cent. of the readers went for the purpose of borrowing works of fiction from the lending library. A noteworthy feature in connection with the library is a museum of objects arranged as an elementary and self-explanatory collection, as an introduction to larger museums of natural history.

IT is proposed to hold a Technical Education Conference at the Society of Arts on June 20. The Society has addressed a letter to Technical Education Committees, asking them to send delegates to the Conference. Among the subjects to be considered is the "lack of a central organisation which might deal especially with such questions as the examination and inspection of classes. In spite of the valuable work which has been done by the City and Guilds of London Institute, and by other bodies, it is only in a portion of the subjects sanctioned as subjects of technical instruction that examinations are held. The wide field of agriculture and home industries is untouched; while no means are provided for anything like a general system of inspection which local authorities may call to their aid should they desire to do so." There are also other points with regard to which common action would be desirable, and it is hoped that by bringing together those who are interested in technical education the best way in which the Society can enlarge the scope of its present action in connection with the subject will be found.

THE Technical Instruction Committee of the Essex County Council have arranged for a short course of elementary instruction in horticulture, to be given at the County Technical Laboratories, Chelmsford, during the first three weeks in July. The course of study is intended to give sound elementary instruction in the cultivation of plants, based upon a knowledge of plant physiology. The teaching throughout will be practical; every lecture will be abundantly illustrated and immediately followed by demonstrations and individual practical work by the students themselves.

SCIENTIFIC SERIALS.

Internationales Archiv für Ethnographie, Band viii. Heft ii.—On the ethnography of Matty Island, by Dr. F. von Luschan. Although Matty is a small island, about ninety-three miles north of German New Guinea, between 142° and 143° E. long., Dr. von Luschan comes to the conclusion that the natives are not Melanesians; they are much lighter than almost any Melanesians, some being of a deep red flesh colour, eyes slit-like, nose narrow, hair black and in long locks. Of the thirty-eight weapons and utensils in the Berlin Museum not one can with certainty be allocated to any known culture-mixture; any Micronesian resemblance is purely superficial. It seems probable that the people have remained isolated for at least 300 years. Three plates of utensils, &c., illustrate the paper.—Dr. O. Schellong's note on some Melanesian drawings is illustrated by two coloured plates, and is supplemented by some notes by J. D. E. Schmeltz. The drawings are interesting as showing how unlike the objects intended native delineations may be. It is to be hoped that more illustrations of this aspect of the art of savages will be forthcoming. Of the notices of recent publications, those on "Arrow-poison" and "Ethnological Botany" are especially interesting.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 2.—"Alternate Current Dynamo Electric Machines." By J. Hopkinson, F.R.S., and E. Wilson.

The paper deals experimentally with the currents induced in the coils and in the cores of the magnets of alternate current machines by the varying currents in and the varying positions of the armature. It is shown that such currents exist, and that they have the effect of diminishing to a certain extent the electromotive force of the machine when working on resistances as a generator without a corresponding effect upon the phase of the armature current. It is also shown that preventing variations in the coils of the electromagnet does not, in the machine experimented upon, greatly affect the result, and that the effect of introducing copper plates between the magnets and the armature has not a very great effect upon the electromotive force of the armature, the conclusion being that the conductivity of the iron cores is sufficient to produce the main part of the effect. A method of determining the efficiency of alternate current machines is illustrated, and the results of the experiments for this determination are utilised to show that in certain cases of relation of phase of current to phase of electromotive force, the effect of the local currents in the iron cores is to increase, instead of to diminish, the electromotive force of the machine.

May 9.—Bakerian Lecture: "On the Laws of Connexion between the Conditions of Chemical Change and its Amount." By A. Vernon Harcourt, F.R.S., and William Esson, F.R.S. "III. Further Researches on the Reaction of Hydrogen and Dioxide and Hydrogen Iodide."

In this paper are considered the effect upon the reaction of (1) substances not directly participating in reaction, (2) temperature.

The general conclusion as to the effect of the medium upon the reaction is expressed as follows:

Each constituent of the medium produces an effect on the rate of change of unit peroxide and unit iodide, proportioned to the mass, and varying with the nature of the constituent. The increment of this rate per unit mass of each constituent is constant so long as the quantity of the predominant constituent present in the medium is sufficiently large, in comparison with the other constituents of the medium, to render the media in successive experiments practically homogeneous. For example, when the ratio of the numbers of H^2SO^4 and HI in the medium exceeds 20, the formula for the rate at a given temperature is

$$\alpha = i\{a + b(i-1) + ds\},$$

a being the theoretical rate with unit of HI, b the increment per unit of hydrogen iodide per unit of iodide, and d the increment per unit of hydrogen sulphate per unit of iodide. If the ratio falls below 20 the formula is

$$\alpha = i\{a + b'(i-1) + d's\},$$

in which b' and d' depend upon the relative masses of sulphate and iodide present in the medium.

Variation of Temperature.

The discussion of the numerous experiments made at temperatures ranging from 0° to 50°, in media in which the quan-

tities of iodide range from 3·64 HI. to 23 HI. the quantities of hydrogen sulphate from 45 H^2SO^4 to 408 H^2SO^4, and the quantities of hydrogen chloride from 70 HCl to 547 HCl, leads to the following law of connexion between chemical change and temperature.

If α_1 is the rate of chemical change at a temperature $t_1°$ in a homogeneous medium consisting of given constituents per unit volume, and α_2 is the rate at a temperature $t_2°$ in the same medium, the ratio of α_1 to α_2 is $\{(273 + t_1)/(273 + t_2)\}^m$, m being a constant depending upon the character of the constituents of the medium. When the temperatures are measured from the absolute zero − 273°, and are denoted by T_1, T_2, the formula assumes the simpler form,

$$\alpha_1/\alpha_2 = (T_1/T_2)^m.$$

The constancy of the value of m for a particular medium is secured when the quantity of the predominant constituent of the medium is sufficiently large in comparison with the quantities of the other constituents to make the medium practically homogeneous. When this is not the case the value of m has some value intermediate to the values which it has when one or other of the constituents is sufficiently predominant to secure a constant value.

In media in which hydrogen sulphate is sufficiently predominant, the value of m is 20·38; similarly for hydrogen chloride the value of m is 21·17. When the medium consists of water and hydrogen iodide, the value of m is 24·1. The introduction of sodium sulphate in large quantity into a medium otherwise consisting mainly of hydrogen sulphate reduces the value of m from 20·38 to 18·1. In a medium in which the main ingredient is sodium hydrogen carbonate, the value of m is approximately 10.

A further confirmation of the law of connexion between chemical change and temperature is obtained from the discussion of experiments on the rate of change of hydrogen chlorate and potassium iodide made by W. H. Pendlebury and M. Seward. The value of m is in the case of this chemical change 40·5.

It follows from the law enunciated above that at the temperature of absolute zero no chemical change can take place.

If the smallest value of m, viz. 10, is taken, a chemical change, which is completed in one minute at a temperature zero, would require for its completion, at a temperature of −200°, a little more than a year. If 20 is taken as the value of m, the minute would be increased to more than half a million of years by the same reduction of temperature.

The law enunciated above may also be stated in the following form.

The increment of each unit of chemical change due to a rise of temperature varies as the increment of each unit of absolute temperature.

This law is expressed by the formula

$$D\alpha/\alpha = mDT/T.$$

Chemical Equilibrium.

A case of equilibrium between the reactions

$$H^2O^2 + 2HI = 2H^2O + I^2,$$
$$2H^2O + I^2 = H^2O^2 + 2HI.$$

leads to a discussion of the general equations of chemical equilibrium, which is given in an appendix to the paper. These equations are employed to interpret the results of experiments published by Dr. Gladstone in the *Transactions* of the Royal Society (*Phil. Trans.*, vol. clxv.). They had been previously applied to the case of chemical equilibrium investigated by Prof. Dixon, in a paper published in vol. clxxv. of the *Transactions* of the Royal Society, the reactions in that case being

$$H^2O + CO = H^2 + CO^2,$$
$$H^2 + CO^2 = H^2O + CO.$$

Physical Society, May 24.—Captain W. de W. Abney, President, in the chair.—Dr. Kuenen read a paper entitled "On the condensation and the critical phenomena of mixtures of ethane and nitrous oxide." If the vapour of a pure substance is compressed at constant temperature, then when a certain pressure is reached the vapour commences to condense, and the pressure remains constant until all the vapour is liquefied. Taking the pressure and temperature as coordinates the corresponding temperatures and pressures at which liquefaction takes place are plotted, the curve obtained is called the vapour pressure curve, and this curve ends at the critical temperature and pressure of the given substance. On the other hand, if a mixture of two vapours is compressed at constant temperature the pressure no longer remains constant while condensation is taking place, but gradually rises. The points at which condensation commences and ends lie on a U-shaped curve having its vertex turned towards the direction of increasing temperatures. Such a curve the author calls a "border curve." The point at which a line parallel to the axis of p touches a border curve corresponds to the critical point (R) of the given mixture. For all temperatures higher than that corresponding to R there is no condensation into liquid possible, while for any temperature below the critical temperature there are two vapour pressures, one corresponding to the commencement, and the other to the conclusion of liquefaction. The envelope of all the border curves for mixtures containing different proportions of the two bodies is a curve, called the plait-point curve, joining the critical points of the two constituents. The point of contact (P) of a border curve with the plait-point curve corresponds to the plait-point on van der Waal's, thermodynamic surface. If when we go along the border curve, starting from its lower branch, we first reach R and then P, and if we indicate the temperatures corresponding to P and R by T_P and T_R, then for temperatures between T_P and T_R as the pressure is increased the quantity of liquid firts increases, reaches a maximum, and after that decreases till it disappears. This is called retrograde condensation of the first kind, and has been observed by the author in the case of mixtures of methyl chloride and carbon dioxide. If P, however, lies beyond R the process of condensation for temperatures between T_P and T_R is different. In this case the volume of vapour increases, reaches a maximum, and then decreases. This constitutes retrograde condensation of the second kind. It was with a view to the experimental observation of this second kind of retrograde condensation that the author undertook his observations. A series of observations were made with each of the pure gases, and gave the following values for the critical temperature:—

Ethane	32°·3 C.
Nitrous oxide	36°·1 C.

In the case of the mixtures, the very interesting result is obtained that the critical temperature is in some cases less than that of either of the constituent gases. Thus a mixture containing 10 per cent. of C_2H_6 has a critical temperature of 32°, the same critical temperature as for pure ethane. All mixtures containing more than 10 per cent. of ethane have a lower critical temperature than 32°; the lowest critical temperature obtained is 25°·8, and belongs to a mixture containing equal volumes of ethane and nitrous oxide. Another important point is that the border curves do not all lie between the vapour pressure curves of ethane and nitrous oxide. Hence for any temperature there is some mixture which gives a maximum vapour pressure. It also appears from the curves, given in the paper that the maximum vapour pressure is obtained with almost the same mixture at all temperatures, and that this maximum vapour pressure does not disappear with increase of temperature, but remains even up to the critical region. For mixtures containing between 20 and 50 per cent. of C_2H_6 retrograde condensation of the second kind takes place, but the author has not been able to observe it, since the difference between T_P and T_R for the two substances experimented on cannot be more than 0°·1, and the temperature could not be maintained sufficiently constant to hope to be able to detect any phenomenon taking place over such a small temperature range. The author showed his arrangement for stirring the liquid and vapour in the experimental tube so as to prevent any retardation of the different phases due to slow diffusion in the long narrow tubes employed. A small piece of iron with enamel beads on the ends is enclosed in the experimental tube, and by means of a small magnetising coil which surrounds the jacket used to keep the temperature of the tube constant, this piece of iron can be moved up and down the tube so as to keep the liquid and vapour thoroughly stirred. Prof. Carey Foster and Prof. Ramsay complimented the author on the very lucid way he had expounded a by no means easy subject. Dr. Sidney Young congratulated the author on the able use he had made of his lucky discovery of two bodies such that their mixture should have a lower critical temperature than that of either of the pure substances. Prof. Ramsay and he (Dr. Young) had made experiments on the vapour pressure of mixtures of alcohol and ether, and had found great difficulty in pre-

venting the separation of the components when the volume was altered, and he could, therefore, thoroughly appreciate the utility of the author's device for overcoming this difficulty. They had also experienced considerable difficulty in filling the tube with a mixture of known composition and free from air, and he considered that when dealing with mixtures it was better to employ gases, although they could not be obtained in so perfect a state of purity as liquids, on account of the greater ease with which a mixture of known composition can be obtained. The plan of making separate observations on the pure substances was a good one, and considering that the author measures the increase of pressure during the process of condensation, so that any air which happened to be present produced the maximum effect, the small rise in pressure obtained indicated a high degree of purity in the gases employed. He would like to ask the author if in the case of mixtures he found it possible to determine accurately the point where condensation commenced and ended, for with the alcohol and ether mixtures they had found it very difficult to determine these points. He also hoped the author would continue his observations in the direction indicated in the paper. Mr. Inwards suggested that in the case of liquids which act on iron, the iron stirrer could be enclosed in glass or india-rubber. It might also be possible to obtain more efficient stirring by means of a small fan or propeller worked by an electro-magnet rotating outside the tube. The author, in his reply, said that when the mixtures were well stirred, the pressures at which condensation commenced and ended were well marked.—Mr. Burstall commenced the reading of a paper on the measurement of a cyclically varying temperature. The experiments were undertaken with a view of measuring the temperature inside the cylinder of a gas engine at different points of the stroke of the piston. A modified form of platinum thermometer is employed to measure the temperature, and since the variations in temperature are extremely rapid, the wire had to be very thin and unprotected by any covering such as is ordinarily employed. The leads of the thermometer pass through a slate plug fixed in a seamless steel tube, asbestos being used as a packing to prevent leakage. The resistance of the thermometer is measured by means of a Wheatstone's bridge. Since the temperature at a certain part only of the *working* stroke had to be measured, the galvanometer circuit was broken in two places; one of these breaks was closed by means of a cam on the shaft of the engine at a given point of each revolution, while the other was closed when an explosion took place by means of a relay worked by the pointer of a steam engine indicator attached to the cylinder of the engine. The remainder of the paper was postponed till the next meeting.

Linnean Society, May 24. Anniversary Meeting.—Mr. C. B. Clarke, President, in the chair.—The Treasurer presented his annual report, duly audited, and the Secretary having announced the elections and deaths during the past twelve months, the usual ballot took place for new members of Council. The following were elected:—Prof. J. B. Farmer, Mr. A. Gepp, Prof. Howes, Dr. St. G. Mivart, and Mr. A. S. Woodward. On a ballot taking place for the election of President and officers, Mr. Charles Baron Clarke was re-elected President, Mr. Frank Crisp Treasurer, Mr. B. D. Jackson Botanical Secretary, and Prof. G. B. Howes Zoological Secretary. The Librarian's report having been read, and certain formal business disposed of, the President delivered his annual address, prefaced by some remarks on the present position of the Society. On the motion of Sir Joseph Hooker, seconded by Dr. John Anderson, a vote of thanks was accorded to the President, with a request that he would allow his address to be printed. The Society's gold medal was then formally awarded to Prof. Ferdinand Cohn, of Breslau, and was received on his behalf by Mr. B. D. Jackson for transmission through the German embassy. The President having called attention to the retirement of the Zoological Secretary, Mr. W. Percy Sladen, after holding office for ten years, an announcement which he felt sure would be received with universal regret, it was proposed by Mr. Carruthers, seconded by Mr. Crisp, and supported by Mr. Charles Breese—"That the Fellows of this Society, regretting the retirement of Mr. Walter Percy Sladen from the post of Zoological Secretary, which he has occupied for the past ten years, desire to record upon the Minutes of the Society an expression of their high appreciation of the services which he has rendered to the Society, and of the very able manner in which he has at all times discharged the duties of his office." This resolution having been put, was carried unanimously, and after a sympathetic reply from Mr. Sladen, the Society adjourned to June 6. In the evening a number of Fellows of the Society dined together at the Grand Hotel, Charing Cross, the President occupying the chair, and being supported by several distinguished visitors.

Royal Meteorological Society, May 15.—Mr. R. Inwards, President, in the chair.—Mr. G. J. Symons, F.R.S., and Mr. G. Chatterton read a paper on the November floods of 1894 in the Thames Valley, which they had prepared at the request of the Council of the Royal Meteorological Society. This consisted of a systematic description of the causes which led to the great floods of November last, and an analysis of the records obtained from the Thames Conservancy Board, from the engineers of several of the towns along the river, and also from rainfall observers throughout the Thames watershed. The information was given chiefly in the form of tables, one of the first being a chronological history of floods in the Thames Valley from the year 9 A.D. down to the present time. This was followed by a short description of the damage wrought in November 1894, which was illustrated by a number of interesting lantern slides. Details were then given of the levels reached at various places in all the principal floods from 1750 to the present time. The authors exhibited a map showing the relative elevation of all the parts of the Thames basin, and then gave details of the rainfall for each day from October 23 to November 18, 1894. The results obtained by the Thames Conservancy Board, showing the flood levels at each lock, were exhibited on a longitudinal section from Lechlade to Teddington, and the hydraulic inclinations from lock to lock were shown in a tabular form. The volume of flood water, as gauged by the Thames Conservancy at Teddington, rose rapidly from 4000 million gallons per diem on November 12, to 10,250 million gallons on the 16th, 12,800 million gallons on the 17th, and to over 20,000 million gallons on the 18th, when the discharge reached its maximum. The last-named discharge is equivalent to 0·37 inch over the whole watershed of the Thames above Teddington Lock.—Mr. F. J. Brodie read a short paper on the barometrical changes preceding and accompanying the heavy rainfall of November 1894, from which it appeared that the latter half of October was characterised by unusually bad weather, especially in the more western and southern parts of the British Isles. The torrential rains from November 11 to 14, which actually caused the floods, were due to two secondary depressions which developed a certain amount of intensity as they passed over the southern part of England.

CAMBRIDGE.

Philosophical Society, May 13.—Prof. J. J. Thomson, President, in the chair.—Exhibition of some recent photographs of the moon, by Mr. Newall.—On the "volume heat" of aniline, by Mr. E. H. Griffiths. The results of an inquiry (by what may be termed an absolute method) into the influence of temperature on the capacity for heat of aniline were published in the *Philosophical Magazine*, January 1895. During last autumn, Mr. C. Green, of Sydney College, made a series of observations on the *density* of the same compound, over the temperature range 15° to 52° C. Three separate sets of determinations of the density gave very concordant results. If the capacity for heat of equal volumes at different temperatures be denoted by the phrase "*volume heat*," then the "volume heat" at any temperature is the product of the capacity for heat and the density. In the case of aniline, the "volume heat" appears to be constant. Our knowledge of the changes in the capacity for heat of water due to changes of temperature is so uncertain that the *relative* values of the changes in the specific heat of other substances are of little absolute value. The author, therefore, has been unable to extend the inquiry into the "volume heat" of other bodies than aniline, for he has not succeeded in finding any other determinations which do not rest on some assumption as to the behaviour of water.—Exhibition of Goldstein's experiments on kathode rays, by Mr. J. W. Capstick. Mr. Capstick showed Goldstein's experiments on the effect of a stream of kathode rays on salts of the alkalies. When the rays are directed on potassium chloride, for instance, the salt becomes of a heliotrope colour, and retains the colour for several days if kept out of contact with moisture. The effect appears to be due to a chemical change in the substance—probably the formation of a sub-chloride—but the layer of altered salt is so exceedingly thin that it is difficult to get unequivocal chemical evidence as to its nature.—On a curious dynamical property of celts, by Mr. G. T. Walker. Mr. G. T. Walker exhibited celts which

possessed the property of spinning in only one direction upon a horizontal surface. On the formation of cloud in the absence of dust, by Mr. C. T. R. Wilson. The cloud-formation is brought about, as in the experiments of Aitken and others, by the sudden expansion of saturated air. A form of apparatus is used in which a very sudden and definite increase in volume is produced, and in which the possibility of dust entering from the outside seems to be excluded. If ordinary air is started with, it is found that after a comparatively small number of expansions, to remove dust particles by causing condensation to take place on them, there is no further condensation unless the expansion exceeds a certain definite amount. With expansion greater than this critical value condensation invariably takes place, and the critical expansion shows no tendency to rise, however many expansions be made. The latest result for the ratio of the final to the initial volume, when the critical expansion is just reached, is 1·258 (when initial temperature = 16°·7). This corresponds to a fall of temperature of 26° C., and a vapour pressure 4·5 times the saturation pressure for a plane surface of water. The radius of a water drop just in equilibrium with this degree of supersaturation = $6·5 \times 10^{-8}$ cm., assuming the ordinary value of the surface tension to hold for drops of that size.

May 27.—Evaluation of an automorphic function, by Mr. H. F. Baker.—On a construction in geometrical optics, by Mr. J. Larmor. Note on the steady motion of a viscous incompressible fluid, by Mr. J. Brill.

PARIS.

Academy of Sciences, May 27.—M. Cornu in the chair.—On an algebraical problem connected with Fermat's last theorem, by M. de Jonquières. A contribution to the history of the cerium earths, by M. P. Schutzenberger.—On the accumulation in the soil of cupric compounds used in the treatment of parasitic diseases in plants, by M. Aimé Girard. The evidence furnished by the author, in addition to the facts made known by other writers, completely proves that continuous treatment with copper compounds for a long period has no influence either upon the quantity or the quality of the crop obtained from the vine or potato.—Dr. Frankland was elected Foreign Associate of the Academy. Injection of ethyl alcohol into venous blood, by M. N. Grehant. From experiments made on a dog, it is concluded that, after the injection into the blood of a considerable volume of alcohol, the proportion of this substance in the blood five minutes after the injection and for more than eight hours afterwards becomes absolutely constant.—Spectroscopic researches on Saturn's rings, by M. H. Deslandres. The rotation of the planet and of its inner and outer rings has been measured by the methods used first by the author with the planet Jupiter, and employed by Keeler in his recently published researches on the subject of this paper. The author differs from Keeler insomuch as he does not regard this kind of evidence as a proof of the meteoric nature of the rings.—On the reduction of nitric oxide by iron or zinc in presence of water, by MM. Paul Sabatier and J. B. Senderens. The reduction of gaseous nitric oxide or nitric oxide dissolved in ferrous sulphate solution results in the production of nitrous oxide and nitrogen, finally the nitrous oxide is completely reduced also. A small amount of ammonia is formed, and a considerable quantity of hydrogen liberated, when the reaction is permitted to go on for a considerable time.—On the reduction of silica by aluminium, by M. Vigouroux. Silicon obtained in the crystalline form by this process is described.—A study of some reactions of lead sulphide, by M. A. Lodin. Mr. James Hannay's conclusions concerning the hypothetical compound PbS_2O_2, and the part played by it in the metallurgy of lead, are controverted. It is found that lead sulphide fuses at 935°, but exerts a considerable vapour pressure at temperatures much lower; hence the explanation of the volatilisation of galena requires no new compound to be supposed to exist. The long admitted equations expressing the reactions taking place in the reverberatory furnace are completely verified by the author.—On certain phenolic derivatives, by M. A. Béhal.—On crystallised cinchonine, by M. Ferdinand Roques.—Transformation of an amide salt into an amido-acid. Pyruvic acid forms with aniline a condensation product, $CH_3 . C(NC_6H_5) . COOH$. Phenylglyoxylic acid, under the same conditions, forms the salt, $C_6H_5 . CO . COOH . NH_2 . C_6H_5$. On dissolving this in methyl alcohol, the condensation product, $C_6H_5 . C(NC_6H_5) . CO_2H$, separates out in the crystalline form in a few minutes in the cold.—On ozobenzene, by M. Adolphe Renard. By the action of ozone on benzene a white explosive substance is produced having the composition $C_6H_6O_6$.—On the fixation of iodine by potato-starch, by M. Gaston Rouvier.—On the elimination of magnesia in the urine of infants suffering from rickets, by M. Oechsner de Coninck.—On the employment of serum from animals immunised against tetanus, by M. L. Vaillard. The antitetanic serum is able to confer complete immunity for from two to six weeks, but if the tetanus has become established, inoculation is not able to prevent progress of the disease. The toxine in tetanus is perhaps the most active of the bacterial poisons, yet the antitoxine of the serum is even more active.—The relation between relief and the frequency and intensity of earthquakes of any region, by M. de Montessus.—Atmospheric and seismic perturbations of the month of May last and their connection with solar phenomena, by M. Ch. V. Zenger.

BOOKS AND SERIALS RECEIVED.

BOOKS.—On certain Phenomena belonging to the Close of the last Geological Period and on their Bearing upon the Tradition of the Flood: Dr. J. Prestwich (Macmillan).—Fallacies of Race Theories as applied to National Characteristics: W. D. Babington (Longmans).—A Junior Course of Practical Zoology: Prof. A. M. Marshall and Dr. C. H. Hurst, 4th edition (Smith, Elder).—Handbuch für Botanische Bestimmungsübungen: Dr. F. Niedenzu (Leipzig, Engelmann).—Cours Élémentaire d'Electricité: M. B. Brunhes (Paris, Gauthier-Villars).—Verlagskatalog von Wilhelm Engelmann in Leipzig bis ende des Jahres 1894 (Leipzig, Engelmann).—The Time Machine: H. G. Wells (Heinemann).—A Text-Book of Physiology: Dr. M. Foster, 6th edition, Part 2, comprising Book 2 (Macmillan).—The Lumleian Lectures on certain Points in the Ætiology of Disease, and the Harveian Oration: Dr. P. H. Pye-Smith (Churchill).—Meteorological Charts of the Red Sea (Eyre and Spottiswoode).

SERIALS.—Proceedings of the Royal Society of Edinburgh, Vol. xx. pp. 305-384 (Edinburgh).—National Review, June (Arnold).—Humanitarian, June (Hutchinson).—Natural Science, June (Rait).—Contemporary Review, June (Isbister).—Scribner's Magazine, June (Low).—Zeitschrift fur Physikalische Chemie, xvii. Band, 1 Heft (Leipzig, Engelmann).—Fortnightly Review, June (Chapman).—North American Fauna, No. 8 (Washington).—Proceedings of the American Philosophical Society, May 1893 (Philadelphia).—Ditto, July to December, 1894 (Philadelphia).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1894, Part 3 (Philadelphia).—Bulletin from the Laboratories of Natural History of the State University of Iowa, Vol. 3, No. 3 (Iowa).—Geographical Journal, June (Stanford).

CONTENTS.

THURSDAY, JUNE 13, 1895.

MASKELYNE'S CRYSTALLOGRAPHY.

Crystallography, a Treatise on the Morphology of Crystals. By N. Story-Maskelyne, M.A., F.R.S., Professor of Mineralogy, Oxford. 521 pp. and xii. pp., 398 figures, 8 plates, 8vo. (Clarendon Press, 1895.)

AFTER wandering in the desert for considerably more than forty years, the English student of crystallography is at length brought within sight of the promised land; it is true that guides have been offered to him in the interval, but they have spoken in strange tongues, and have sometimes been mere dust-clouds of unnecessary formulæ and notations, calculated rather to bewilder than to lead.

The long-expected treatise of Prof. Maskelyne will be found to fully justify the anticipations with which it has been awaited; those who desire to study crystals and crystallography are no longer confronted by the want of an authoritative handbook, and need no longer lose themselves among the works of foreign authors. The English books hitherto available are few in number. The remarkable "Treatise" and "Tract" of the late Prof. Miller established, in the most rigid manner, a mathematical basis for the science, and must always remain standard works—masterpieces of precision. These two books contain, in a few pages, all that is essential; but being condensed into a bald sequence of theorems, they appeal almost exclusively to the mathematician. Mr. Gurney's little introduction to the subject, and the text-book of the late Prof. G. H. Williams, are excellent stimulants to the beginner, but will not suffice for the more advanced student; the present work supplies most completely what was wanted.

It is easy to state what is required from the practical point of view in a text-book on the morphology of crystals: the learner desires to know what are the forms of crystals, and how they differ from other figures; he must be told how they are determined and described, and for educational purposes it is especially important that the geometrical relations should be established by simple methods of proof from intelligible principles.

All this the present volume satisfactorily accomplishes. A crystal is considered to be, for morphological purposes, a complex of planes which obey a simple geometrical law—that known as the law of rational indices, and the early part of the book is consequently devoted to the investigation of such a complex, and shows, further, how it is denoted and represented; this involves a series of propositions relating to axes and indices, to stereographic projection, and to the relations of zones. The idea of symmetry superimposed on such a geometrical complex is considered in the two following chapters, and the six systems, having thus been established, are considered in detail in chapter vii.

Although this treatise will certainly not prove attractive to readers who are totally unfamiliar with mathematical methods and conceptions, yet it succeeds in giving simple and elegant proofs (many of them new) of all the necessary theorems without introducing any advanced mathematics. At the same time the book is far from being a geometrical study. The eighth and ninth chapters, comprising more than one hundred pages, are devoted to the practical methods employed in the goniometrical measurement and calculation of angles, and to the manner in which crystals are depicted by projections and perspective drawings; further, each crystalline type is represented by copious examples from minerals and chemical products, and frequent references will be found to the bearing of certain physical investigations upon the points discussed. Such a complete treatment, for example, as is here given of the twinning of diamond, quartz, and felspar is infinitely more satisfactory than the meagre sketch usually found in text-books, whether of crystallography or mineralogy.

But the book contains far more than is indicated above: it is, at least so far as regards certain aspects of the subject, a really philosophic treatise, of which the originality and peculiar interest will be best appreciated by a reader who refers to the discussion of crystalloid symmetry contained in the fifth and sixth chapters. Here the nomenclature is to a large extent new, although some of the terms have become familiar in Mr. Gurney's little book, where they are mentioned as due to Prof. Maskelyne. Many of them are invaluable aids to precision: haplo- and diplo-hedral, meta- and anti-strophic, holo- and hemi-systematic, for example, are terms which avoid much circumlocution, introduce clear conceptions, and once used can scarcely be dispensed with.

The chapters dealing with symmetry must have been familiar to Prof. Maskelyne's pupils many years ago, at a time when the importance of this subject was by no means recognised; to him is undoubtedly due the credit of first in this country directing to crystal symmetry the consideration which it deserves, which, moreover, it failed to receive in the methods of Miller. In the present book symmetry is of cardinal importance; the systems are deduced from a discussion of the possible forms which may be assumed by the systematic triangle, *i.e.* the triangle formed by the intersection of a sphere with three adjacent planes of symmetry; the mero-symmetrical divisions of the systems are then considered as resulting from the possible "presence or absence of certain faces consequent upon the abeyance of the actual symmetrical character of planes which are otherwise potentially planes of symmetry"; in other words, the symmetry of the system is regarded as a complete type latent in the hemihedral and tetartohedral crystals, and exercising a symmetrical influence by virtue of the axes of symmetry, which are themselves the result of dormant planes of symmetry.

Now in recent years new methods of treating crystallography, also mainly from the point of view of symmetry, have been developed in other countries; to avoid criticising the present treatise in the light of the newer teaching, would be to shirk a responsibility obviously imposed upon a conscientious review.

One method frames a theory of crystal structure which shall accord with the observed homogeneity of crystals, finds in how many ways such structures may be symmetrical, and so deduces the systems; such is the course pursued in Mallard's magnificent treatise upon the basis of Bravais' theory of structure, and a similar method

might be based upon a more extended theory, such as that of Sohncke or that of Fedorow and Schonflies, and would lead to all known varieties of crystal symmetry. Such a deductive method is not, however, one which has ever commended itself to scientific teachers in this country, and it is not one which can be logically adopted in a book dealing solely with the morphology of crystals.

The second method is the one introduced by Gadolin; it inquires in how many ways a figure obeying the law of rational indices can be symmetrical according to the number and distribution of its planes and axes of symmetry, and it leads satisfactorily to all the known varieties of crystals. It was employed by Liebisch, and has been carried to its utmost extreme in the new edition of Groth's "Physikalische Krystallographie," where the systems are geometrically little more than artificial groups constructed by synthesis of the various types, the conception of mero-symmetry being completely abandoned. Prof. Maskelyne treats of planes before axes of symmetry, and regards the latter as begotten by the former; accordingly he is compelled to introduce the idea of mero-symmetry as a second empirical law, whereas the method of Gadolin requires the one law of rational indices alone. In the opinion of the present writer, Gadolin's is the most, indeed the only, logical process. It must, however, be confessed that the method of Prof. Maskelyne possesses a simplicity which is important from the educational point of view, and might alone be sufficient justification for its use; that he has considered and rejected other possible courses is clear from the discussion on p. 171, which leads to the following suggestive remark: "It is, however, evident that the whole treatment of crystallographic symmetry on the assumption of planes and axes of symmetry, actual or potential, represents a geometrical abstraction; an abstraction that needs for its development and due explanation a complete science of position applied to the molecular mass-centres."

In the preface it is stated that the greater part of the present treatise has long been in print; this being the case, the earlier part must inevitably be somewhat out of touch with recent discovery, and since there is no list of errata, statements which are not, like the geometrical propositions, unassailable, must be received with due caution. Thus milk-sugar is stated to be orthorhombic, it has recently been proved mono-symmetric; the whole of § 314 should now be cancelled. Again, § 140 must be read in the light of § 266. Cuprite is described both as holo-symmetrical and as hemi-symmetrical; but the intelligent reader will find the most important of such contradictions implicitly corrected in a table of crystalline types, with authentic examples, given on p. 502. This table is introductory to eight useful plates which deserve special attention, since they represent all the varieties of merohedra and their relations, and render the previous descriptions easily intelligible.

The appearance of this book is an interesting event in the history of crystallography. The volume stands as a striking and permanent record of the original manner in which this science has for many years been treated by the Oxford Professor in lectures, of which the substance is now for the first time made public. By those who have had the privilege of personal acquaintance with his teaching, it will be welcomed as the familiar echo of a style of exposition singularly adapted to kindle enthusiasm for an abstruse subject, and by the scientific public, as an authoritative treatise on a science of which the growing importance is continually becoming more fully recognised.

H. A. MIERS.

THE STUDY OF STEREOCHEMISTRY.

Stéréochimie. Exposé des théories de Le Bel et Van't Hoff. Par E. G. Monod, avec une préface de M. C. Friedel. (Paris: Gauthier-Villars et Fils, 1895.)

THIS is a small book of 162 pages which gives a clear account of the fundamental ideas upon which is founded the modern doctrine of chemistry in space, which sprang, as every one knows, out of Pasteur's classical researches on the relation between optical activity and crystalline form. Much fault need not be found with this book because it contains rather dogmatic statements of debatable propositions, but we venture to think the treatment of the subject too sketchy and superficial to afford much real help to the student.

M. Monod's little book relates only to the stereochemistry of carbon, and the isomerism of nitrogen compounds is not referred to. Now this department of theoretical chemistry is one which should be entered by the student at a comparatively advanced stage of his progress, when he is already familiar with the more important facts upon which the theory is based. It seems doubtful, therefore, whether so scanty an outline as this will supply what is wanted by students at this stage. They will desire to be told not only that a certain number of groupings are possible with a stated number of carbon atoms, which is usually obvious enough, but they will require to be told something of the secondary hypotheses with which the fundamental idea has become encrusted. For example, the union between two carbon atoms joined by a single bond is shown (p. 17) to be "mobile," that is, each carbon is supposed to be able to rotate, together with its attached radicles, round the axis joining the two carbons; but the student is left at that point to wonder why it should rotate at all. It is only much later (p. 63), in connection with the isomerism of fumaric and maleic acids, that reference is made to the doctrine of attractions between the radicles associated with carbon atoms adjacent to each other. In this case it is not justifiable to say that the attraction of CO_2H for H is *evidently* greater than that of CO_2H for CO_2H or H for H. There is nothing *evident* about the statement, which is almost purely hypothetical, such evidence as does exist tending almost as much one way as the other.

Throughout the book the conventional tetrahedron is the symbol used, and we have not been able to find any account of Wunderlich's hypothesis as to the configuration and union of carbon atoms, nor of Baeyer's strain theory in the formation of closed chains, nor of any other explanation of the way in which two carbons may unite by double or triple bonds, and the consequences of such union.

The most interesting part of the book is the brief fourth section, which relates to the researches and hypotheses of Guye as to the relation between the rotatory power of the substance and the masses of the radicles attached to an asymmetric carbon in the molecule of an optically active compound.

One word more. The short preface by Prof. Friedel explains, as follows, the object of the book: "La branche de la science chimique à laquelle on a donné le nom de stéréochimie ou chimie dans l'espace est de date toute récente. Elle a été créé par MM. A. Le Bel et Van't Hoff: . . . A l'étranger les publications d'ensemble faites pour répandre ces notions ne manquent pas. Il n'en est pas de même en France," &c. This seems strange, while close by, rue S. André des Arts, may be had Meyerhoffer's edition of Van't Hoff's celebrated "Dix années dans l'histoire d'une théorie," a book of infinitely greater interest than the volume before us.

A practice has grown up of late years of inserting into text-books by obscure authors little prefaces by better-known men, containing nothing in particular in the way of information, and in which the laudatory expressions are not always quite justified by the character of the book. So long as "puffing" is regarded as allowable, there is no very clear reason why it should not be permitted in connection with books; but the sort of preface referred to, has rather too strong a family likeness to the "certificate" so often found on the label of hair-restorers and packets of cocoa, to the virtues of which these writers of prefaces would probably in most cases shrink from testifying.

OUR BOOK SHELF.

The Telephone Systems of the Continent of Europe. By A. R. Bennett. (London: Longmans, 1895.)

With what object was this book written? The introduction is a violent diatribe against the telephone powers that be in England; and yet by his titles, the author seems to have been nursed in their service. Moreover, England and Germany with their 162,000 telephones, rank next to the United States, and possess more telephones than all the rest of Europe put together. In fact, next to Scandinavia and Switzerland, England ranks above Germany in telephonic development—the rest of Europe being "nowhere." Why, therefore, this wailing and gnashing of teeth? Why should England and Germany alone in Europe excite his wrath? Is it that they will not adopt at home his views of low rates and, perhaps, no profits, and did his apparent rough treatment in Berlin prejudice his judgment of German ways? The book is full of statistics of the growth and development of the business in different European countries—except England. It indicates the public uses to which telephones can be applied, but it contains little that is scientific or practical. Its facts are fleeting, and its *raison d'être* is not evident.

The development of telephony in Sweden is very remarkable. The difficulty of locomotion, and the long dark days in winter, may account for much of it. In a population of 4,824,000 there are 26,201 telephones in use. This means one telephone to 184 inhabitants. In the United States there is one telephone to 270 inhabitants.

In Switzerland it is even more developed than in Sweden. The difficulties of locomotion and internal communication, the isolation of valleys, that gold mine to the country—the great summer tourist traffic—and hotel life, may account for this, but the author attributes its success solely to its cheapness. In fact it is too cheap, for it does not pay, and this state of things is not conducive to future prosperity.

The great development of telephony in the United States, where there are 232,140 subscribers in spite of very high rates, does not support the views of the author.

The annual charge in Switzerland was originally 150 francs per annum for an unlimited local service, and an additional 25 francs per annum to cover trunk or inter-urban service. It was soon found necessary to charge 20 centimes per talk of five minutes on trunk lines. Since 1890 the local charges have been 80 francs per annum with 800 free talks, and 5 centimes per extra talk, and the trunk charges per three minutes, 30 centimes for any distance up to 50 kil., 50c. to 100 kil., and above 100 kil. 75c. From January 1, 1896, it will be a very practical and sensible tariff, viz. an initial annual charge of 40 francs and a uniform charge of 5 centimes for all local talks, the trunk charges remaining unchanged. The number of talks per annnm per subscriber during 1894 was—local 504 and trunk 85, but the trunk traffic in many places far exceeds the local. In Affoltern, for instance, during 1894, there were only 105 local talks, while the trunk talks amounted to 8167 (*Journal Télégraphique*, May 25, 1895). There were at the end of 1894, 18,814 subscribers in Switzerland. This means one telephone to 147 inhabitants.

A word is wanted badly to express a telephonic conversation or talk analogous to "telegram." The author's "telephonogram" is lengthy. "Phonogram" is in use in connection with the phonograph. "Telelogue" has been proposed, but has not met with general approval

The Elements of Health. By Louis C. Parkes, M.D., D.P.H. (London: J. and A. Churchill, 1895.)

The author of this manual states in the preface that his "main idea has been to give some simple yet practical information on the preservation of individual or personal health." It is impossible to say, with any degree of certainty, who is to be accorded the distinction of having originated such an "idea." Certainly Hippocrates undertook the writing of treatises on hygiene, and even he was only following in the footsteps of others. This preliminary remark mainly arises out of the fact that when another manual of hygiene appears, one's natural impulse is to turn to the preface, in order to see if the author has any new motive to suggest for its appearance; for the fact is, there is, at present, a superabundance of such works. Dr. Parkes' manual, good as it is, contains practically nothing that cannot be found in any of the other dozen or more elementary treatises dealing with the same subject; and to those who are familiar with the same author's work upon "Hygiene and Public Health," it will be sufficient to state that the present volume under review is practically that work popularised and very much abridged.

The illustrations are excellent; and it is a positive relief to find that they show a little freshness in their treatment, and are something more than the stock figures that appear in so many similar publications.

Dr. Parkes occupies a deservedly high position among sanitarians, and it goes without saying that his teachings are sound. There are only two points which call for adverse criticism. The table on page 168 needs revision: the author is well aware that the fat in butter does not average 88 per cent.; indeed, on a subsequent page (196) he himself puts it down at 83 per cent.; and his statement that it is "doubtful if alum (in bread), unless present in considerable quantity, is able to influence health adversely," is also open to criticism. In the first place, it is doubtful whether, if such be the case, it is prudent to make so loose a statement in what is designed to be a popular work for the lay reader. There is little doubt that the hydrate of alumina, which results from the use of baking powders containing alum, is soluble in the hydrochloric acid of the gastric juice, and there are many good reasons for regarding such addition as very undesirable; it would, moreover, probably prove harmful when present in what may be held to constitute less than a *considerable quantity*.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Hypnotised Lizards.

SEVERAL communications relating to the so-called "death-feigning instinct" of certain reptiles have appeared in the columns of NATURE during the last few months. The following observations bearing on this question may be of sufficient interest to justify publication. They refer to a species of lizard of the genus Stellio (identified in Tristram's "Fauna and Flora of Palestine" as *S. cordylina*), which is extremely common in these parts. When one of these lizards is captured, it makes a few vigorous efforts to escape, and then, if held firmly, falls into a limp, motionless state, which might easily lead an inexperienced person to think it dead. A very little examination, however, shows that the animal is not dead, but in a trance-like condition. Gentle respiratory movements are visible just behind the shoulders, and sometimes show a rising and falling rhythm with short intervals of complete rest; the eyes may remain wide open, but are commonly half-closed, and the lids wink slowly from time to time spontaneously or by reflex action; the mouth is almost always open—sometimes wide, sometimes but little—and in either case the jaw is quite rigid, and if closed by force is apt to reopen when the pressure is withdrawn; the limbs lie extended and semi-flaccid, with some approach to a cataleptic condition, *i.e.* if bent, or stretched into positions not too strained, they maintain such positions when let go; and the same is true of the trunk and tail. If, now, the lizard be laid down gently on the floor or on a table, it will lie perfectly still and seemingly unconscious for some minutes (unless roused by a sudden jar or loud noise), the eyes preserving throughout a peculiarly vacant, expressionless aspect, quite suggestive of death. While in this state the lizard may be put into a variety of positions without eliciting any sign of consciousness, and will lie as quietly on its back as in the natural position; and I have without difficulty made one maintain various grotesque postures, such as standing erect with one hand resting on the edge of a book, like a preacher behind a pulpit; bending sharply around, and seizing the tail with the claws of one fore-foot; cocking the tail over the back, scorpion fashion, &c.

Although some reflex actions are maintained (*e.g.* winking, as above mentioned), there is a considerable degree of cutaneous anæsthesia, as shown by the fact that a pin may be run through a fold of skin without fully rousing the animal, a sluggish, feeble wriggle being the sole result.

This trance state (obviously akin to some phases of hypnotism) lasts, as before stated, for several minutes. I have on several occasions timed it, the lizard being laid on its back, and myself concealed or standing quite still at a distance, and in each instance recovery seemed to come suddenly after about five minutes (sometimes a few seconds less, sometimes more), the animal showing no sign of consciousness until by one brusque effort it turned over into the normal position; this done, it lies quite still, but evidently awake and observant, for a few moments more, and then scuttles off in a hurry.

I find that the readiest way of inducing the trance is to take the lizard's head between my finger and thumb, making gentle pressure upon the angles of the jaw and upon the tympanic membranes; but similar pressure on the sides of the trunk, just behind the fore limbs, is just about as effective.

Such are the facts: and it seems to me that, so far as the animal in question is concerned, they lend no support whatever to the hypothesis of voluntary or conscious death-feigning; but, on the contrary, are perfectly consistent with the view that such phenomena belong to the same class as the various manifestations of hypnotism, &c., with which we are all more or less familiar in the human subject.

Supposing, however, for the sake of argument, that we have to do here with a true instinct, and not, as I believe, with a mere neurosis—an incidental reaction of the higher nervous centres—what possible purpose could such an instinct serve? The natural enemies of these lizards are foxes, jackals, martens, birds of prey, and snakes. Can any one believe that any one of these animals, having captured a lizard, would be in the least inclined to let it go because it lay motionless and apparently dead in the captor's grasp? Or will it be argued that the trance condition is a special gift "in mercy" to the victim, to mitigate or abolish the pain of death? If the last be the true explanation, one is tempted to ask why such tenderness is shown to a favoured few of the victims in nature's wondrous system, while the majority (*pace* Dr. A. R. Wallace) are left in possession of consciousness and sensibility more or less acute until they have sustained enough mechanical injury to kill or stun them.

W. T. VAN DYCK.

Beyrout, Syria, May 16.

Stridulating Organ in a Spider.

IT is exactly twenty years now since I described to Geoffry Nevill the sound made by our large "Bhaluk Mokra" (or Bear Spider). I noticed that Wood Mason, who sat opposite me, appeared to be highly amused, but he said nothing.

Next morning when he joined Nevill and me at table, Mason was in high glee, and said, "I've found out all about your wonderful spider. I thought yesterday you were telling Nevill a stiff yarn for amusement, but as it wasn't your usual custom, I unbottled a lot of the big spiders, and found the stridulating apparatus."

He there and then made me recite all over again, and promise to write out, what he quoted in the *Trans. Ent. Soc.*, 1877, and give him a sketch, which is plate vii.: a previous notice of it all appearing in our *Proc. As. Soc.*, Bengal, 1876, and *Ann. and Mag. Nat. Hist.*

It was in the cold season of 1869–70 that I captured the specimen, and noticed the stridulating phenomena. The sound can be heard easily at ten or twelve yards, and is like pouring small shot on a plate.

I should not have mentioned the above, were it not that my report of "sound-producing Ants" seems to have been overlooked. If I mistake not, Sir John Lubbock looks on them as a silent group; but it is ten or twelve years now since I drew attention to the sounds made, and gave a small "Morse" diagram of the same, either in NATURE or the *English Mechanic*, one kind of ant giving a series of triple sounds, another kind a set of five or six, gradually decreasing.

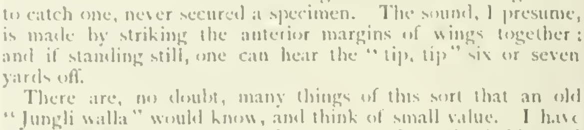
I described how the sounds were made by rasping the horny tip of the last abdominal segment on any resonant material, such as thin dry bark, dry leaves, &c.

I am not aware if the tolerably loud percussive "tok-tok" of the Mahsir (*BarbessMacrocep*) is known. I described it to a friend in England in 1879, and saw it quoted in the *Daily Telegraph* (about August to October) soon after.

While on this subject, I may mention that we have a rather rare butterfly here, which is dark in colour, some three inches across, a very hard flyer, and when darting about (generally after sunset), in a shady avenue, makes a series of taps, sounding like "tip, tip, tip."

Three or four of these butterflies generally fly together. I have not seen one alone; and though I have often enough tried to catch one, never secured a specimen. The sound, I presume, is made by striking the anterior margins of wings together; and if standing still, one can hear the "tip, tip" six or seven yards off.

There are, no doubt, many things of this sort that an old "Jungli walla" would know, and think of small value. I have been surprised at the little often known about the habits and appearance of many animals and insects. Not three years ago,



a well-known naturalist was quite interested in my description of the "happy family" one often finds in the holes, a little above water level, in our clay banks of small rivers, at low water during cold season; fish of several kinds, and crabs (three and four inches across) living together in the hole under water as a "colony." But for these tolerably deep holes, the otters would leave no fish in the smaller rivers.

S. E. PEAL.

Sibsagar, Asam, May 9.

The Migrations of the Lemmings.

Although I have dwelt among the Lemmings for many years, and paid great attention to their migrations, I have thought it might be more satisfactory to my readers to record the result of an interview with a captive member of the tribe, as recorded by the aid of a phonograph, assisted by a certain legitimate amount of amplification which the poverty of the language necessitates. This, however, I am convinced is what my little prisoner intended to say. "I am amused by the reasons men give for our sudden appearances and inexplicable migrations. But, although I do not see why I should enlighten you on either of these points, especially as you would probably only stick the harder to your own opinion, I will venture to ask whether you think we cross wide lakes, the opposite shores of which are quite invisible to us, in order to find the food which we thus abandon; indeed, though I fear I am somewhat letting what you call the cat out of the bag by saying it, I have often wondered why I myself did not wander along the green shores of Heimdalsvand and down the valley amid sweet grasses and clover, instead of swimming across to barren Valders, and getting caught by you for my pains. But, after all, it is no worse than when my friends the swallows leave their flies, and even their families, and start on their travels, when the impulse seizes them, whilst the former are still plentiful, and the latter not yet ungrateful. So I feel indignant at the suggestion that we travel because we are overcrowded and underfed at home. I admit that our temper as a race is somewhat short; it has been impaired by incessant bullying. Dogs, wolves, and lynxes eat us wholesale; and the reindeer disgustingly declare that we are a mere bag of succulent *saur-kraut*. Shadows annoy us, and you men have even invoked spiritual weapons to aid your carnal implements of destruction. But let me seriously advise you not to fling about inappropriate epithets; our customs are at least as good as your own, and probably somewhat older, for we too have had an ancestry, and *noblesse oblige*. Enough; let me out; I want to get on." W. Duppa-Crotch.

Richmond, Surrey.

Boltzmann's Minimum Theorem.

There is a point of great interest, in connection with Mr. Burbury's letter in your issue of May 30, on which he has not touched.

The expression obtained in the Boltzmann theorem for the value of $\frac{dH}{dt}$ depends on the assumption that the actual distribution is at every instant absolutely identical with the most probable distribution. This we know cannot be exactly true. Therefore the value of $\frac{dH}{dt}$ in Boltzmann's theorem is not identified with the *most probable* value of $\frac{dH}{dt}$. It is, for instance, quite possible, in the absence of proof to the contrary, that no matter in what way the actual distribution differs from the most probable one, the actual $\frac{dH}{dt}$ may be numerically smaller than the value corresponding to the most probable distribution.

In that case Boltzmann's theorem would give the maximum rate of subsidence instead of the most probable rate. Can Mr. Burbury or Dr. Boltzmann throw any light on this question?

Edwd. P. Culverwell.

Trinity College, Dublin, June 1.

THE CAMBRIDGE NATURAL HISTORY.[1]

Although the third in the series, this volume is the first of the long-promised "Cambridge Natural History" to appear, and as such excites additional interest because it affords some clue to the probable style of the remainder—probable, since "complete uniformity of treatment has not," we are told, "been aimed at. It is worthy of remark that, contrary to what obtains in most popular works on natural history, the Invertebrates are to receive their fair share of attention, and to extend over nearly seven of the ten volumes projected. It is almost a Cambridge work in a double sense, for with the exception of Prof. Herdman, who is to write on the "Ascidians and *Amphioxus*," and Mr. F. E. Beddard, who will undertake two such widely separated subjects as "Earthworms and Leeches" and "Mammals," all the contributors are connected with that University.

"'The Cambridge Natural History' is intended," the publishers announce, "in the first instance for those who have not had any special scientific training, and who are not necessarily acquainted with scientific language. At the same time an attempt is made not only to combine popular treatment with the latest results of modern scientific research, but to make the volumes useful to those who may be regarded as serious students in the various subjects. Certain parts have the character of a work of reference."

By this standard, then, the present volume must be judged; and on opening its leaves and turning over its pages, with their abundance of new and beautiful illustrations, it is at once manifest that artist and engraver, printer and publisher, have vied with each other to produce a work worthy of the conception.

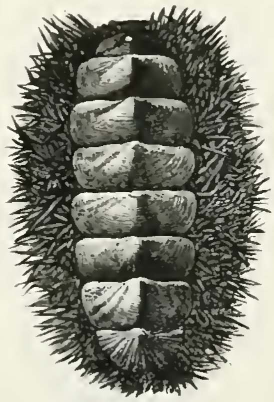


Fig. 1.—*Chiton Spinosus*, Brug.

The major portion, or, to be precise, 459 pages of the whole, is devoted to the Mollusca. It is no fault of the author's if it has to be admitted that a treatise on this branch of natural history, at once popular and scientific, still remains to be written. Mr. Cooke, who is responsible for this section, save for a casual passage or phrase here and there, has produced a most readable work: but the burden laid on his shoulders is greater than one man can bear nowadays, for no single individual can be a specialist in all the numerous branches of the subject; and yet nothing short of special knowledge in every ramification is adequate for the production of a text-book. The co-operation of specialists is yearly becoming more and more of a necessity in compiling manuals if good work is to be achieved, and in our opinion the system of minute subdivision, adopted for example in the "Standard Natural History," which was published some years ago in America, is the only wise one.

It is not, therefore, any matter for wonder that Mr. Cooke has had to resort largely to compilation, with the inevitable result that facts here presented in one form of phraseology, would, with a more intimate personal knowledge, have been differently expressed. Thus, for example, when speaking of barriers to distribution, we are told that "ranges of inferior altitude, such as the Pyrenees, the Carpathians, and the Alleghanies, may be turned in flank as well as scaled," and when he wrote, "The Mediterranean offers no effectual barrier" the author evidently did not take into consideration the altered distribution of sea and land in the Mediterranean region during Pleistocene

[1] "The Cambridge Natural History." Edited by S. F. Harmer, M.A., and A. E. Shipley, M.A. Vol. iii. "Molluscs." By the Rev. A. H. Cooke, M.A. "Brachiopods" (Recent). By A. E. Shipley, M.A. "Brachiopods" (Fossil). By F. R. C. Reed, M.A. Pp. xii. 535; 334 Figures in text, and 3 Maps. 8vo. (London: Macmillan and Co., 1895.)

times. Whilst in this respect the work, for a text-book, suffers unavoidably from too much of the "study," it, on the other hand, would have been better if an extension of time had been allowed the author in which to weld his mass of interesting and valuable material into a more homogeneous whole.

The method of treatment of the subject, differing as it does in many respects from that of the ordinary handbook, will best be gathered from a brief recapitulation of the order in which the main points are taken.

Prefixed is a scheme of the classification adopted; and concerning this it will be more convenient to speak later on. The opening pages are devoted to a brief introductory statement defining the relationship of the Mollusca to the rest of the animal kingdom, and sketching their classification so far as the principal groups are concerned. Only one phylogenetic table is submitted, and that, unfortunately, the misleading one dividing the Mollusca into the utterly unnatural groups of Glossophora and Aglossa. On the other hand, Mr. Cooke is cordially to be congratulated on refusing to have aught to do with that mythical monster, the "archi-" or "schematic-mollusc." A discussion on the origin of the land and fresh-water mollusca follows, and leads up to chapter ii., which deals with the habits and economy of the non-marine forms. Enemies of the mollusca, means of defence, parasitic mollusca, commensalism, and variation occupy the next chapter. Field malacologists especially will appreciate the bionomical facts and fancies here carefully gathered together from innumerable minor sources, and presented in available form; indeed, were it not from lack of space, we would gladly quote largely out of this, the most interesting portion of the work from a popular point of view. In the succeeding four chapters (v.–viii.), the anatomy, or rather the comparative anatomy, and embryology of the several classes are dealt with. The shell and the designation of its parts come next. Distribution (in space) forms the theme of the three subsequent chapters, and here the non-marine have preference by two to one over the marine mollusca. Three maps accompany and "illustrate" this section, by obviating the necessity of referring to an atlas. Finally, there is the systematic portion, in which a brief description is given of the principal characters of each family with its distribution in time, and a list of the more important genera composing it.

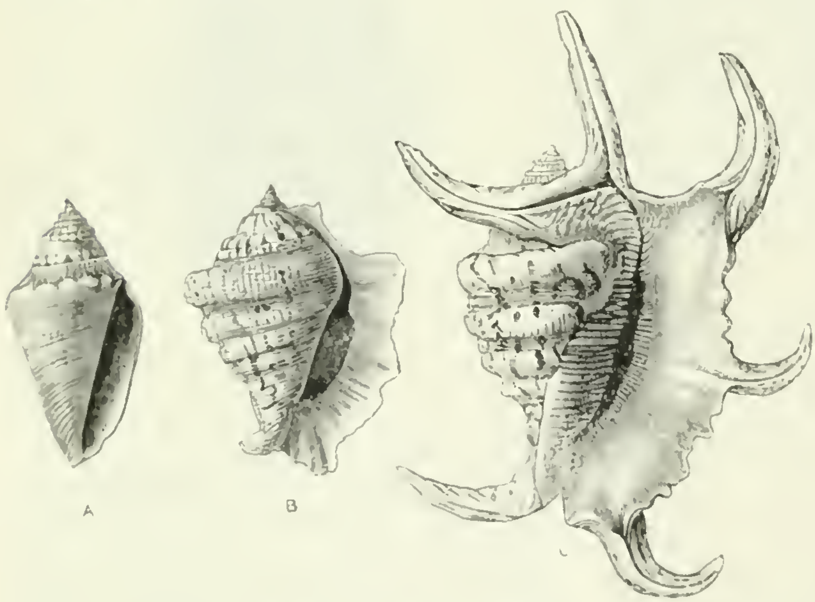


Fig. — Three stages in the growth of *Pteroceras rugosa*, Sow.

In great part, therefore, the present work reverses the method adopted in most modern text-books, wherein it is customary to describe the animals first and discuss their habits afterwards; the writer has, in fact, followed the arrangement adopted in the preliminary chapters of Woodward's "Manual," rather than that in Fischer's. This system of inversion also obtains in the anatomical portion; reproduction, usually reserved for the last, being put first apparently, with some idea of starting at the beginning of the molluscan career. The principle may undoubtedly possess advantages, but it also has its drawbacks. For example, the nomenclature of the parts, or topography, of the shell is not given till the close of chapter ix., whilst many of the terms there first defined have previously been freely employed, and that although the student is theoretically not expected to be acquainted with anything beforehand.

This is a detail which the editors should have attended to, for wherein their utility if not to assist by bringing a fresh and impartial eye to bear on the work they supervise, since however able a writer may be, he is naturally apt when engrossed with his task to overlook such minutiæ. So, too, they should have noted that the "classes" have in the opening pages, by a slip of the pen, been called "orders." They might also, though it is not fair to charge it to their account, have observed that whilst at p. 14 *Dreissensia* and *Mytilopsis* are spoken of as "scarcely modified *Mytili*," these two genera are in the systematic part correctly referred to a totally different order from *Mytilus*. The author himself, however, must be held responsible for having overlooked Dr. Carpenter's retraction of his theory of shell formation in the later editions of "The Microscope," and for such other oversights as referring the well-known and beautiful *Choanopoma hystrix*, from Cuba, to the genus *Cylindrella*, or describing *Strombus* as "frugivorous."

Although on so vexed a question as classification the greatest latitude seems allowable, yet certain points in the one here adopted call for remark. For instance, the Amphineura are retained as an order of Gastropoda (Mr. Cooke prefers the older and, we think, less correct spelling of Gasteropoda) in contradistinction to the opinion of recent authorities such as Pelseneer, Simroth, &c. Moreover, by-the-by, why is Pilsbry's classification of the Chitons passed over for an older and less complete one? What to do with the Pteropoda, Mr. Cooke was apparently in doubt when he began his book (pp. 6, 7), but in the systematic part at the close, their affinities with the Tectibranchiate Opisthobranchs is duly pointed out. At the same time, though their two main divisions, Thecosomata and Gymnosomata, are most closely allied to the Bulloidea and the Aplysioidea respectively, the Pteropoda are here for convenience sake retained as a group by themselves of equal rank with the Tectibranchiata as a whole. This, if not exactly logical, is comprehensible, but not so the separation of these two sections by the interposition of the Ascoglossa and Nudibranchiata.

About the Heteropoda, on the other hand, our author has no scruples, and though they retained their independence to a later date than the Pteropoda, they are referred without comment, albeit correctly, to the Prosobranchiata, and even, following Lang, to the Tænioglossa.

The term Platypoda, founded to include all the Pectinibranchiate Prosobranchs except the Heteropoda, is here restricted and made to apply, without reason given, to the Tænioglossa other than the Heteropoda.

Those interesting and somewhat anomalous genera *Siphonaria* and *Gadinia*, Mr. Cooke, in accordance with the conclusions lately arrived at by Köhler, Haller and Plate, places with the Tectibranchiata, creating for them the sectional name of Siphonarioidea. Pelseneer, we may incidentally remark, in his "Recherches sur divers Opisthobranches," which has only just been published, objects to this conclusion of his German *confrères*, and seemingly on very good grounds.

The Brachiopoda, which are incorporated at the end of the volume, are subdivided into "recent" and "fossil." The former (pp. 463-88) have been undertaken by one of the editors, Mr. A. E. Shipley; the latter (pp. 491-512), by Mr. F. R. Cowper Reed.

Mr. Shipley's chapter is a compact little summary, pithily written, and whilst not erring on the side of popularity, ought to be readily followed by any average student or reader.

It consists almost entirely of anatomical description, embryology, &c., for in "habits" the Brachiopoda are extremely deficient, preceded by a short sketch of the historical bibliography of the group, and followed by a few notes on their distribution, with a synopsis of their classification by Davidson.

Mr. Shipley concludes that the affinities of the Brachiopoda "seem to be perhaps more closely with the

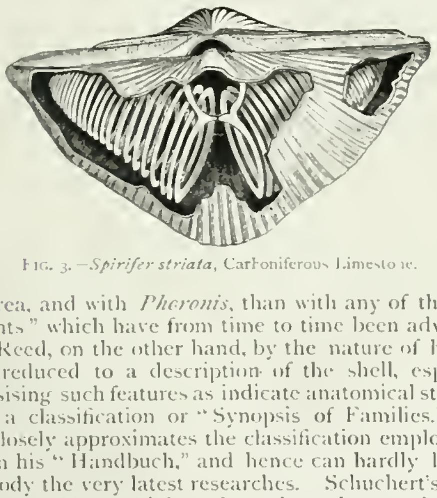


FIG. 3.—*Spirifer striata*, Carboniferous Limestone.

Gephyrea, and with *Phoronis*, than with any of the other claimants" which have from time to time been advanced.

Mr. Reed, on the other hand, by the nature of his subject, is reduced to a description of the shell, especially emphasising such features as indicate anatomical structure and to a classification or "Synopsis of Families." The latter closely approximates the classification employed by Zittel in his "Handbuch," and hence can hardly be said to embody the very latest researches. Schuchert's classification should, we think, at least, have been referred to. Some allusion, too, ought to have been made to *Trematobolus*, which its discoverer, Mr. G. F. Matthew, describes as possessing articulate valves, though it is allied to the Obolidæ. Mr. Reed's descriptive writing must be accorded equal praise with that of Mr. Shipley for clearness of style.

Through the kindness of the editors and the publishers, we are enabled to reproduce some of the illustrations in the text. These of themselves should serve to distinguish "The Cambridge Natural History" from most of its competitors for popular favour, with their plentiful reproduction of ancient blocks, now, alas, too familiar to the eye, and by no means always joys for ever.

NOTES.

THE Ladies' soirée of the Royal Society took place yesterday evening, at the time NATURE went to press.

AN unknown donor has given to the University of the City of New York, funds for a central building, on University Heights, for a library, museum, and hall, so arranged that all may be turned into a library capable of holding 1,000,000 volumes. The gift will amount to 250,000 dollars, being the largest ever received in the sixty-six years of the existence of the University. The only condition is that the name of the donor shall never be revealed.

ARRANGEMENTS are being made by the Marine Biological Association for a series of dredging and trawling expeditions during July, August, and September, to investigate the fauna and flora of the outlying grounds between the Eddystone Rocks and Start Point. In order to make the results as complete as possible it is extremely desirable that the investigation of each group should be carried out by a competent naturalist. Zoologists and botanists who are willing to take part in these expeditions, or to assist in working out the material collected, are requested to communicate with the Director, the Laboratory, Plymouth.

THE summer meeting of the Institution of Naval Architects was opened at Paris on Tuesday, when Lord Brassey delivered his presidential address, and several papers were read and discussed. In the afternoon the members of the Institution visited the Paris Observatory and the Arts et Metiers, and a banquet was given at the Hotel Continental in the evening. After the close of the meeting, we shall give a report of the proceedings.

THE annual meeting of the Société des Amis des Sciences was held at Paris last week. The Society was founded by Thénard in 1857, for the purpose of affording assistance to men of science or their families. It numbers more than two thousand members or subscribers, and since its foundation has distributed nearly £50,000 to deserving investigators. Grants are only made to persons who have had papers or memoirs presented to the Academy of Sciences, or who have published papers of equal merit to those approved by the Academy. The Society lays stress on the fact that the grants must not be regarded as charitable doles, but as rewards for services to science, and of a similar nature to the pensions which a grateful country gives to its servants. The awards are therefore publicly announced, and are looked upon as honours for meritorious work.

THE Committee of the American Public Health Association, appointed to determine the possibility of establishing co-operative investigation into the bacteriology of water supplies, have made arrangements for a conference of bacteriologists to be held on June 21 and 22, in the Academy of Medicine, New York city. The conference will consider how to obtain increased exactitude in the details of bacteriological research, and establish standard methods. The conference will, in fact, attempt to establish some common ground-plan for systematic work in bacteriology in general, and in the bacteriology of water supplies in particular. The Bacteriological Departments of many State and Provincial and Municipal Boards of Health will be represented at the conference, as also the principal universities of the United States and Canada.

IN the eyes of the law, the Royal Agricultural Society is not a scientific institution which can claim exemption from local rates. It was decided in the Queen's Bench Division on Tuesday, that, as the funds of the Society are not exclusively applied to the purposes of science, but are used to promote "the comfort and welfare of labourers," the Society does not come within the statute under which exemption from rates is claimed.

COLONEL J. WATERHOUSE has been elected President of the Photographic Society of India for the current year.

THE summer meeting of the Geological Society of America will take place at Springfield, Massachusetts, on August 27 and 28.

We learn from *Science* that nearly a thousand dollars have been subscribed in the United States towards the memorial to Helmholtz.

This year's conversazione of the Institution of Electrical Engineers will be held in the Galleries of the Royal Institute of Painters in Water Colours, on Wednesday, July 3.

The third International Congress of Physiology will be held at Berne, from September 9 to September 13. An exhibition of physiological apparatus will be held at the same time. Those who desire to become members, or to read papers, should communicate with Prof. H. Kronecker, Berne, before August 1. The subscription is ten francs.

Several clearly marked earthquake disturbances have been felt at Florence during the past week. A strong shock, followed by two slighter shocks, was felt there at 1.30 a.m. on Thursday last. The shocks have done no damage in Florence, nor, so far as can be learned, in the surrounding country. The earthquake was most violent at Pontassieve, Rignano, and San Casciano.

Sir Samuel Wilson, whose death is announced, was greatly interested in science and education. Among other generous acts, he presented £30,000 to Melbourne University in 1875. He was Vice-President of the Melbourne International Exhibition of 1880, and a Royal Commissioner for the Fisheries Exhibition.

Among the recent appointments abroad we notice the following: Dr. Celakovsky to be Professor of Pharmacology in the Bohemian University at Prague; Dr. Rohde to be assistant in the Zoological Institute at Breslau; Dr. F. Trendelenburg, Professor of Surgery in Bonn University, to succeed the late Prof. Thiersch at Leipzig; Prof. J. v. Kries to be the late Prof. Ludwig's successor at Leipzig; Dr. F. Schütt, of Kiel, to Greifswald University as Professor of Botany, and Director of the Botanical Gardens and Museum; Dr. v. Knorre to the new chair of Electro-chemistry in the Technical High School at Charlottenburg; Prof. A. Overbeck, of Greifswald, to be Professor of Physics in the University of Tubingen; Dr. Hermann Struve to be Professor of Astronomy in Konigsberg University; Prof. E. Koken to be Professor of Geology and Mineralogy in Tubingen; Prof. R. Brauns to be Professor of Geology and Mineralogy in Giessen; Dr. T. Smith to be Professor of Applied Zoology in Harvard University.

In all parts of the British Islands, and especially over England, the weather has continued persistently dry: in the neighbourhood of London the total fall during the first eleven days of June did not exceed half a tenth inch, and the aggregate fall since the beginning of May, a period of six weeks, was but little over half an inch. The *Weekly Weather Report* of the 8th inst. showed that the amount of rainfall since the beginning of the year was below the average in all districts, except the north-east of England. In the west of Scotland the deficiency amounted to 10·4 inches. High summer temperatures have occurred during the past week in many parts of the country, the shade readings having reached 84° in the east of Scotland, and 83° in the south of England. In London, readings of 80° were recorded both on Saturday and Sunday last.

The Whitsuntide party at the Port Erin Biological Station included the following naturalists:—Mr. F. W. Gamble (Owens College), Mr. W. I. Beaumont (Cambridge), Dr. H. O. Forbes (Liverpool Museum), Mr. A. Leicester (Southport), and Prof. R. Boyce, Mr. A. Scott and Prof. Herdman, from Liverpool. Dredging, tow netting, shore collecting, and laboratory work were carried on much as usual. Amongst the more noteworthy animals obtained were *Polygordius* sp., *Sarcobotrylloides* sp., *Embletonia pulchra*, *Elysia viridis*, and *Cynthia morus*. The tow nets contained some fish eggs, but fewer than at Easter. Diatoms and gelatinous Algæ were nearly absent: Copepoda and larval forms were present in great abundance. Prof. R. Boyce and Prof. Herdman have commenced an investigation on the effect of surrounding conditions upon oysters, and their connection with disease. A number of oysters have been laid down in different parts of Port Erin bay and on the shore, and others are being experimentally treated with various fluids and food matters in the aquarium. Mr. W. I. Beaumont stays on for some weeks at the laboratory studying the Nemertines of the district, and the Rev. T. S. Lea goes to Port Erin shortly to assist Prof. Herdman in working out the detailed "zoning" of the shore and the distribution of the littoral animals.

The general arrangements for the sixth International Geographical Congress, to be opened in London towards the end of next month, are made known in a circular just distributed. The Congress promises to be truly international, for delegates have been appointed to represent Governments and Geographical Societies in all parts of the world. The provisional programme of the meetings is as follows:—The Congress will be opened on Friday, July 26, at 9 p.m., when short addresses of welcome will be delivered by H.R.H. the Duke of York, Honorary President, and by Mr. Clements R. Markham, President. On the following day, Mr. Markham will deliver his inaugural address, after which the Congress will meet in two sections to discuss papers on geographical education, by Profs. Levasseur and Lehmann, and others; and on mathematical geography, especially the use of photography in surveying, by Colonel Laussedat, Colonel Tanner, and others. On Monday, July 29, a general meeting of the Congress will discuss the subject of Arctic and Antarctic exploration, introduced by Prof. Neumayer and Admiral A. H. Markham. In the afternoon two sections will be formed, in one of which questions in geodesy will be treated by General Walker and M. Lallemand, while in the other papers will be read by Prince Roland Bonaparte on glaciers, and M. Martel on speleology. On July 30, report of Committees and papers on the proposed map of the world on the scale of 1 : 1,000,000, and on international geographical bibliography, will be presented at the general meeting, and two sections will then deal with oceanography, and with the orthography of place names. On Wednesday, July 31, Sir John Kirk will initiate a discussion on Europeans in Africa in the general meeting, and in the afternoon the sections will consider applied geography (commercial geography) and limnology, the latter to be introduced by Prof. Forel. The general meeting on August 1 will deal with the terminology of land forms, and in the afternoon cartography and other subjects will be treated. On Friday, August 2, the forenoon will be devoted to papers by Baron Nordenskiold, Prof. Hermann Wagner, and others, on the history of maps; and all the remaining papers will be taken in the afternoon. On August 3 the votes proposed for consideration will probably be discussed, the date and place of meeting of the next Congress considered, and the President will deliver his concluding address. After the close of the Congress, a series of excursions will be organised to places of geological and geographical interest.

The Rev. O. Fisher contributes a short paper on the age of the earth to the *Geological Magazine* for June. Arguing in favour of a comparatively thin crust and a liquid substratum, he urges that the continual laving of the bottom of the crust by the molten rock will retard the cooling of the crust, and will produce an effect on the temperature-gradient at the surface similar to that to which Prof. Perry has recently drawn attention (Nature, vol. li. pp. 224–227). If this be the case, then, no trustworthy estimate of the earth's age, based on the present temperature-gradient at the surface, has yet been made.

Dr. M. Cinelli has recently compiled a valuable list of the records of the Vicentini microseismograph at Siena between

July 15 and October 31, 1894 (*Atti. R. Acc. dei Fisiocritici*, v., 1895). An examination of the traces corresponding to seismic movements shows that they exhibit different kinds of oscillations, some short, others long, in period. When the earthquakes occur in neighbouring districts, the disturbance of the instrument is brief and the vibrations are rapid. But, with distant earthquakes, the disturbances last for much longer intervals. They begin with rapid oscillations which generally present several maxima, so as to appear as if distributed in groups; while towards the end, either alone or in company with the former, succeed much slower oscillations, which perhaps correspond to undulatory movements of the earth's crust.

DR. HOEBER has been making some interesting experiments to ascertain whether the presence of water-weeds affects the vitality of anthrax germs in water. For this purpose he constructed small fresh-water aquaria, each of which contained about eight litres of ordinary river Main water, some river sand, and a supply of water-weeds, and in addition about 200,000 anthrax microbes. These aquaria were only submitted to diffused light, and were kept at 10° and 19° C., respectively. Dr. Hoeber presumably worked with anthrax bacilli only, but special precautions were not taken to ensure the absence of spores; no anthrax germs, however, could be found after three days at the lower, and four days at the higher temperature, respectively. In his second report to the Royal Society, Prof. Percy Frankland states that *sporiferous* anthrax retained their vitality in ordinary river Thames water for upwards of seven months without losing their virulence; but when exposed to sunshine they were destroyed after eighty-four hours. On the other hand, when using *anthrax bacilli free from spores*, as derived from the dead body of an animal, the same authority (*Proceedings* Royal Society, 1894, p. 549) states that in sterilised river Thames and loch water they were destroyed in about five days at 5° C., and in fourteen days at 13° C.; but that at 19° C. they multiplied enormously, and were present in large numbers on the forty-second day. This different behaviour was found to be due to the bacilli having formed spores in the water at the higher temperature. The danger of anthrax germs gaining access to water depends, therefore, upon the temperature of the water and the presence or absence of spores in the morbific material. Judging by Dr. Hoeber's experiments, it would appear that the presence of water-weeds and the competition of water-bacteria may offer obstacles to the vitality of anthrax bacilli in water.

A SALE of much interest recently took place at the dispersal of the herd of white polled cattle belonging to Mr. R. E. Lofft, of Troston Hall, Bury St. Edmunds. The herd, which comprised twenty cows and heifers and five bulls, represented the old "monks' cattle," descended from the oldest historic breed of cattle in the British Isles—the polled white, with black or red points on the ears, muzzle, rims of eyes, and hoofs. Under the wave of improvement which set in with the work of Bakewell, of Dishley, more than a century ago, the old hornless white breeds no longer enjoyed the pride of place, and Mr. Lofft's herd really embodied an attempt to restore this ancient breed to something like its former position. It is probable that these cattle were originally selected by the monks, who in their day were the leaders of agriculture. Being hornless, the animals would be more easily domesticated, and less adapted to purposes of sport, such as the chase and bull-baiting. After the dissolution of the monasteries, these cattle were dispersed over the country, and mostly became merged in the common local varieties. A few, however, were kept pure, and at the beginning of this century there were two herds in Suffolk, which quite escaped the notice of the late Rev. John Storer, the historian of the breed. It is satisfactory to know that some of Mr. Lofft's quaint cattle were purchased by Mr. Assheton-Smith, of Vaynol Park, Carnarvonshire, where he has a herd of black-eared and black-muzzled white horned cattle, and is now going to keep some of the polled type.

PROF. L. DE MARCHI, the author of an Italian Hand-book of Meteorology, has contributed an important essay on the theory of cyclones to the *Pubblicazioni* of the Milan Observatory. The discussion consists of 42 small folio pages and 15 plates, and while giving a general account of recent researches, treats the subject chiefly from a mathematical point of view. The following is a brief summary of the principal results arrived at: The changes in the shape and path of a cyclone, as well as all the principal dynamic phenomena that accompany it, may be deduced from the equations of the horizontal motion, if account is taken of the distribution of temperature round the cyclone, both as regards that which previously existed in the mass of air subsequently occupied by the cyclone, and that drawn into the same area by the vertical movements produced by the earth's rotation. Therefore in some cases, if not always, it is useless to have recourse to external causes, and particularly to the general circulation of the atmosphere, to explain the persistence, change of form, or the motion of a cyclone. The general circulation may be the determining cause of a cyclone at a given point; its propagation, or the successive transference of cyclonic conditions to contiguous masses of air, is determined and maintained, at least in some cases, by the disturbances of thermic equilibrium caused by the sun at the surface of the earth, and induced by the earth's rotation.

THE old and fascinating problem concerning the manner in which the ether moves with or through matter has been attacked by Herr L. Zehnder, who contributes an interesting paper on the subject to *Wiedemann's Annalen*. He endeavoured to decide whether the ether is pushed along by atoms or bodies, or whether it passes through them without resistance, or, finally, whether only a portion of the ether adheres to the particles of bodies, and this portion only is carried along. The apparatus used consisted of a cast-iron cylinder in which a piston moved air-tight. A narrow tube led out from one end of the cylinder, doubled back upon itself, and returned by the other end. Now if the cylinder was exhausted of air, and the piston pushed the ether before it, the latter would stream through the narrow tube with a velocity greater than that of the piston in the ratio of the sectional areas of the cylinder and the tube. This ratio was 560, and exhaustion was carried to 1-40,000th of an atmosphere. To test the motion of the ether, a beam from a brilliant sodium flame was passed through two thick parallel glass plates, the second one being silvered at the back. This plate, by its two reflecting surfaces, split the beam into two, each of which travelled through one portion of the narrow tube. The two beams, reflected near the cylinder by a rectangular prism, were recombined by the same thick plate and returned along the way they had travelled, being finally reflected into the reading telescope by the first plate. Interference fringes were thus produced in the field of view, the motion of which would have indicated a motion of the ether. But no such motion was observed when the tubes were thoroughly exhausted, so that it must be concluded that the ether passes freely through solid bodies. The corollary to this conclusion, that there is a relative motion between the earth and the luminiferous ether, though investigated by the author by means of a new and ingenious apparatus on the Rosskopf, near Freiburg, could not be proved.

THE thirteenth part of Kerner and Oliver's "Natural History of Plants," just published by Messrs. Blackie and Son, refers chiefly to the production and characteristics of plant hybrids.

THE June *Journal* of the Chemical Society contains, in addition to papers read before the Society, and abstracts of other papers,

a description of the life-work of the late Prof. J. C. G. de Marignac, by Prof. P. T. Clève, together with a portrait of that lamented chemist.

Mr. C. L. Prince has sent us details of observations made made by him at Crowborough Hill, Sussex, during the great frost of January and February last. In his report, he contrasts the period with other periods of severe cold which have occurred during the present century.

The Guide-books to Middlesex and Hertfordshire, published by Messrs. Iliffe and Son, will direct the tourist's steps aright, and afford him instruction upon points of more or less historical interest, but they furnish very little information with regard to the counties from a scientific point of view.

We have received a "Guide to the Bristol Museum," by Mr. Edward Wilson, the Curator. The Museum contains a large number of valuable objects, and geology is very well represented. With this guide to assist them, students of science must find the collections more helpful than they used to be.

The Lumleian Lectures on certain points in the ætiology of disease, delivered by Dr. P. H. Pye-Smith, F.R.S., before the Royal College of Physicians in 1892, and the Harveian Oration, delivered before the College in the following year, have been published in volume form by Messrs. J. and A. Churchill. The volume also contains a memoir of the life and works of Harvey.

The fact that the report of the Marlborough College Natural History Society for the year 1894 runs into one hundred and fifty pages, may be taken as an indication that the Society is in a satisfactory condition. The report contains summaries of lectures delivered during the year, a description of the College museum and the collections in it, notes and observations, and accounts of the work of sections: it is altogether a creditable production.

Publishers' catalogues are frequently of great assistance to librarians and bibliographers. A catalogue lately issued by W. Engelmann, of Leipzig, belongs to that class of useful publications. It contains descriptions of all the books, memoirs, and periodicals published by Engelmann from the foundation of the firm to February of this year. The books are arranged alphabetically according to the authors' names, and are also classified into subjects. There is, therefore, no difficulty in finding a volume of which the author or the title is known.

The annual report of the Zoological Society of Philadelphia shows that, but for grants made by the City Councils, the Gardens would have had to be closed, the receipts from admissions having been too low to meet expenses. We notice that, in addition to nearly three thousand free admission tickets to charitable institutions, donors, &c., the Society issued fifty thousand tickets to the Board of Education, for the admission of pupils of the elementary schools. The collection of animals now comprises 251 mammals, 416 birds, and 245 reptiles and amphibians, or a total of 912 specimens.

The new editions received during the past few days include the second part of Dr. Michael Foster's standard "Text book of Physiology" (Macmillan), dealing with the tissues of chemical action and their respective mechanisms, and with nutrition. The work, which is now in its sixth edition, has been brought into line with the present state of physiological knowledge. Messrs. J. and A. Churchill have published a sixth revised edition of "A Treatise on Practical Chemistry," by Prof. Frank Clowes; and Messrs. Smith, Elder, and Co. have published a fourth edition of Marshall and Hurst's "Junior Course of Practical Zoology."

There are only four papers in the June number of *Science Progress*, but each of them is an important contribution to scientific literature. Prof. Marshall Ward describes the growth of knowledge concerning the fixation of free nitrogen by plants. He briefly states the aspects of the question, and gives references to the most important papers upon it. A valuable paper on the ratio of the specific heats of gases is contributed by Mr. J. W. Capstick: it affords interesting reading in connection with the recent discussion in these columns of points arising from the kinetic theory of gases, and also with reference to the atomicity of argon. Mr. J. W. Rodger concludes his most useful statement of the progress made in physical chemistry during 1894. The papers are classified in such a manner that it is easy to find what was done in every branch of the subject. The fourth paper is by Mr. J. E. S. Moore, and has for its subject "The Protoplastid Body and the Metaplastid Cell."

The current *Journal* of the Anthropological Institute (No. 4) contains the presidential address delivered by Prof. A. Macalister, F.R.S., in January last. The Institute by no means possesses a membership in proportion to the importance of the subjects fostered by it. "When we consider," remarks Prof. Macalister, "the wide-reaching importance of the myriad of practical problems with which we as anthropologists are concerned, and the useful work which the Institute has done in the past, it is scarcely conceivable that our membership of 302 should be taken as representing the number of persons to whom these matters are interesting. And further, it is little short of a national disgrace that in the largest empire of the world, within whose bounds there are nearly as many separate peoples, and tribes and kindreds and tongues as in all the other nations put together, there is no Imperial department whose function should be to collect and classify the facts of the physical, psychical, and ethical histories of our fellow subjects."

Two years ago the American Philosophical Society, of which Benjamin Franklin was the first President, held a meeting, at Philadelphia, in commemoration of the 150th anniversary of its foundation. The meeting was attended by delegates representing learned societies and institutions in most parts of the world, and was completely successful. The volume containing full reports of the proceedings has only lately appeared, but the delay in its publication is probably due to the many addresses, memoirs, and plates contained in it: for the printing of the communications, and the preparation of nearly sixty plates, necessarily takes time when the work is so carefully done as it seems to be in the volume before us. Among the addresses is one by Dr. Roberts (the delegate of the Royal Astronomical Society), entitled "Illustrations of Progress made during Recent Years in Astronomical Science." This address is illustrated by thirteen plates representing some of Dr. Roberts' classical photographs. A richly illustrated paper on Tertiary Tipulidæ, by Dr. S. H. Scudder, has already been noticed in these columns (vol. 50, p. 111). Seven plates illustrate Dr. A. S. Packard's "Study of the Transformation and Anatomy of *Lagoa crispata*, a Bombycine Moth," and sixteen embellish a paper by Prof. A. Hyatt on "The Phylogeny of an Acquired Characteristic." Limits of space prevent us from referring to the many other papers. Suffice it to say that the volume is a worthy memorial of a remarkable meeting.

The *Zeitschrift für Anorganische Chemie* gives a very complete account of the synthesis of metallic ores by crystallisation from solution in the appropriate molten metal, by Friedrich Roessler. The work includes the production of crystalline sulphides and selenides of such metals as lead, bismuth, and silver, and of arsenides, antimonides, and bismuthides of platinum, palladium, and gold. The production of silver bismuth

sulphide will serve to illustrate the method followed. Twenty grams of bismuth were melted in a covered crucible, and two grams of silver sulphide were added. By solution of the slowly cooled product in nitric acid of specific gravity 1·1, there remained small dark crystals intermixed with silver-white crystals. The latter consisted of a bismuth-silver alloy, and, in time, dissolved in the acid. On drying, the remaining dark crystals were found to possess a steel-blue lustre. They formed pretty groups of octahedra (figure given in the paper) attached in rows. Analyses proved their composition to be well represented by the formula, $AgBiS_2$ or $Ag_2S + Bi_2S_3$.

IN the current number of the *Comptes rendus*, M. Cleve gives the results of a determination of the density of the new gas helium by M. Langlet. The gas, extracted from cleveite, was freed from hydrogen by passage over red-hot copper oxide, and from nitrogen by metallic magnesium. It contained no argon. The density was found to be notably less than the number given by Prof. Ramsay, being 0·139 (air = 1) or 2·02 (hydrogen = 1). The determination of the specific heat of the gas has been taken in hand by the same investigators; their results will be awaited with much interest.

THE additions to the Zoological Society's Gardens during the past week include two Rhesus Monkeys (*Macacus rhesus*, ♂ ♀), from India, presented, respectively, by Sir Henry W. Peek, and Mr. R. Edmeades; a Patas Monkey (*Cercopithecus ruber*, ♀), from West Africa, presented by Mr. C. H. Armitage; a Campbell's Monkey (*Cercopithecus campbelli*, ♂), from West Africa, presented by Miss L. Panther; a Herring Gull (*Larus argentatus*), British, presented by Mr. J. T. Gorvin; three Ocellated Skinks (*Sepsocellatus*), a —— Skink (*Chalcides sepoides*), a Defenceless Lizard (*Agama inermis*), two Diademed Snakes (*Zamenis diadema*), two —— Snakes (*Cœlopeltis moilensis*), four Egyptian Eryx (*Eryx jaculus*), two Cerastes Vipers (*Vipera cerastes*), two Egyptian Cobras (*Naia haje*), from Lower Egypt, presented by Dr. John Anderson, F.R.S.; a White-crowned Monkey (*Cercocebus æthiops*), a White-necked Stork (*Dissura episcopus*), from West Africa; two White Pelicans (*Pelecanus onocrotalus*), from North Africa; a Barraband's Parrakeet (*Polytelis barrabandi*), from New South Wales; three Hamadryads (*Ophiophagus elaps*), from India; fifty Tree Frogs (*Hyla*), from America, deposited; a Red Deer (*Cervus elaphus*), an Argus Pheasant (*Argus giganteus*), three Ruddy-headed Geese (*Bernicla rubidiceps*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1892 V. (BARNARD). The orbit of this comet, which had been discussed by Mr. J. G. Porter (*Astronomical Journal*, No. 310), has recently been made the subject of a further investigation by M. J. Coniel; but the elements, resting as they do on a very few observations, still remain uncertain to a considerable extent. The comet was discovered, photographically, on October 12, 1892, and regular observations do not extend beyond November 22, 1892, about six weeks only from the date of discovery; but an isolated observation made at Nice on December 8, not taken into the discussion by Mr. Porter, induced M. J. Coniel to reopen the inquiry, with the hope of making a better determination of the period. Mr. Porter's orbit represented this observation within the errors - 0·05s. and - 12″·9, in R.A. and Declination respectively; and considering the difficulty of the observation, such a discrepancy is not more than might be anticipated, and consequently does not suggest the possibility of decided improvement. The feature of the more recent discussion is to show that the observations can be equally well satisfied with elements in which the mean daily motion is altered by some 20″. This is not quite the twentieth part of the whole motion, therefore the period is uncertain to its twentieth part, or about 0·3 of a year.

M. Coniel judges that the uncertainty in the mean motion may extend to 25″ on either side of his result, and that the period may lie between 6·23 and 6·84 years. The consequence of this uncertainty is that the search for the comet at future returns must be greatly extended. In 1899, the sweeping ephemeris must be based on a mean motion corresponding to 6·65–6·84 years. In 1905, the comet may be visible if the period lies between 6·34–6·55 years; while in 1911, and even 1912, the return may be expected with still other and possible values of the mean motion. No near approach to Jupiter will take place during this interval, but the situation of the orbit is such that the comet can approach both Jupiter and Mars. Confining attention to the most probable period (6·52 years), the comet approaches the orbit of Jupiter within 0·07R, in Hel. Long. 207°, and that of Mars within 0·012R, in Hel. Long. 28°. This interest in the comet's path is still further increased by the speculation, due to M. Schulhof, that this comet formed originally a part of Wolf's comet, from which it possibly separated in 1815.

MEASUREMENT OF RADIAL VELOCITIES.—The methods at present employed for the measurement of the movements of the heavenly bodies towards or away from the earth usually involve the use of a comparison spectrum, whether the observations be made by eye or by photography. In special cases, however, other methods are employed, as, for example, the use of telluric lines by Duner in the measurement of the sun's rotation. It has not, however, yet been considered practicable to utilise the objective prism for the work, on account of the difficulty of obtaining reference spectra. A new method, which has the great advantage of being applicable to spectra photographed with or without slits, has recently been suggested by M. Orbinsky, of Odessa. (*Astr. Nach.* 3289.) The principle of the method is based on the fact that the displacements of lines are different at different wave-lengths, so that the distance between two lines in a spectrum depends upon the velocity of the source of light; the higher the velocity, the greater or less will be the distance between any two lines in the spectrum, according as the source of light is approaching or receding, and providing the dispersion be sufficient, it may be possible to measure the velocities by this means. Obviously the measurements are much more delicate than the direct measures of the displacements. In practice it is proposed to employ reference stars, the velocities of which have been determined in the ordinary way by photographic comparison spectra of hydrogen or iron. One of these being photographed on the same plate as the star under investigation, the results will give the velocity relatively to the comparison star, and hence the absolute velocity. The instruments employed should give the greatest possible range of wave-lengths, and it will be specially advantageous to obtain as great a difference of dispersion as possible between the extreme ends of the spectrum. It is shown by actual figures that the measurements are quite practicable, both in the case of the Potsdam spectrograph and in the objective prism employed at Harvard College. It is in the case of the latter class of instruments that the method seems most likely to be of practical value.

TWO REMARKABLE BINARY STARS.—Apart from the binary stars which can only be recognised as such by the aid of the spectroscope, the two binary stars of shortest periods at present known are κ Pegasi and δ Equulei. The orbits of these have been redetermined by Dr. See, using all available observations, many of which are due to the industry of Profs. Burnham and Barnard. The elements deduced are as follows (*Astr. Nach.* 3285, 3290):

	κ Pegasi.	δ Equulei.
P	11·42 years	11·45 years
T	1890·03	1892·8
e	0·49	0·14
a	0″·4216	0″·452
i	81°·2	79°·05
☊	110°·25	22°·2
λ	89°·2	0°·0
n	− 31°·5236	− 31°·441

Prof. Burnham has repeatedly called attention to the importance of systematic observations of rapid binaries with large telescopes, so that we should in a few years get good orbits, which in the case of most binaries would require the observations of centuries.

It will be seen that there is still a great gap between the telescopic and spectroscopic binaries, but it is quite possible that as the powers of both instruments are increased the gap may be gradually shortened from both sides.

THE SUN'S PLACE IN NATURE.[1]

VI.

WE come now to the third new point of view. Many apparent stars are really centres of nebulæ, *i.e.* of meteoritic swarms.

In that very simple statement we have perhaps the very greatest and the most fundamental change which has been suggested by the new hypothesis. I am quite certain that all of you who have read text-books of astronomy will be perfectly familiar with the statement that all stars are distant suns. I have written that myself several times, but I now know that it is not true. Some stars, instead of being distant suns like our sun, a condensed mass of gas with a crust gradually forming on it, and a thick atmosphere over it, are simply the brighter condensations, the central condensations of nebulæ, whether they be like that of Andromeda, or planetary nebulæ, or such a nebula as that of Orion. You see the idea is perfectly new and completely different from the old one, which taught us that all stars were suns. Shortly after I made this assertion, photography came to our aid, and I am so fortunate as to be able to prove to you the absolute truth of it by an appeal to Nature herself; that is, I refer for demonstration to autobiographical records with which the heavens themselves have supplied us. Among the finest and most wonderful of the nebulæ is one which, unfortunately, we do not see here, because it is in the southern hemisphere; it is that surrounding the star η in a wonderful constellation, Argo, which it is quite worth while to go south to see, were there no other reasons. From the photograph you see that there is such an intimate connection, such an obvious relation, between star and nebula, that it is impossible for us to imagine for one moment that they are not most closely and intimately connected.

FIG. 24.—Nebula round η Argus (Dr. Gill).

I will now bring before you another case which we can, all of us, see, so far as a certain part of the phenomena is concerned, and especially at this time of the year. I refer to those "stars," the six Pleiads, which you will remember once lost a sister, that one sees in the constellation of the Bull. Here they are, photographed by Dr. Roberts. You see they are not stars; they are nebulæ. What we see in this photograph (see Fig. 25), in the case of each so-called "star," is obvious; we see the centre of condensation, and more than that, it is not a simple condensation, but there are stream-lines going in all directions, and the maximum luminosity, where we locate the "star," is just at the place where, according to this photograph, the greatest number of these streams cut each other, and where, therefore, we should get the greatest possible number of collisions per second of time. The main point demonstrated by this photograph, then, is that we are not dealing with stars anything like our sun; we are simply dealing with nebulous condensations. I can show you the spectra of the brighter parts of these condensations, and you will see that they resemble the spectra of ordinary stars. Broad dark lines of hydrogen are represented in every one; hence, although we are dealing not with a star like the sun, but a meteoric condensation—a place of intersection of streams of nebulous matter—we get a spectrum such as is generally associated with the spectrum of a star. And for this there is very good reason.

Here an interesting point comes in. Suppose that we wished to observe spectroscopically what was going on in these condensations, and that I allow the image of one of them to fall on the slit of the spectroscope, so that we have the condensation at the centre, and the ends of the slit of the spectroscope beyond the condensation. At the centre, where the slit crosses the condensation, of course we should have the spectrum which you have already seen on the screen, a spectrum indicating that there is something there which gives us a continuous spectrum, *i.e.* one rich in all the colours of the rainbow; but that some of the light is absorbed here and there in consequence of the surrounding atmosphere of hydrogen gas. So much for the centre. Next consider what will happen when I observe, for instance, this or that part of the nebula where the condensation is absent; we shall not get absorption phenomena, but we shall get radiation phenomena, and therefore a long bright line representing the radiation of hydrogen over a large area, and at the middle of it the ordinary spectrum of a star. Prof. Campbell, at the Lick Observatory, has recently subjected another star to a similar treatment, and you will see (Fig. 26) what he has found. By putting the slit of the spectroscope upon the image of the star, he finds that he gets the spectrum from one end to the other; but you see that at the place occupied by one of the hydrogen lines he gets a much longer image of the slit, showing that he had to deal there with a star immersed in something which was competent to give a spectrum of hydrogen. What was that something? You can understand perfectly well that, if one of the Pleiads had been examined in the same way, it would be quite possible that we should get just such an appearance as Prof. Campbell was fortunate enough to obtain. This raises an interesting question, in which astronomic thought has been going up and down now for the last fourteen or fifteen years, and I think I can show you exactly how the matter lies. The diameter of the sun is very nearly a million miles. Now, suppose that the diameter of the solar atmosphere was ten million miles; then if we were by any means whatever to spectroscopically examine the image of the sun under such conditions that all the light coming from these different regions could enter the slit of the spectroscope at the same time, and give us, added together, the whole light, we should be able to determine practically what we might be able to see under these conditions by some such considerations as these:—

Diameter of the sun, one million miles.

Diameter of the sun's atmosphere, ten million miles.

We should therefore get the light from the sun in the ratio of 1 to 99 of the light from the atmosphere. Now suppose that there is any chemical connection between the absorption in the light of the sun and the radiation in the light of the sun's atmosphere, if we sweep the slit of the spectroscope along the edge of the sun, the part of the spectrum which writes for us what is going on in the solar photosphere, gives us the spectrum crossed by dark lines; the effect of the atmosphere is to absorb the light of the more distant sun at which we look, and the result of the absorption is to give us dark lines.

But when we look at the atmosphere which is resting on the edge of the sun, and look at it where there is no brighter sun behind, absorption no longer comes into play, and we get bright lines. This is what happens when we look at the solar atmosphere above the sun's edge and the solar atmosphere between us and the sun. So long as we are telling the story of the sun, we get

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page [illegible].)

the dark lines; so long as we are telling the story of the sun's atmosphere, we get bright lines.

We found that the area from which the sunlight comes to us is represented by 1, whereas the area from which the atmospheric light comes to us is represented by 99: so that if the light of the atmosphere is very much dimmer than the light of the central sun, in consequence of its enormous area we may get some light from it intermingled with the light of the sun itself in our spectroscopes.

Therefore, when we look at the complete spectrum, we may lose the dark hydrogen lines in the spectrum of the star, and we may get bright lines instead of dark ones for every line in the spectrum of a star which is filled up by the absorption of a substance the line of which may be seen bright in the spectrum of that star's atmosphere. Thus there is the possibility that when we have to deal with bright lines in the spectrum of an apparent star, we may be dealing with the atmosphere of the star. You will at once see that; if we are dealing with a pure meteoric agglomeration, then of course we shall get that appearance beyond all possible question.

FIG. 25. The Pleiades (Dr. Roberts).

Now, let me give you one or two cases showing you how this thing works out. The strongest case would be that we should get the bright hydrogen lines putting out the dark hydrogen lines, so that if we got a class of stars without any dark hydrogen lines, we should be justified in supposing that those stars had an enormous atmosphere of hydrogen, and that the fainter bright lines from the larger area just cancelled the effect of the other light from the very much smaller area. Another way that we might expect this thing to work would be that we should not get the bright hydrogen lines entirely putting out the dark hydrogen lines, but that we should get a thinner line in the centre of a broader dark one. Now, that really happens in several stars in the heavens.

I give in Fig. 27 untouched photographs of a star in Orion, and a star in Cassiopeiæ. The latter is very like the star in Orion, because all the absorption lines are common to the two stars; but I may point out to you that we get a bright hydrogen line running down the centre of the dark ones. We may have such an effect produced either by a star having an enormous atmosphere, or by the star with which we are dealing being simply the central condensation of an enormous nebula.

FIG. 26. Prof. Campbell's observation of the F line of hydrogen in the spectrum of a bright line star.

I am bound to say that when I began this work in 1876, I was under the impression that such phenomena were due only to the effects of the atmosphere. But one lives and learns, and since then I have come to the conclusion that that explanation is not the best one, and that when we get such phenomena as those you now see on the screen, we have really to deal with the central condensations of nebulous swarms. I do not hesitate to bring these facts before you, because it is particularly in this connection of thought and experiment and comparison that whatever progress which is now being made in astronomical science is being secured.

Associated with this view we have the statement that stars with bright lines are closely associated with nebulæ, as evidenced by their structure. You will see that there is one method which enables us to compare the bright lines in stars like γ Cassiopeiæ with the nebulæ, as it gives us an opportunity of determining whether or not the bright lines seen in the so-called bright-line stars are or are not the same as the bright lines seen in nebulæ. In the first inquiry in this direction, which consisted of a statistical statement of the number of times certain lines were seen in the spectra, both of nebulæ and of bright-line stars, it was stated that nine lines were coincident, and that and other work done about that time was of such a very trenchant nature that Prof. Pickering, who is one of our very highest authorities in all these matters, accepted at once the grouping together of stars having bright lines in their spectra with the nebulæ. That, you see, was another very definite step in advance indeed.

I can show you a map giving you the evidence of this kind which has been brought into court. We have in it the lines seen in the spectrum of the nebula of Orion, and the longer the line is the stronger it is in the photograph. Then we have underneath the lines recorded in the Orion stars, in the bright line stars, and in the planetary nebulæ; and if you will cast your eyes down these chief lines, you will see that there is a considerable number of lines common to all these bodies.

That is the kind of evidence on which we have been compelled to rely to answer the question: Is there any chemical relationship, and therefore physical relationship, between the bright line stars and the nebula of Orion? And you see the evidence is very strongly in favour of an affirmative statement. Not only does Prof. Pickering accept it, but Prof. Keeler also confirms it. He says the spectra of the planetary nebulæ have a remarkable resemblance to the bright line stars.

But even more fortunate for us than all this is the fact that Prof. Campbell has just finished a most important and laborious

study of these stars at the Lick Observatory, and has observed all the lines in the spectra of a much greater number of stars than was available when I began the inquiry; his measurements are very much more accurate than any that were possible then to me. What happens when we come to deal with his results? The thing is a thousand times more convincing than it ever was. When we take Campbell's list, we get very many more coincidences than we had when we dealt with Pickering's. So that, the further we go in this inquiry, the greater is the number of coincidences. I told you that in the first inquiry there were nine coincidences observed; now we get nineteen coincidences out of thirty-three. We are therefore justified in saying that the more these phenomena are observed, the more closely associated are they seen to be.

Let us take the case of one of the brightest stars of this class in Argo, the spectrum of a star which my friend Respighi and myself were the first to see on a very hot night in Madras in 1871, a beautiful spectrum with many bright lines. Now, here these bright lines are indicated in the diagram, and we find by attempting to study their real positions that some of them are due to carbon, and some of them to iron, and some of them to sodium. Prof. Campbell has recently included the study of this star in his work at Lick, and everything that he has done there seems to confirm the idea. The great question is the question of carbon. You know the importance of carbon in a star like this, because we have had carbon differentiating comets from nebulæ, and finally the discovery of carbon in the nebulæ.

I have some apparatus here to show you, which illustrates what one has to do in studying the spectrum of carbon; we must not only deal with it in its ordinary form, and observe the spectrum as seen in the Bunsen flame, and so on, but we must get different compounds of carbon, and expose them to different temperatures and different pressures. That has been done by myself and others; during the last twenty years I suppose I have made thousands of observations on the spectrum of carbon in different forms and conditions.

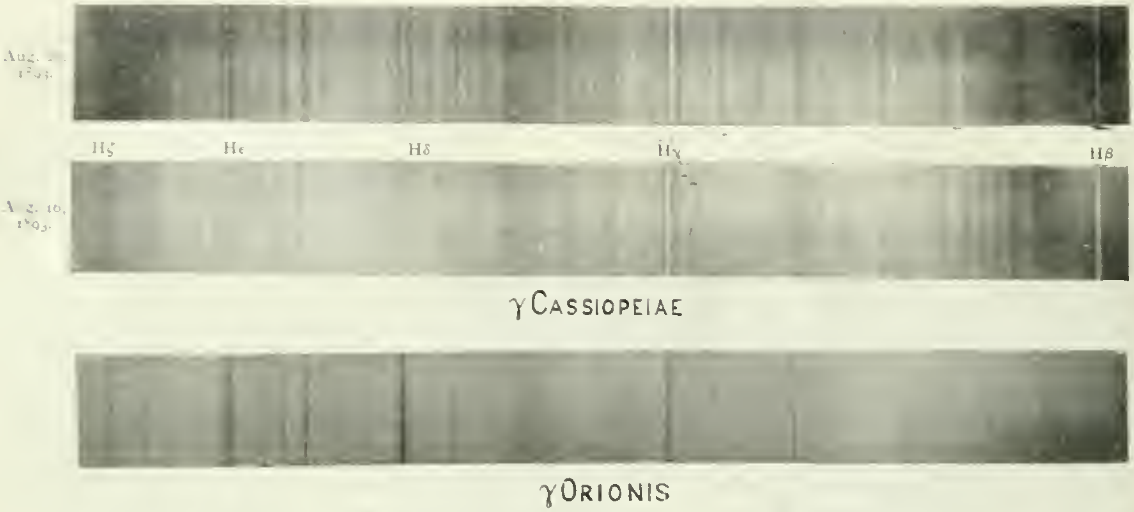


FIG. 27.—Spectra of γ Cassiopeiae and Bellatrix, from photographs taken at South Kensington.

Fig. 28 shows a series of photographs of the same carbon compound in the same tube, taken under different conditions; you will see that there is a very considerable difference in the intensity of the same bands, as the pressure of the gas has been changed; the particular part of one of the bands which you see enhanced seems to be playing a rôle of considerable importance in the spectra of some of these stars.

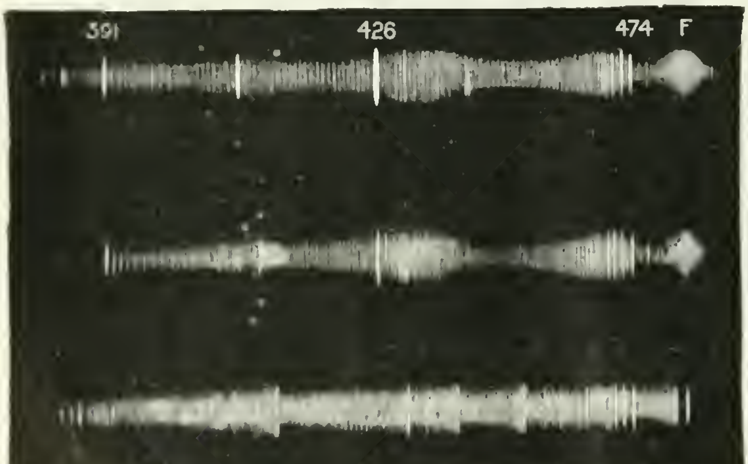


FIG. 28.—Spectrum of carbon at different temperatures.

This is shown merely as an indication of the kind of minute work which is absolutely essential to determine what is happening in the chemical elements in these bodies.

J. NORMAN LOCKYER.

(To be continued.)

THE MANAGEMENT OF EPPING FOREST.

AS a sequel to the continued agitation in the newspapers about Epping Forest, a deputation was received by the Committee at their meeting on Tuesday last at the Guildhall. The object of the deputation was to present the following memorial:—

"Your memorialists have heard with grave concern that your Committee have been urged to put a stop to all further removals of trees in Epping Forest for a period of years. The undersigned have examined the area in question and are of opinion that such a resolution, if sanctioned by your Committee, would be productive of undoubted injury to the Forest, especially as regards those portions of Loughton, Epping, Waltham and Sewardstone Manors which are covered with a dense growth of pollarded trees.

"Those who have approached you with the request to which we have referred do not appear to have apprehended the altered

conditions which were brought about by the arrest of pollarding enacted in 1878.

"Many of these pollards, whether single trees or groups, are capable of picturesque development, but only under healthy conditions and with adequate space. To leave them all to grow together—several hundreds to the acre—will lead to mutual destruction, while the continuous overhead shade destroys the undergrowth and the varied vegetation which constitutes the chief charm of a forest and the hope of its reproduction in the future.

"The evils we have indicated are already sufficiently manifest, and it must be obvious to all competent observers that, unless timely steps are taken, a few years' further growth must produce a singularly monotonous, artificial, and unhealthy result.

"Some of us have been familiar with the Forest for many years, and can certify to the great improvement and the increase of natural growth which has already resulted from the operations of your Committee, now continued for many years."

The following signatures were attached:—The Earl of Gainsborough, Viscount Powerscourt, Lords Northbourne, Rayleigh (Lord Lieutenant of Essex) and Walsingham, Sir John Lubbock, Sir W. H. Flower, Right Hon. J. Bryce (President of the Board of Trade), Right Hon. G. Shaw Lefevre (President Local Government Board), Mr. Justice Wills, Sir Robert Hunter (Solicitor to the Post Office), Prof. G. S. Boulger, Mr. Horace T. Brown, Mr. F. Chancellor (Mayor of Chelmsford), Mr. W. Cole (Secretary to the Essex Field Club), Dr. M. C. Cooke, Prof. J. B. Farmer, Prof. W. R. Fisher (Royal Indian Engineering College), Mr. W. Forbes (Agent to the Duke of Richmond and Gordon), Mr. F. Carruthers Gould, Mr. J. E. Harting, Mr. T. V. Holmes, Mr. David Howard (President of the Essex Field Club), Mr. Andrew Johnston (Chairman of the Essex County Council), Mr. H. Joslin (High Sheriff of Essex), Mr. T. Kemble, Colonel Lockwood, M.P., Dr. Maxwell Masters, Prof. R. Meldola, Mr. Briton Riviere, R.A., Prof. E. B. Poulton, Mr. A. Savill, Prof. Stewart, Mr. W. White (Curator of the Ruskin Museum).

The following memorial, bearing the signatures of about forty residents in the Forest district, was at the same time presented:—

"We, the undersigned, being residents in the Forest parishes, beg to state that we have witnessed with satisfaction a great improvement in the aspect of the Forest directly due to the removal, during the past sixteen years, of inferior stems, and to the consequent advance in beauty of those that remain, as well as the encouragement of healthy young growth. We are certain that it will be an irreparable misfortune if the careful thinning which has been hitherto carried out is not steadily continued.

"We further beg to assure the Committee that in our opinion the operations in Hawk Wood, so far from being excessive, still fall short of what is required for the healthy growth of oak trees.

"In Monk Wood there is already a marked improvement following on your removal, eighteen months ago, of a proportion of the poorest pollarded trees. The same is true, even in a more marked degree, of Lord's Bushes. We believe that, if the gentlemen who have appeared as critics of your management were to judge of it by the appearance of the portions thinned three or four years after thinning, instead of immediately after, when they necessarily have a bare and unattractive effect, they would themselves be of a different opinion.

"In conclusion, we beg to assure you that the view that the action of the Committee has been destructive is not entertained by those living on the spot who are most qualified to judge."

The deputations were formally introduced by the Chairman of the Essex Council, and the first memorial was presented by Prof. Meldola. The Committee was addressed also by Sir Robert Hunter, Prof. Boulger, and Mr. F. C. Gould. After these representations the public may safely disregard all future expressions of irresponsible and unskilled opinions in the press. The Chairman of the Committee assured the deputation that their policy would not be influenced by such criticisms.

SCIENCE IN THE MAGAZINES.

MR. HERBERT SPENCER'S second article on "Professional Institutions" appears in the *Contemporary*. The article deals with the intimate relation between the priest and the medicine-man of early societies, and shows how the physician was originated from the priest. Many proofs are given that medical treatment was long associated with priestly functions, and that the uncultured mind still believes in some of the methods of the primitive medicine-man. Mr. Spencer has also an article in the *Fortnightly*, in which he exhibits the insecure base upon which Mr. Balfour has laid his "Foundations of Belief," and describes that distinguished author's dialectic efforts, as well as Lord Salisbury's address to the British Association at Oxford, as sacrificial offerings of effigies to an apotheosised public. Neither one nor the other have produced the faintest impression in the world of science. Another article which may interest our readers, deals with University degrees for women, the writer comparing the action of Göttingen, in recently granting a degree to Miss Chisholm, with the policy of Oxford and Cambridge Universities as to women students.

In a superbly illustrated paper, entitled "The Discovery of Glacier Bay," that veteran explorer Mr. John Muir gives, in the *Century*, an account of his journey to the now famous Glacier Bay of Alaska, in 1879. The great public library in Boston is described in the same magazine: its artistic aspects by Mrs. S. Van Rensselaer, and its ideals and working conditions by Mr. Lindsay Swift.

That fluent writer Eha, the author of "A Naturalist on the Prowl" and other equally attractive works, contributes a short paper, entitled "Voices of the Indian Night," to the *Sunday Magazine*. Ethnologists may be interested in an article by Miss A. Spinner in the *National*, on beliefs concerning "Duppies" prevalent in the West Indies. A "Duppy" is not simply the negro equivalent for a ghost, but is regarded as the shadow of the departed.

There are two popularly-written papers in *Longman's*, one, of a Selbornian character, by Mr. H. G. Hutchinson, and another concerned with the natural processes involved in the evolution of soil in general, and golf-links in particular, by Dr. Edward Blake.

Science Gossip has among its articles one on explosions in electric light mains, by Mr. J. A. Wanklyn and Mr. W. J. Cooper, and some suggestions with reference to the work of a scientific society, by the Rev. H. N. Hutchinson. *Chambers's Journal* contains short papers on soluble paper, Scottish goldfields, forest dwarfs of the Congo, and the habits and tastes of Lepidoptera. *Scribner* has some common-sense remarks, by Dr. J. W. Roosevelt, on cycling from a physiological point of view.

We have received, in addition to the magazines named in the foregoing, *Humanitarian* and *Good Words*, but no articles in them call for comment here.

ARGON.[1]

IT is some three or four years since I had the honour of lecturing here one Friday evening upon the densities of oxygen and hydrogen gases, and upon the conclusions that might be drawn from the results. It is not necessary, therefore, that I should trouble you to-night with any detail as to the method by which gases can be accurately weighed. I must take that as known, merely mentioning that it is substantially the same as is used by all investigators nowadays, and introduced more than fifty years ago by Regnault. It was not until after that lecture that I turned my attention to nitrogen; and in the first instance I employed a method of preparing the gas which originated with Mr. Vernon Harcourt, of Oxford. In this method the oxygen of ordinary atmospheric air is got rid of with the aid of ammonia. Air is bubbled through liquid ammonia, and then passed through a red-hot tube. In its passage the oxygen of the air combines with the hydrogen of the ammonia, all the oxygen being in that way burnt up and converted into water. The excess of ammonia is subsequently absorbed with acid, and the water by ordinary desiccating agents. That method is very convenient; and, when I had obtained a few concordant results by means of it, I thought that the work was complete, and that the weight of nitrogen was satisfactorily determined. But then I reflected that it is always advisable to employ more than one method, and that the method that I had used—Mr. Vernon Harcourt's method—was not that which had been used by any of those who had preceded me in weighing nitrogen. The usual method consists in absorbing the oxygen of air by means of red-hot copper; and I thought that I ought at least to give that method

[1] A discourse delivered at the Royal Institution on Friday, April 5, by the Right Hon. Lord Rayleigh, F.R.S.

a trial, fully expecting to obtain forthwith a value in harmony with that already afforded by the ammonia method. The result, however, proved otherwise. The gas obtained by the copper method, as I may call it, proved to be one-thousandth part heavier than that obtained by the ammonia method; and, on repetition, that difference was only brought out more clearly. This was about three years ago. Then, in order, if possible, to get further light upon a discrepancy which puzzled me very much, and which, at that time, I regarded only with disgust and impatience, I published a letter in NATURE inviting criticisms from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia. For, before going further, I ought to explain that, in the nitrogen obtained by the ammonia method, some—about a seventh part—is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically derived nitrogen were partly dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

The next step in the inquiry was, if possible, to exaggerate the discrepancy. One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavour to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation: and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the principle suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmospheric air in the ammonia method, so that the whole, instead of only a part, of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified some five times. The nitrogen so obtained from ammonia proved to be about one-half per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call for brevity "atmospheric" nitrogen.

That result stood out pretty sharply from the first: but it was necessary to confirm it by comparison with nitrogen chemically derived in other ways. The table before you gives a summary of such results, the numbers being the weights in grams actually contained under standard conditions in the globe employed.

ATMOSPHERIC NITROGEN.

By hot copper (1892)	2·3103
By hot iron (1893)	2·3100
By ferrous hydrate (1894)	2·3102
Mean	2·3102

CHEMICAL NITROGEN.

From nitric oxide	2·3001
From nitrous oxide	2·2990
From ammonium nitrite purified at a red heat ...	2·2987
From urea	2·2985
From ammonium nitrite purified in the cold ...	2·2987
Mean	2·2990

The difference is about 11 milligrams, or about one-half per cent.; and it was sufficient to prove conclusively that the two kinds of nitrogen—the chemically derived nitrogen and the atmospheric nitrogen—differed in weight, and therefore, of course, in quality, for some reason hitherto unknown.

I need not spend time in explaining the various precautions that were necessary in order to establish surely that conclusion. One had to be on one's guard against impurities, especially against the presence of hydrogen, which might seriously lighten any gas in which it was contained. I believe, however, that the precautions taken were sufficient to exclude all questions of that sort, and the result, which I published about this time last year, stood sharply out, that the nitrogen obtained from chemical sources was different from the nitrogen obtained from the air.

Well, that difference, admitting it to be established, was sufficient to show that some hitherto unknown gas is involved in the matter. It might be that the new gas was dissociated nitrogen, contained in that which was too light, the chemical nitrogen—and at first that was the explanation to which I leaned; but certain experiments went a long way to discourage such a supposition. In the first place, chemical evidence—and in this matter I am greatly dependent upon the kindness of chemical friends—tends to show that, even if ordinary nitrogen could be dissociated at all into its component atoms, such atoms would not be likely to enjoy any very long continued existence. Even ozone goes slowly back to the more normal state of oxygen: and it was thought that dissociated nitrogen would have even a greater tendency to revert to the normal condition. The experiment suggested by that remark was as follows—to keep chemical nitrogen—the too light nitrogen which might be supposed to contain dissociated molecules—for a good while, and to examine whether it changed in density. Of course it would be useless to shut up gas in a globe and weigh it, and then, after an interval, to weigh it again, for there would be no opportunity for any change of weight to occur, even although the gas within the globe had undergone some chemical alteration. It is necessary to re-establish the standard conditions of temperature and pressure which are always understood when we speak of filling a globe with gas, for I need hardly say that filling a globe with gas is but a figure of speech. Everything depends upon the temperature and pressure at which you work. However, that obvious point being borne in mind, it was proved by experiment that the gas did not change in weight by standing for eight months—a result tending to show that the abnormal lightness was not the consequence of dissociation.

Further experiments were tried upon the action of the silent electric discharge—both upon the atmospheric nitrogen and upon the chemically derived nitrogen—but neither of them seemed to be sensibly affected by such treatment; so that, altogether, the balance of evidence seemed to incline against the hypothesis of abnormal lightness in the chemically derived nitrogen being due to dissociation, and to suggest strongly, as almost the only possible alternative, that there must be in atmospheric nitrogen some constituent heavier than true nitrogen.

At that point the question arose, What was the evidence that all the so-called nitrogen of the atmosphere was of one quality? And I remember—I think it was about this time last year, or a little earlier—putting the question to my colleague Prof. Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. That advice I quickly followed, and I was rather surprised to find that Cavendish had himself put this question quite as sharply as I could put it. Translated from the old-fashioned phraseology connected with the theory of phlogiston, his question was whether the inert ingredient of the air is really all of one kind; whether all the nitrogen of the air is really the same as the nitrogen of nitre. Cavendish not only asked himself this question, but he endeavoured to answer it by an appeal to experiment.

I should like to show you Cavendish's experiment in something like its original form. He inverted a U tube filled with mercury, the legs standing in two separate mercury cups. He then passed up, so as to stand above the mercury, a mixture of nitrogen, or of air, and oxygen; and he caused an electric current from a frictional electrical machine like the one I have before me to pass from the mercury in the one leg to the mercury in the other, giving sparks across the intervening column of air. I do not propose to use a frictional machine to-night, but I will substitute for it one giving electricity of the same quality of the construction introduced by Mr. Wimshurst, of which we have a fine specimen in the Institution. It stands just outside the door of the theatre, and will supply an electric current along insulated wires, leading to the mercury cups; and, if we are successful, we shall cause sparks to pass through the small length of air included above the columns of mercury. There they are; and after a little time you will notice that the mercury rises, indicating that the gas is sensibly absorbed under the influence of the sparks and of a piece of potash floating on the mercury. It was by that means that Cavendish established his great discovery of the nature of the inert ingredient in the atmosphere, which we now call nitrogen; and, as I have said, Cavendish himself proposed the question, as distinctly as we can do, Is this inert ingredient all of one kind? and he proceeded to test that question. He found, after days and weeks of protracted experiment, that, for the most part, the nitrogen of the atmosphere absorbed in this manner, was converted into nitrous acid; but that there was a small residue remaining after prolonged treatment with sparks, and a final absorption of the residual oxygen. That residue

amounted to about $\frac{1}{120}$ part of the nitrogen taken; and Cavendish draws the conclusion that if there be more than one inert ingredient in the atmosphere, at any rate the second ingredient is not contained to a greater extent than $\frac{1}{120}$ part.

I must not wait too long over the experiment. Mr. Gordon tells me that a certain amount of contraction has already occurred; and if we project the U upon the screen, we shall be able to verify the fact. It is only a question of time for the greater part of the gas to be taken up, as we have proved by preliminary experiments.

In what I have to say from this point onwards, I must be understood as speaking as much on behalf of Prof. Ramsay as for myself. At the first, the work which we did was to a certain extent independent. Afterwards we worked in concert, and all that we have published in our joint names must be regarded as being equally the work of both of us. But, of course, Prof. Ramsay must not be held responsible for any chemical blunder into which I may stumble to-night.

By his work and by mine the heavier ingredient in atmospheric nitrogen which was the origin of the discrepancy in the densities has been isolated, and we have given it the name of "argon." For this purpose we may use the original method of Cavendish, with the advantages of modern appliances. We can procure more powerful electric sparks than any which Cavendish could command by the use of the ordinary Ruhmkorff coil stimulated by a battery of Grove cells; and it is possible so to obtain evidence of the existence of argon. The oxidation of nitrogen by that method goes on pretty quickly. If you put some ordinary air, or, better still, a mixture of air and oxygen, in a tube in which electric sparks are made to pass for a certain time, then in looking through the tube you observe the well-known reddish-orange fumes of the oxides of nitrogen. I will not take up time in going through the experiment, but will merely exhibit a tube already prepared (image on screen).

One can work more efficiently by employing the alternate currents from dynamo machines which are now at our command. In this Institution we have the advantage of a public supply; and if I pass alternate currents originating in Deptford through this Ruhmkorff coil, which acts as what is now called a "high potential transformer," and allow sparks from the secondary to pass in an inverted test tube between platinum points, we shall be able to show in a comparatively short time a pretty rapid absorption of the gases. The electric current is led into the working chamber through bent glass tubes containing mercury, and provided at their inner extremities with platinum points. In this arrangement we avoid the risk, which would otherwise be serious, of a fracture just when we least desired it. I now start the sparks by switching on the Ruhmkorff to the alternate current supply; and, if you will take note of the level of the liquid representing the quantity of mixed gases included, I think you will see after, perhaps, a quarter of an hour that the liquid has very appreciably risen, owing to the union of the nitrogen and the oxygen gases under the influence of the electrical discharge, and subsequent absorption of the resulting compound by the alkaline liquid with which the gas space is enclosed.

By means of this little apparatus, which is very convenient for operations upon a moderate scale, such as for analyses of "nitrogen" for the amount of argon that it may contain, we are able to get an absorption of about 80 cubic centimetres per hour) or about 4 inches along this test tube, when all is going well. In order, however, to obtain the isolation of argon on any considerable scale by means of the oxygen method, we must employ an apparatus still more enlarged. The isolation of argon requires the removal of nitrogen, and, indeed, of very large quantities of nitrogen, for, as it appears, the proportion of argon contained in atmospheric nitrogen is only about 1 per cent., so that for every litre of argon that you wish to get you must eat up some hundred litres of nitrogen. That, however, can be done upon an adequate scale by calling to our aid the powerful electric discharge now obtainable by means of the alternate current supply and high potential transformers.

In what I have done upon this subject I have had the advantage of the advice of Mr. Crookes, who some years ago drew special attention to the electric discharge or flame, and showed that many of its properties depended upon the fact that it had the power of causing, upon a very considerable scale, a combination of the nitrogen and the oxygen of the air in which it was made.

I had first thought of showing in the lecture room the actual apparatus which I have employed for the concentration of argon; but the difficulty is that, as the apparatus has to be used, the working parts are almost invisible, and I came to the conclusion that it would really be more instructive as well as more convenient to show the parts isolated, a very little effort of imagination being then all that is required in order to reconstruct in the mind the actual arrangements employed.

First, as to the electric arc or flame itself. We have here a transformer made by Pike and Harris. It is not the one that I have used in practice; but it is convenient for certain purposes, and it can be connected by means of a switch with the alternate currents of 100 volts furnished by the Supply Company. The platinum terminals that you see here are modelled exactly upon the plan of those which have been employed in practice. I may say a word or two on the question of mounting. The terminals require to be very massive on account of the heat evolved. In this case they consist of platinum wire doubled upon itself six times. The platinums are continued by iron wires going through glass tubes, and attached at the ends to the copper leads. For better security, the tubes themselves are stopped at the lower ends with corks and charged with water, the advantage being that, when the whole arrangement is fitted by means of an indiarubber stopper into a closed vessel, you have a witness that, as long as the water remains in position, no leak can have occurred through the insulating tubes conveying the electrodes.

Now, if we switch on the current and approximate the points sufficiently, we get the electric flame. There you have it. It is, at present, showing a certain amount of soda. That in time would burn off. After the arc has once been struck, the platinums can be separated; and then you have two tongues of fire ascending almost independently of one another, but meeting above. Under the influence of such a flame, the oxygen and the nitrogen of the air combine at a reasonable rate, and in this way the nitrogen is got rid of. It is now only a question of boxing up the gas in a closed space, where the argon concentrated by the combustion of the nitrogen can be collected. But there are difficulties to be encountered here. One cannot well use anything but a glass vessel. There is hardly any metal available that will withstand the action of strong caustic alkali and of the nitrous fumes resulting from the flame. One is practically limited to glass. The glass vessel employed is a large flask with a single neck, about half full of caustic alkali. The electrodes are carried through the neck by means of an indiarubber bung provided also with tubes for leading in the gas. The electric flame is situated at a distance of only about half an inch above the caustic alkali. In that way an efficient circulation is established; the hot gases as they rise from the flame strike the top, and then as they come round again in the course of the circulation they pass sufficiently close to the caustic alkali to ensure an adequate removal of the nitrous fumes.

There is another point to be mentioned. It is necessary to keep the vessel cool; otherwise the heat would soon rise to such a point that there would be excessive generation of steam, and then the operation would come to a standstill. In order to meet this difficulty the upper part of the vessel is provided with a water-jacket, in which a circulation can be established. No doubt the glass is severely treated, but it seems to stand it in a fairly amiable manner.

By means of an arrangement of this kind, taking nearly three-horse power from the electric supply, it is possible to consume nitrogen at a reasonable rate. The transformers actually used are the "Hedgehog" transformers of Mr. Swinburne, intended to transform from 100 volts to 2400 volts. By Mr. Swinburne's advice I have used two such, the fine wires being in series so as to accumulate the electrical potential and the thick wires in parallel. The rate at which the mixed gases are absorbed is about seven litres per hour; and the apparatus, when once fairly started, works very well as a rule, going for many hours without attention. At times the arc has a trick of going out, and it then requires to be restarted by approximating the platinums. We have already worked fourteen hours on end, and by the aid of one or two automatic appliances it would, I think, be possible to continue operations day and night.

The gases, air and oxygen in about equal proportions, are mixed in a large gasholder, and are fed in automatically as required. The argon gradually accumulates: and when it is desired to stop operations the supply of nitrogen is cut off, and only pure oxygen allowed admittance. In this way the remaining nitrogen is consumed, so that, finally, the working vessel is charged with a mixture of argon and oxygen only, from which

the oxygen is removed by ordinary well-known chemical methods. I may mention that at the close of the operation, when the nitrogen is all gone, the arc changes its appearance, and becomes of a brilliant blue colour.

I have said enough about this method, and I must now pass on to the alternative method which has been very successful in Prof. Ramsay's hands—that of absorbing nitrogen by means of red-hot magnesium. By the kindness of Prof. Ramsay and Mr. Matthews, his assistant, we have here the full scale apparatus before us almost exactly as they use it. On the left there is a reservoir of nitrogen derived from air by the simple removal of oxygen. The gas is then dried. Here it is bubbled through sulphuric acid. It then passes through a long tube made of hard glass and charged with magnesium in the form of thin turnings. During the passage of the gas over the magnesium at a bright red heat, the nitrogen is absorbed in a greater degree, and the gas which finally passes through is immensely richer in argon than that which first enters the hot tube. At the present time you see a tolerably rapid bubbling on the left, indicative of the flow of atmospheric nitrogen into the combustion furnace; whereas, on the right, the outflow is very much slower. Care must be taken to prevent the heat rising to such a point as to soften the glass. The concentrated argon is collected in a second gas-holder, and afterwards submitted to further treatment. The apparatus employed by Prof. Ramsay in the subsequent treatment is exhibited in the diagram, and is very effective for its purpose; but I am afraid that the details of it would not readily be followed from any explanation that I could give in the time at my disposal. The principle consists in the circulation of the mixture of nitrogen and argon over hot magnesium, the gas being made to pass round and round until the nitrogen is effectively removed from it. At the end that operation, as in the case of the oxygen method, proceeds somewhat slowly. When the greater part of the nitrogen is gone, the remainder seems to be unwilling to follow, and it requires somewhat protracted treatment in order to be sure that the nitrogen has wholly disappeared. When I say "wholly disappeared," that, perhaps, would be too much to say in any case. What we can say is that the spectrum test is adequate to show the presence, or at any rate to show the addition, of about $1\frac{1}{2}$ per cent. of nitrogen to argon as pure as we can get it; so that it is fair to argue that any nitrogen at that stage remaining in the argon is only a small fraction of $1\frac{1}{2}$ per cent.

I should have liked at this point to be able to give advice as to which of the two methods—the oxygen method or the magnesium method—is the easier and the more to be recommended; but I confess that I am quite at a loss to do so. One difficulty in the comparison arises from the fact that they have been in different hands. As far as I can estimate, the quantities of nitrogen eaten up in a given time are not very different. In that respect, perhaps, the magnesium method has some advantage; but, on the other hand, it may be said that the magnesium process requires a much closer supervision, so that, perhaps, fourteen hours of the oxygen method may not unfairly compare with eight hours or so of the magnesium method. In practice a great deal would depend upon whether in any particular laboratory alternate currents are available from a public supply. If the alternate currents are at hand, I think it may probably be the case that the oxygen method is the easier; but, otherwise, the magnesium method would, probably, be preferred, especially by chemists who are familiar with operations conducted in red-hot tubes.

I have here another experiment illustrative of the reaction between magnesium and nitrogen. Two rods of that metal are suitably mounted in an atmosphere of nitrogen, so arranged that we can bring them into contact and cause an electric arc to form between them. Under the action of the heat of the electric arc the nitrogen will combine with the magnesium; and if we had time to carry out the experiment we could demonstrate a rapid absorption of nitrogen by this method. When the experiment was first tried, I had hoped that it might be possible, by the aid of electricity, to start the action so effectively that the magnesium would continue to burn independently under its own developed heat in the atmosphere of nitrogen. Possibly, on a larger scale, something of this sort might succeed, but I bring it forward here only as an illustration. We turn on the electric current, and bring the magnesiums together. You see a brilliant green light, indicating the vaporisation of the magnesium. Under the influence of the heat the magnesium burns, and there is collected in the glass vessel a certain amount of brownish-looking powder which consists mainly of the nitride of magnesium. Of course, if there is any oxygen present it has the preference, and the ordinary white oxide of magnesium is formed.

The gas thus isolated is proved to be inert by the very fact of its isolation. It refuses to combine under circumstances in which nitrogen, itself always considered very inert, does combine—both in the case of the oxygen treatment and in the case of the magnesium treatment; and these facts are, perhaps, almost enough to justify the name which we have suggested for it. But, in addition to this, it has been proved to be inert under a considerable variety of other conditions such as might have been expected to tempt it into combination. I will not recapitulate all the experiments which have been tried, almost entirely by Prof. Ramsay, to induce the gas to combine. Hitherto, in our hands, it has not done so; and I may mention that recently, since the publication of the abstract of our paper read before the Royal Society, argon has been submitted to the action of titanium at a red heat, titanium being a metal having a great affinity for nitrogen, and that argon has resisted the temptation to which nitrogen succumbs. We never have asserted, and we do not now assert, that argon can under no circumstances be got to combine. That would, indeed, be a rash assertion for any one to venture upon; and only within the last few weeks there has been a most interesting announcement by M. Berthelot, of Paris, that, under the action of the silent electric discharge, argon can be absorbed when treated in contact with the vapour of benzine. Such a statement, coming from so great an authority, commands our attention; and if we accept the conclusions, as I suppose we must do, it will follow that argon has, under those circumstances, combined.

Argon is rather freely soluble in water. That is a thing that troubled us at first in trying to isolate the gas; because, when one was dealing with very small quantities, it seemed to be always disappearing. In trying to accumulate it we made no progress. After a sufficient quantity had been prepared, special experiments were made on the solubility of argon in water. It has been found that argon, prepared both by the magnesium method and by the oxygen method, has about the same solubility in water as oxygen—some two-and-a-half times the solubility of nitrogen. This suggests, what has been verified by experiment, that the dissolved gases of water should contain a larger proportion of argon than does atmospheric nitrogen. I have here an apparatus of a somewhat rough description, which I have employed in experiments of this kind. The boiler employed consists of an old oil-can. The water is supplied to it and drawn from it by coaxial tubes of metal. The incoming cold water flows through the outer annulus between the two tubes. The outgoing hot water passes through the inner tube, which ends in the interior of the vessel at a higher level. By means of this arrangement the heat of the water which has done its work is passed on to the incoming water not yet in operation, and in that way a limited amount of heat is made to bring up to the boil a very much larger quantity of water than would otherwise be possible, the greater part of the dissolved gases being liberated at the same time. These are collected in the ordinary way. What you see in this flask is dissolved air collected out of water in the course of the last three or four hours. Such gas, when treated as if it were atmospheric nitrogen, that is to say after removal of the oxygen and minor impurities, is found to be decidedly heavier than atmospheric nitrogen to such an extent as to indicate that the proportion of argon contained is about double. It is obvious, therefore, that the dissolved gases of water form a convenient source of argon, by which some of the labour of separation from air is obviated. During the last few weeks I have been supplied from Manchester by Mr. Macdougall, who has interested himself in this matter, with a quantity of dissolved gases obtained from the condensing water of his steam engine.

As to the spectrum, we have been indebted from the first to Mr. Crookes, and he has been good enough to-night to bring some tubes which he will operate, and which will show you at all events the light of the electric discharge in argon. I cannot show you the spectrum of argon, for unfortunately the amount of light from a vacuum tube is not sufficient for the projection of its spectrum. Under some circumstances the light is red, and under other circumstances it is blue. Of course when these lights are examined with the spectroscope—and they have been examined by Mr. Crookes with great care—the differences in the colour of the light translate themselves into different groups of spectrum lines. We have before us Mr. Crookes' map, showing

the two spectra upon a very large scale. The upper is the spectrum of the blue light; the lower is the spectrum of the red light; and it will be seen that they differ very greatly. Some lines are common to both; but a great many lines are seen only in the red, and others are seen only in the blue. It is astonishing to notice what trifling changes in the conditions of the discharge bring about such extensive alterations in the spectrum.

One question of great importance, upon which the spectrum throws light is, Is the argon derived by the oxygen method really the same as the argon derived by the magnesium method? By Mr. Crookes' kindness I have had an opportunity of examining the spectra of the two gases side by side, and such examination as I could make revealed no difference whatever in the two spectra, from which, I suppose, we may conclude either that the gases are absolutely the same, or, if they are not the same, that at any rate the ingredients by which they differ cannot be present in more than a small proportion in either of them.

My own observations upon the spectrum have been made principally at atmospheric pressure. In the ordinary process of sparking, the pressure is atmospheric: and, if we wish to look at the spectrum, we have nothing more to do than to include a jar in the circuit, and put a direct-vision prism to the eye. At my request, Prof. Schuster examined some tubes containing argon at atmospheric pressure prepared by the oxygen method, and I have here a diagram of a characteristic group. He also placed upon the sketch some of the lines of zinc, which were very convenient as directing one exactly where to look. (See Fig. 1.)

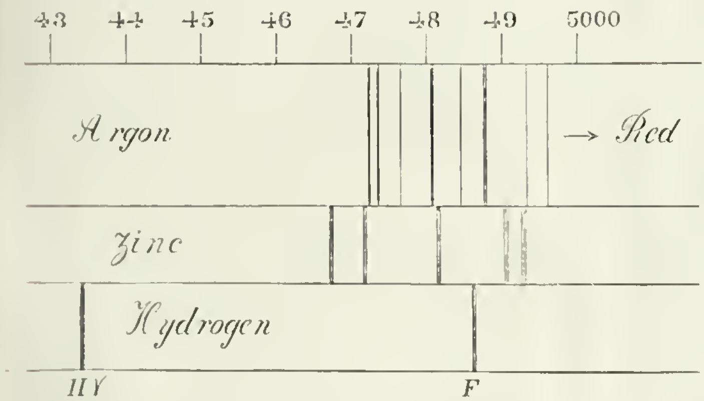


FIG. 1.

Within the last few days, Mr. Crookes has charged a radiometer with argon. When held in the light from the electric lamp, the vanes revolve rapidly. Argon is anomalous in many respects, but not, you see, in this.

Next, as to the density of argon. Prof. Ramsay has made numerous and careful observations upon the density of the gas prepared by the magnesium method, and he finds a density of about 19·9 as compared with hydrogen. Equally satisfactory observations upon the gas derived by the oxygen method have not yet been made, but there is no reason to suppose that the density is different, such numbers as 19·7 having been obtained.

One of the most interesting matters in connection with argon, however, is what is known as the ratio of the specific heats. I must not stay to elaborate the questions involved, but it will be known to many who hear me that the velocity of sound in a gas depends upon the ratio of two specific heats—the specific heat of the gas measured at constant pressure, and the specific heat measured at constant volume. If we know the density of a gas, and also the velocity of sound in it, we are in a position to infer this ratio of specific heats; and by means of this method, Prof. Ramsay has determined the ratio in the case of argon, arriving at the very remarkable result that the ratio of specific heats is represented by the number 1·65, approaching very closely to the theoretical limit, 1·67. The number 1·67 would indicate that the gas has no energy except energy of translation of its molecules. If there is any other energy than that, it would show itself by this number dropping below 1·67. Ordinary gases, oxygen, nitrogen, hydrogen, &c., do drop below, giving the number 1·4. Other gases drop lower still. If the ratio of specific heats is 1·65, practically 1·67, we may infer then that the whole energy of motion is translational; and from that it would seem to follow by arguments which, however, I must not stop to elaborate, that the gas must be of the kind called by chemists monatomic.

I had intended to say something of the operation of determining the ratio of specific heats, but time will not allow. The result is, no doubt, very awkward. Indeed I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had the very best intentions in the matter. The facts were too much for us; and all we can do now is to apologise for ourselves and for the gas. Several questions may be asked, upon which I should like to say a word or two, if you will allow me to detain you a little longer. The first question (I do not know whether I need ask it) is, Have we got hold of a new gas at all? I had thought that that might be passed over, but only this morning I read in a technical journal the suggestion that argon was our old friend nitrous oxide. Nitrous oxide has roughly the density of argon; but that, as far as I can see, is the only point of resemblance between them.

Well, supposing that there is a new gas, which I will not stop to discuss, because I think the spectrum alone would be enough to prove it, the next question that may be asked is, Is it in the atmosphere? This matter naturally engaged our earnest attention at an early stage of the inquiry. I will only indicate in a few words the arguments which seem to us to show that the answer must be in the affirmative.

In the first place, if argon be not in the atmosphere, the original discrepancy of densities which formed the starting point of the investigation remains unexplained, and the discovery of the new gas has been made upon a false clue. Passing over that, we have the evidence from the blank experiments, in which nitrogen originally derived from chemical sources is treated either with oxygen or with magnesium, exactly as atmospheric nitrogen is treated. If we use atmospheric nitrogen, we get a certain proportion of argon, about 1 per cent. If we treat chemical nitrogen in the same way, we get, I will not say absolutely nothing, but a mere fraction of what we should get had atmospheric nitrogen been the subject. You may ask, why do we get any fraction at all from chemical nitrogen? It is not difficult to explain the small residue, because in the manipulation of the gases large quantities of water are used; and, as I have already explained, water dissolves argon somewhat freely. In the processes of manipulation some of the argon will come out of solution, and it remains after all the nitrogen has been consumed.

Another wholly distinct argument is founded upon the method of diffusion introduced by Graham. Graham showed that if you pass gas along porous tubes you alter the composition, if the gas is a mixture. The lighter constituents go more readily through the pores than do the heavier ones. The experiment takes this form. A number of tobacco pipes—eight in the actual arrangement—are joined together in series with indiarubber junctions, and they are put in a space in which a vacuum can be made, so that the space outside the porous pipes is vacuous, or approximately so. Through the pipes ordinary air is led. One end may be regarded as open to the atmosphere. The other end is connected with an aspirator so arranged that the gas collected is only some 2 per cent of that which leaks through the porosities. The case is like that of an Australian river drying up almost to nothing in the course of its flow. Well, if we treat air in that way, collecting only the small residue which is less willing than the remainder to penetrate the porous walls, and then prepare "nitrogen" from it by removal of oxygen and moisture, we obtain a gas heavier than atmospheric nitrogen, a result which proves that the ordinary nitrogen of the atmosphere is not a single body, but is capable of being divided into parts by so simple an agent as the tobacco pipe.

If it be admitted that the gas is in the atmosphere, the further question arises as to its nature.

At this point I would wish to say a word of explanation. Neither in our original announcement at Oxford, nor at any time since, until January 31, did we utter a word suggesting that argon was an element; and it was only after the experiments upon the specific heats that we thought that we had sufficient to go upon in order to make any such suggestion in public. I will

not insist that that observation is absolutely conclusive. It is certainly strong evidence. But the subject is difficult, and one that has given rise to some difference of opinion among physicists. At any rate this property distinguishes argon very sharply from all the ordinary gases.

One question which occurred to us at the earliest stage of the inquiry, as soon as we knew that the density was not very different from 21, was the question of whether, possibly, argon could be a more condensed form of nitrogen, denoted chemically by the symbol N_3. There seem to be several difficulties in the way of this supposition. Would such a constitution be consistent with the ratio of specific heats (1·65)? That seems extremely doubtful. Another question is, Can the density be really as high as 21, the number required on the supposition of N_3? As to this matter, Prof. Ramsay has repeated his measurements of density, and he finds that he cannot get even so high as 20. To suppose that the density of argon is really 21, and that it appears to be 20 in consequence of nitrogen still mixed with it, would be to suppose a contamination with nitrogen out of all proportion to what is probable. It would mean some 14 per cent. of nitrogen, whereas it seems that from $1\frac{1}{2}$ to 2 per cent. is easily enough detected by the spectroscope. Another question that may be asked is, Would N_3 require so much cooling to condense it as argon requires?

There is one matter on which I would like to say a word—the question as to what N_3 would be like if we had it? There seems to be a great discrepancy of opinions. Some high authorities, among whom must be included, I see, the celebrated Mendeleef, consider that N_3 would be an exceptionally stable body; but most of the chemists with whom I have consulted are of opinion that N_3 would be explosive, or, at any rate, absolutely unstable. That is a question which may be left for the future to decide. We must not attempt to put these matters too positively. The balance of evidence still seems to be against the supposition that argon is N_3, but for my part I do not wish to dogmatise.

A few weeks ago we had an eloquent lecture from Prof. Rücker on the life and work of the illustrious Helmholtz. It will be known to many that during the last few months of his life Helmholtz lay prostrate in a semi-paralysed condition, forgetful of many things, but still retaining a keen interest in science. Some little while after his death we had a letter from his widow, in which she described how interested he had been in our preliminary announcement at Oxford upon this subject, and how he desired the account of it to be read to him over again. He added the remark, "I always thought that there must be something more in the atmosphere."

A SPECTROSCOPIC PROOF OF THE METEORIC CONSTITUTION OF SATURN'S RINGS.[1]

THE hypothesis that the rings of Saturn are composed of an immense multitude of comparatively small bodies, revolving around Saturn in circular orbits, has been firmly established since the publication of Maxwell's classical paper in 1850. The grounds on which the hypothesis is based are too well known to require special mention. All the observed phenomena of the rings are naturally and completely explained by it, and mathematical investigation shows that a solid or fluid ring could not exist under the circumstances in which the actual ring is placed.

The spectroscopic proof which Prof. Keeler has recently obtained of the meteoric constitution of the ring, is of interest because it is the first *direct* proof of the correctness of the accepted hypothesis, and because it illustrates in a very beautiful manner the truthfulness of Doppler's principle, and the value of the spectroscope as an instrument for the measurement of celestial motions.

Since the relative velocities of different parts of the ring would be essentially different under the two hypotheses of rigid structure and meteoric constitution, it is possible to distinguish between these hypotheses by measuring the motion of different parts of the ring in the line of sight. The only difficulty is to find a method so delicate that the very small differences of velocity in question may not be masked by instrumental errors. Success in visual observations of the spectrum is hardly to be expected.

After a number of attempts, Prof. Keeler obtained two fine photographs of the lower spectrum of Saturn on April 9 and 10 of the present year. The exposure in each case was two hours, and the image of the planet was kept very accurately central on the slit-plate. After the exposure the spectrum of the Moon was photographed on each side of the spectrum of Saturn, and nearly in contact with it. Each part of the lunar spectrum has a width of about one millimetre, which is also nearly the extreme width of the planetary spectrum. On both sides of the spectrum of the ball of the planet are the narrow spectra of the ansæ of the ring. The length of the spectrum from *b* to D is 23 millimetres.

These photographs not only show very clearly the relative displacement of the lines in the spectrum of the ring, due to the opposite motions of the ansæ, but exhibit another peculiarity, which is of special importance in connection with the subject of the present paper. The planetary lines are strongly inclined, in consequence of the rotation of the ball, but the lines in the spectra of the ansæ do not follow the direction of the lines in the central spectrum; they are nearly parallel to the lines of the comparison spectrum, and, in fact, as compared with the lines of the ball, have a slight tendency to incline in the opposite direction. Hence the outer ends of these lines are less displaced than the inner ends. Now it is evident that if the ring rotated as a whole, the velocity of the outer edge would exceed that of the inner edge, and the lines of the ansæ would be inclined in the same direction as those of the ball of the planet. If, on the other hand, the ring is an aggregation of satellites revolving around Saturn, the velocity would be greatest at the inner edge, and the inclination of lines in the spectra of the ansæ would be reversed. The photographs are therefore a direct proof of the approximate correctness of the latter supposition.

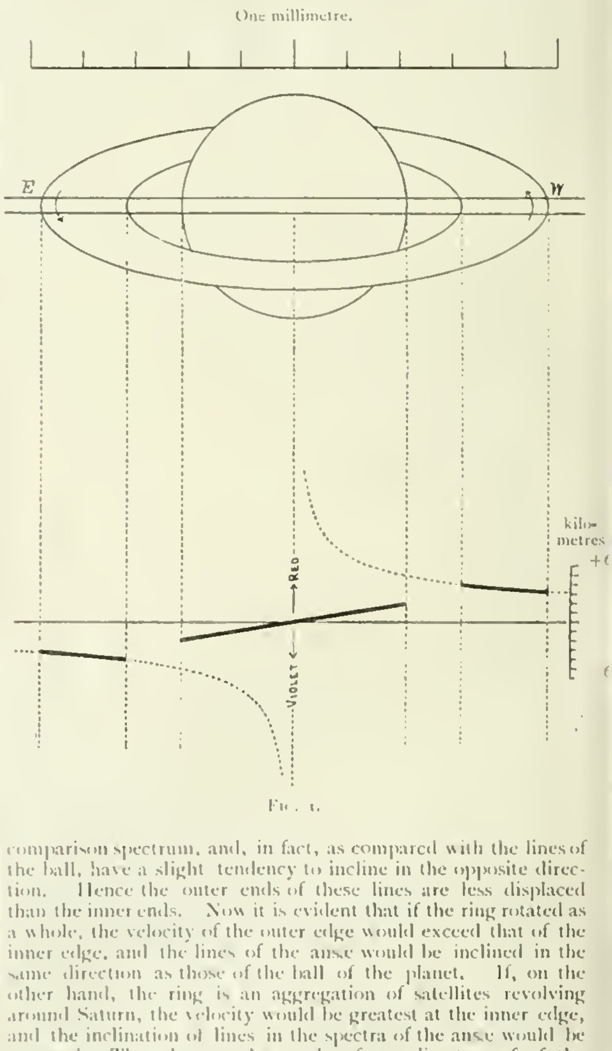


FIG. 1.

It is interesting to determine the form of a line in the spectrum of Saturn when the slit is in the major axis of the ring, on the assumption that the planet rotates as a solid body, and that the ring is a swarm of particles revolving in circular orbits according to Kepler's third law. At present the motion of the system as a whole is neglected. The upper part of Fig. 1 represents the image of Saturn on the slit of the spectroscope (the scale

[1] [illegible] Prof. James E. Keeler in the *Astrophysical Journal* [illegible]

above it applies to the instrument used at Allegheny), and the narrow horizontal line in the lower part of the figure represents an undisplaced line in the spectrum, or solar line.

By Doppler's principle, the displacement of any point on this line is proportional to the velocity in the line of sight. The inclination of the planetary line to the solar line can be expressed by a simple formula. It is also possible to determine the form of a line in the spectrum of the ring, regarded as a collection of satellites, by the application of Kepler's third law. With the computed motions of different parts of the system, the dotted curves in the figure were plotted. For the ordinates, however, twice the calculated values were taken, since the displacement of a line, due to motion in the line of sight, is doubled in the case of a body which shines by reflected and not by inherent light, provided (as in this case) the Sun and the Earth are in sensibly the same direction from the body. The planetary line is drawn to the same scale, and the heavy lines in the figure represent accurately the aspect of a line in the spectrum of Saturn, with the slit in the axis of the ring, as photographed with a spectroscope having about three times the dispersion of the instrument used by Prof. Keeler.

The width of slit used is also represented in the figure.

If the whole system has a motion in the line of sight, the lines in the figure will be displaced towards the top or the bottom, as the case may be, but their relative positions will not be altered.

It is evident that in making a photograph of this kind the image must be kept very accurately in the same position on the slit-plate, as otherwise the form of the lines shown in the figure would be lost by the superposition of points having different velocities. The second plate was made with special care, and as the air was steadier than on the first occasion, the definition is on the whole somewhat better than that of plate 1, although the difference is not great. On both plates the aspect of the spectrum is closely in accordance with that indicated by theory, and represented in the figure. The planetary lines are inclined from 3° to 4°, and the lines in the spectra of the ansæ have the appearance already described.

If the ring revolved as a whole, the displacement of lines in its spectrum would follow the same law as for a rotating sphere; that is, the lines would be straight and inclined, their direction passing through the origin. If the ring rotated in the period of its mean radius, a glance at the figure shows that the lines would practically be continuations of the planetary lines. Such an aspect of the lines as this would be recognisable on the photographs at a glance.

It will be seen from the foregoing considerations that the photographs prove not only that the velocity of the inner edge of Saturn's ring exceeds the velocity of the outer edge, but that, within the limits of error of the method, the relative velocities at different parts are such as to satisfy Kepler's third law.

Besides (1) the proof of the meteoric constitution of the rings, explained above, each line of the photographs gives (2) the period of rotation of the planet, (3) the mean period of the rings, (4) the motion of the whole system in the line of sight. Prof. Keeler has measured a number of lines on each plate, and compared the results with the computed values of the corresponding quantities.

The results for (2) and (3) from both photographs are:

(2) Velocity of limb = $10 \cdot 3 \pm 0 \cdot 4$ kilometres,
(3) Mean velocity of ring = $18 \cdot 0 \pm 0 \cdot 3$ kilometres;

the computed values being 10·29 and 18·78 kilometres respectively.

Prof. Keeler has not yet determined from his photographs the motion of the whole system in the line of sight.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. T. J. I. Bromwich, Scholar of St. John's College, is the Senior Wrangler of the year. There are thirty Wranglers, of whom St. John's furnishes ten, and Trinity six. One lady only is among the Wranglers, namely Miss N. A. L. Thring, of Newnham, who is placed twenty-third in the list.

The Tyson Medal for Astronomy is awarded to Mr. A. V. G. Campbell, of Trinity.

Sir Edward Maunde Thompson, K.C.B., has been appointed the first Sandars Reader in Bibliography for the year 1895-6.

The Board of Managers of the Arnold Gerstenberg Studentship give notice that a Studentship on this Foundation will be offered for competition in 1896. The competition will be open to men and women who have obtained honours in Part I. or Part II. of the Natural Sciences Tripos, and whose first term of residence was not earlier than the Michaelmas term of 1890. The Studentship will be awarded to the writer of the best essay on one of the six subjects printed below. The essays must be sent before October 1, 1896, to Dr. Sidgwick, Newnham College, Cambridge. The Studentship will be of the value of nearly £90. It will be tenable for one year only, but subject to no conditions of tenure.

Subjects:—"A statement of the physicist's 'working conceptions' of Matter and Motion, together with a discussion of the philosophical questions to which they give rise." "A criticism of the diverse views that have prevailed from the time of Newton onwards as to the conceivability or otherwise of *Actio in distans*." "A critical examination of the doctrines of J. S. Mill concerning the ground of Induction and the Methods of Inductive Inquiry." "The limits and relations of mechanical and teleological explanations of natural phenomena." "A brief historical account and a critical examination of the views which make the phenomena of life dependent on the existence of a special vital principle." "Natural Selection considered as a special example of the general principle of Evolution."

WITH the view of encouraging University Extension students to take up systematic courses of study, the Local Examinations and Lectures Syndics have remodelled their scheme of Local Lectures Certificates, and have made several other changes of importance. The certificates are now arranged so as to form successive steps in a ladder of continuous work, beginning with the Terminal Certificate for one term's work passing through the Sessional Certificate for a year's work to the Vice-Chancellor's Certificate of Systematic Study for four years' work. There is also an Affiliation Certificate obtainable only at centres affiliated to the University. This certificate is accepted by the Education Department as qualifying a person to be recognised as an assistant teacher. This system is thus adapted to the needs of persons who merely desire a general acquaintance with the subjects taught, as well as to students who are anxious to make a more thorough study of them.

THE Technical Education Board of the London County Council will proceed in July next to award five of its valuable Senior County Scholarships. These scholarships, which are reserved as a rule for young men and women under nineteen years of age, are intended to enable promising and deserving students, who would otherwise be unable to afford the expense, to go through a three years' course at a University or at a Technical Institute of University rank. They are limited to those candidates whose parents are in receipt of not more than £400 a year. The scholarships not only give free tuition, but also a money payment of £60 during each of the years that the scholarship is tenable. They are primarily intended to encourage the pursuit of some branch of science, art, or technology, but they may also be awarded for the promotion of studies in modern languages or other branches of education. In making the award, the Board takes mainly into account the record of each candidate's past career and distinctions, and the evidence as to ability, industry, and good character which the candidate is able to supply. At the same time it reserves the right to apply any examination test that it may think fit. Full particulars may be obtained from the Secretary of the Board, at 13 Spring Gardens, S.W. Candidates should send in their names not later than June 29.

THE summer assembly of the National Home-Reading Union will be held at Leamington Spa, from Saturday, June 29, to Monday, July 8. Lectures will be given by Major Leonard Darwin, M.P., on "The National and International Advantages of the Study of Geography"; Sir Robert Ball, on "Comets"; Mr. H. Yule Oldham, on "The Discovery of America"; Mr. J. E. Marr on "The Geology of the District"; Mr. G. F. Scott Elliot, on "Interesting Problems in Botany, suggested by the Flora of the District." There will also be a conference on "The Wider Education," at which the chair will be taken by Dr. Hill, Master of Downing College, Cambridge. Addresses will be given by Miss Mondy, Dr. R. D. Roberts, a representative of the Oxford Delegacy for University Extension, Mr. T.

C. Horsfall, Mr. J. E. Flower (Secretary Recreative Evening Schools Association), and other speakers. Excursions will be made to a number of places in the district, and Profs. W. Ridgeway and T. McKenny Hughes, Mr. J. G. Marr, Mr. Scott Elliot, and others, will accompany the excursions for the purpose of explaining the archæology, geology, and botany of the places visited.

Mr. C. J. FORTH, Mathematical Master at Bolton Grammar School, has been appointed Lecturer in Mathematics at the Plymouth Technical Schools.

THE textile department of the Yorkshire College at Leeds has just been added to by the opening of a museum which is to contain a complete collection of woven samples and models of weaving machinery. The building has cost the Clothworkers' Company £3000, and they will, to the extent of £1200, defray the cost of equipping the museum. The opening ceremony was performed by Mr. Sidney Wilson, Master of the Clothworkers, assisted by Mr. J. E. Horne, his senior warden, and other members and officials. Twenty years ago the Clothworkers established the textile department of the college at the cost of £34,000, and they make an annual grant to it of £2500.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, May 16.—Mr. A. G. Vernon Harcourt, President, in the chair.—The following papers were read:—Kjeldahl's method for the determination of nitrogen, by B. Dyer. The author describes an exhaustive series of experiments made with the various modifications of Kjeldahl's process in order to ascertain their applicability to organic nitrogen compounds of different types.—Note on liquation in crystalline standard gold, by T. K. Rose.—Preparation of the active lactic acids, and the rotation of their metallic salts in solution, by T. Purdie and J. W. Walker. The optical activity of the metallic lactates in aqueous solution is in the opposite sense to that of the active acid from which they are derived; cryoscopic determinations made with the lithium and strontium lactates show that the racemic form is resolved into the two active ones in aqueous solution.—Derivatives of succinyl and phthalyl dithiocarbimides, by A. E. Dixon and R. E. Doran. On heating succinyl or phthalyl chlorides with lead thiocyanate and dry benzene, succinyl or phthalyl dithiocarbimide, respectively, is formed; a number of derivatives of these two substances are described.—The action of nitrous acid on dibromaniline, $C_6H_3BrBr.NH_2$ = 1 : 4 : 2, by R. Meldola and E. R. Andrews. The authors were unsuccessful in preparing a diazoxide from dibromaniline under the conditions which yield these compounds in the naphthalene series; in the present case a diazoamido-derivative, $C_6H_3Br_2.N_2.NH.C_6H_3Br_2$, was obtained.—A new modification of benzilosazone, by H. Ingle and H. H. Mann. The unstable α-benzilosazone, corresponding to the known β isomeride, is obtained, together with dibenzaldiphenylhydrotetrazone by the action of iodine on a mixture of benzalphenylhydrazone and sodium ethoxide.—Affinity of weak bases, by J. Walker and E. Aston.—Substitution derivatives of urea and thiourea, by A. E. Dixon. The properties of a number of substituted ureas are described.—Note on some reactions of ammonium salts, by W. R. E. Hodgkinson and N. E. Bellairs. Fused ammonium nitrate and sulphate are readily attacked by many metals with evolution of ammonia; other products, such as hydrogen and sulphites, also result in certain cases.

Zoological Society, May 21.—Lieut.-Colonel H. H. Godwin-Austen, F.R.S., Vice-President, in the chair.—Dr. R. Bowdler Sharpe gave an account of the ornithological collection made by Dr. Donaldson Smith during his recent expedition into Somaliland and Gallaland. The present series contained about 500 specimens, which were referred to 182 species. Of these twelve were considered to be new to science.—Mr. G. A. Boulenger, F.R.S., read a synopsis of the genera and species of apodal batrachians, and gave a description of a new genus and species proposed to be called *Bdellophis vittatus*.—Lieut.-Colonel H. H. Godwin-Austen, F.R.S., read a list of the land-molluscs of the Andaman and Nicobar groups of islands in the Bay of Bengal, and gave descriptions of some new species, together with a complete account of the distribution of all the species in the various islands of these two groups.—A communication was read from Dr. J. Anderson, F.R.S., containing the description of a new species of hedgehog from Somaliland, which he proposed to name *Erinaceus sclateri*.—A communication from Mr. R. Lydekker contained notes on the structure and habits of the sea-otter (*Latax lutris*).—A communication was read from Dr. B. C. A. Windle containing remarks on some double malformations observed amongst fishes.—Mr. F. E. Beddard, F.R.S., read a paper on the visceral and muscular anatomy of *Cryptoprocta*, dealing chiefly with the brain, alimentary canal, and muscles of this carnivore.

Geological Society, May 22.—Dr. Henry Woodward, F.R.S., President, in the chair.—On a human skull and limb-bones found in the palæolithic terrace-gravels at Galley Hill, Kent, by E. T. Newton, F.R.S. A human skull with lower jaw and parts of the limb-bones were obtained by Mr. R. Elliott from the high-terrace gravels at Galley Hill, in which numerous palæolithic implements have been found. The skull is extremely long and narrow, its breadth-index being about 64, it is hyperdolichocephalic; it is likewise much depressed, having a height-index of about 67. The small extent of the cranium in both height and width shows that it has undergone little or no post-mortem compression, although it has become somewhat twisted in drying. The supraciliary ridges are large, the forehead somewhat receding, the probole prominent, and the occiput flattened below. All the chief sutures are obliterated. Three lower molars and two premolars are in place and are well worn, the three molars being as nearly as possible equal in size. The limb-bones indicate an individual about 5 ft. 1 in. in height. These remains were compared with the fossil human relics which have been found in Britain and on the continent of Europe, as well as with the dolichocephalic races now living, and their relations to the "Spy," "River-bed," "Long-barrow," "Eskimo," and other types were pointed out. The gravels, in which these human bones were found, overlie the chalk at a height of about 90 feet above the Thames, and are about 10 feet thick. They form part of the high-terrace gravels extending from Dartford Heath to Northfleet, and their palæolithic age is shown by the numerous implements which have been found in them, as well as by the mammalian remains which have been met with in similar beds near by, although not at Galley Hill. The human bones were seen *in situ* by Mr. R. Elliott and Mr. Matthew Heys, both of whom speak positively as to the undisturbed condition of the 8 feet of gravel which overlay the bones when discovered.—Geological notes of a journey round the coast of Norway and into Northern Russia, by G. S. Boulger. The author accompanied the Jackson-Harmsworth Polar Expedition as far as Archangel, and returned by way of the River Dvina. His observations relate mainly to four points: the origin of the foliation of the Norwegian gneiss; the question of raised beaches on the north-western coast of Norway; the boulders and boulder-formation of Northern Russia; and the Trias of the Dvina valley. Between Christiansund and Tromsö the author was struck with the wide-sweeping folds of the foliation-planes of the gneissose rocks, which appeared to him more readily explicable on a theory of dynamo-metamorphism of rocks originally in part igneous, than by any process of diagenesis. He noted that the terraces observed in the transverse fjords would be perfectly explained by the formation of ice-dammed lakes, though the terraces of the Gulf of Onega seemed less dubious raised beaches than those of the north west of Norway. He confirmed the views of previous writers that many of the boulders of the boulder-formation of Northern Russia were of Scandinavian origin. The beds on the Dvina consist of sands and loams, often coloured red, with bands of alabaster and anhydrite. The strata are horizontal or inclined at a low angle. North of Ustyug Veliki the strata are marked as Permian on the Russian maps, and those to the south as Trias, but the author saw no perceptible break in the succession.—On some Foraminifera of Rhætic Age, from Wedmore in Somerset, by Frederick Chapman. The author has examined six samples of clays and limestones collected from a quarry south-east of the village of Wedmore, which has yielded Megalosaurian remains. The microscopical details of the various clay-washings were given, and the great abundance of some forms of the acervuline foraminifer *Stacheia* was noticed. In a comparison made with the foraminiferal fauna of the older and younger rocks respectively, the Rhætic fauna shows marked affinities with both the Upper Palæozoic and the Liassic facies. Twenty-six species of foraminifera, chiefly of arenaceous types, were described, nine of which are new forms.

PARIS.

Academy of Sciences, June 4.—M. Lœwy in the chair. Notice on the works of M. Neumann, by M. J. Bertrand. Franz Neumann, correspondent of the Geometry Section, died at Königsberg on May 23 last. He will be chiefly remembered by his great memoir "On the theory of undulations," in which he considers luminous vibrations as occurring in the plane of polarisation. His great mathematical ability was especially shown by the general formulæ in which he expressed the results of Faraday's discoveries and Lenz's rules.—Volume of salts in their aqueous solutions, by M. Lecoq de Boisbaudran. The author compares the dilatometer and pyknometer methods, and describes a special form of dilatometer used in this work.—A contribution to the study of the acetylcyanacetic esters of the general formula,

$$C_nH_{2n+1}.CO.CH\left\langle\begin{matrix}CN\\ CO_2R\end{matrix}\right.$$

or

$$C_nH_{2n+1}.CH:COH.CH\begin{matrix}CN\\ \\ CO_2R\end{matrix},$$

by M. A. Haller.—A projected Swedish exploration of Tierra del Fuego, by M. Daubree. The Swedish Government is about to send out an expedition of three persons to explore the unknown parts of Tierra del Fuego, and the Argentine Government will assist by conveying the members of the exploring party to their destination and finding attendants. MM. Nordenskiöld, Dusen, and Ohlin will arrive at Buenos Ayres in September, and hope, during the Antarctic summer, to explore those parts of the island unvisited by the French expedition of 1882-1883. They aim particularly at gathering material for a comparison of the southern island with Northern Europe; for instance, the quaternary rocks of Tierra del Fuego will be compared with rocks of the same age in the boreal continents.—Report on the project of a balloon expedition to the Polar regions, by M. J. A. Andrée (Committee: MM. Faye, Daubree, Blanchard). It is reported that the conditions for the success of such an expedition have been fully considered, the funds necessary have been raised, and the expedition will set out from Spitzbergen in July of the coming year. The conditions formulated by M. Andrée are: (1) The balloon must have an ascensional power sufficient to carry three persons, all the necessary instruments, food for four months, arms, a boat transformable into a sledge, and the ballast, in all about 3000 kilograms. (2) The balloon must have the quality of impermeability to such an extent that it can remain thirty days in the air. (3) It must be to a certain extent dirigable.—Memoirs presented: By M. A. Lucas, on the centrifugal and centripetal forces and on a new value of *g*; by M. Bonoal, an alcoholimeter allowing the simultaneous estimation of alcohol and extract in wines.—Observations of Charlois' planet BX, made with the Coudé equatorial at Algiers Observatory, by MM. Rambaud and Sy.—On the movement of a plane figure in its plane, by M. A. Pellet.—On a category of groups of substitutions associated with groups of which the order equals the degree, by M. R. Levavasseur.—On the density of helium (a letter from M. Clève to M. Berthelot). (See Notes.)—On the reduction of nitrous oxide by metals in presence of water, by MM. Paul Sabatier and J. B. Senderens. The results fully confirm those formerly obtained. Dissolved nitrous oxide is reduced to the state of nitrogen by magnesium, zinc, iron, and even cadmium, with the simultaneous formation of a little ammonia.—Heat of formation of sodium acetylide, by M. de Forcrand.—On phthalyl chloride and phthalide, by M. Paul Rivals.—Conductibility of some β-ketonic esters, by M. J. Guinchant. The sodium salts of the *cyanomethinic* acids behave quite normally with regard to conductibility. These acids, as well as acetylacetone, obey Ostwald's law (K = const.) as far as can be expected with compounds containing an acid group and an ether function. Their chemical affinity deduced from thermochemical data agrees well with that obtained from their conductibilities. The values of K for homologous acids diminish as the molecular weight increases.—Estimation of volatile acids in wines, by M. E. Burcker.—Considerations on the chemical phenomena of ossification, by M. C. Chabrie.—On the flora of the coal deposits of Asia Minor, and the presence in this flora of the genus *Phyllotheca*, by M. R. Zeiller.—On the chlorosis of American vines and its treatment by sulphuric acid, by MM. Gastine and Degrully. The authors find treatment by ferrous sulphate and by sulphuric acid to yield identical results; it is concluded that the sulphuric acid is the active agent in overcoming chlorosis.—*Oidium albicans*, a general pathogenic agent. Pathogeny of morbid disorders, by MM. Charrin and Ostrowsky. In conclusion, the study of the general disease which determines inoculation by *Oidium albicans* reveals a series of processes peculiar to this fungus. Comparing these processes with those due to bacteria, some analogies, but more differences, are observed.

BERLIN.

Physiological Society, May 3.—Prof. H. Munk, President, in the chair.—After the President had dwelt on the loss physiology had suffered by the death of Prof. Ludwig, Prof. I. Munk spoke on Kjeldahl's method for determining nitrogen in organic substances as compared with Dumas' method. The former has largely supplanted the latter owing to the greater ease with which it may be carried out, but some chemists have found it less accurate than that of Dumas, notably when applied to casein. The speaker had recently repeated the analysis, and found the above statement confirmed as long as he used oxide of copper in Kjeldahl's process. But when he used oxide of mercury (Wilfarth) or potassium bichromate (Krüger), the two methods gave identical results for the nitrogen. He had also found Kjeldahl's method applicable to nitrogenous compounds with closed rings, such as pyridin, chinolin, &c. Prof. Gad developed Fick's hypothesis as to the two-fold nature of the chemical processes taking place in a contracting muscle, a hypothesis to which he gave his support on the basis of his experiments made together with Heymans (see NATURE, vol. xl. p. 288), on the influence of temperature on muscular contraction. He described several experiments on the production of heat in muscles contracting isotonically and isometrically, which can be most readily explained on the basis of Fick's hypothesis of two mutually interfering chemical processes.

May 17.—Prof. H. Munk, President, in the chair.—Dr. W. Cowl spoke on the action of diaphragms in microscopes, aod explained a general improvement he had obtained by applying an iris-diaphragm to the ocular, capable of regulation from the outside.—Dr. Thierfelder gave an account of experiments made with Dr. Nutan on guinea-pigs.

Physical Society, May 10.—Prof. von Bezold, President, in the chair. After election of officers, Prof. Konig spoke on experiments made in conjunction with Dr. Rubens on the distribution of energy in the spectrum of a triplex burner. The methods employed made it possible to measure the energy by means of a bolometer between W.L. 800 μ to W.L. 420 μ, and at the same time to measure the intensity of the light at the same part of the spectrum by means of a Lummer photometer. He dealt in great detail with the correction which is necessary on account of the fact that diffused light acts on the bolometer in addition to that of each giveo wave-length. The curve of energy thus obtained was so steep that it could only be recorded by logarithms; the energy of the extreme red was more than a thousand times as great as that of the blue. By comparing the relative intensities of the rays of a normal amylacetate flame with that of the above burner, the distribution of energy in the amylacetate flame was deduced by calculation, and in this case also the curve was very steep; the energy of the red end being 300 times that of the blue. The curve for the spectrum of the cloudless sky ascended from the red towards the blue end, whereas it was nearly horizontal for the light from a cloud. Prof. Neesen exhibited two automatic mercurial air-pumps.

May 24.—Prof. du Bois Reymond, President, in the chair.—Prof. Neesen described an automatic mercurial valve added to his automatic pumps. Prof. von Bezold spoke on a theory of terrestrial magnetism, based on the construction of the isonomals of terrestrial magnetic potentials. He explained the methods by which he had calculated the isonomals, and discussed the results observable on a chart of the same for the year 1880. The mean values of magnetic potential are simple functions of geographical latitude, and the isonomals have both their poles in the southern hemisphere. The determination of the potential and the construction of the lines of equilibrium is far simpler by Prof. von Bezold's method than by the employment of Gauss's formulæ, and will make it possible to attack a whole series of important problems concerning terrestrial magnetism. As soon as isonomal charts have been constructed for different periods it will be possible to draw conclusions as to the causes of magnetic disturbances.

NEW SOUTH WALES.

Linnean Society, April 24.—The President, Mr. Henry Deane, in the chair.—Description of a fly-catcher, presumably new, by C. W. de Vis. The name *Arses borealis* was proposed for a fly-catcher from Cape York, with the lower surface entirely white in the male, ochreous in a band on the lower throat in the female, and with white lores in both sexes.—On the specific identity of the Peripatus, hitherto supposed to be *P. leuckarti*, Sanger, by J. J. Fletcher. It was shown by a translation of Sanger's paper (in Russian) descriptive of the Australian Peripatus, that *P. insignis*, Dendy, is a synonym of *P. leuckarti*. Various considerations point to the following classification of Australian Peripatus: *Peripatus leuckarti*, Sang. Australian Peripatus with 14 or 15 pairs of walking legs; without or with an accessory tooth at the base of the fang of the outer jaw blade, or with several (three in one case, indications of even more in another). Males with a pair of (accessory genital) pores between the genital papilla and the anus; with a white tubercle on each leg of the first pair only, or of the last pair only, or of all or only some of the pairs with the exception of the first. (1) *P. leuckarti*, Sang., var. *typica* (*P. leuckarti*, Sang.: *P. insignis*, Dendy). With 14 pairs of walking legs; no accessory tooth; New South Wales, Victoria, Tasmania. (2) *P. leuckarti*, Sang., var. *occidentalis*. With 15 pairs of walking legs; no accessory tooth; West Australia (Mr. A. M. Lea). (3) *P. leuckarti*, Sang., var. *orientalis* (*P. leuckarti*, Sang.) With 15 pairs of walking legs; with one or more accessory teeth; viviparous; Queensland, New South Wales. (4) The Victorian Peripatus described by Dr. Dendy as *P. oviparus*. Victoria and Tasmania (probably—for a specimen in the Macleay Museum).—Description of *Peripatus oviparus*, by Dr. A. Dendy. In the light of knowledge gained from the translation of Sanger's description of *P. leuckarti*, already referred to, and the consequent necessary revision of the nomenclature at present in use, the author dealt at length with the larger Victorian Peripatus, which he proposed to call *P. oviparus*.—Notes on the sub-family *Brachyscelinae*, with descriptions of new species, by W. W. Froggatt. This paper comprised notes upon the classification and systematic position of the gall-making Coccids, some corrections in the earlier descriptions of *Brachyscelis Thorntoni*, together with descriptions of three new species proposed to be called *B. dipsaciformis*, *B. sessilis*, and *B. [illegible]iformis*.—On a Fiddler Ray (*Trygonorhina fasciata*) with abnormal pectoral fins, by J. P. Hill. The specimen observed, a young male 20·0 cm. long, presented a striking appearance by reason of the anterior portion of each pectoral fin being separated from the head by a wide and deep notch. The significance of the abnormality was discussed at some length.

AMSTERDAM.

Royal Academy of Sciences, April 18.—Prof. Van de Sande Bakhuyzen in the chair.—Prof. MacGillavry gave a sketch of two methods employed by him to detect the adulteration of butter with less than one per cent. of oleo-margarine or with oils.—Prof. Pekelharing read a paper on the objections raised against his view as to the nature of the fibrine ferment, viz. that it is a compound of nucleoproteid and lime, more particularly on the objections brought forward by Halliburton, who, by his important and extensive investigations, has contributed so much to our knowledge in this department. The author had found (1) that artificial fibrine ferment, prepared by treating nucleoproteid first with lime-water and then with carbonic acid, became only partly [illegible] by being kept under alcohol for a long time, whereas [illegible] treated in the same manner as Schmidt's ferment, it yielded a [illegible]ful fibrino-plastic solution; (2) that magnesium sulphate [illegible] liquid, not for want of nucleoproteid, but because it did not contain enough calcium salts. The magnesium [illegible] prevented the combination of nucleoproteid and lime; but when the combination had once been brought about, $MgSO_4$ [illegible] the coagulation in a much smaller degree. Magnesium [illegible] was coagulated by artificial fibrine just as well as [illegible] from blood serum; (3) that intravenous injection of [illegible] ferment had the same consequence [illegible] of nucleoproteid, viz. Wool[illegible] coagulation of the [illegible] from the vessels. On the other hand, it [illegible] of fibrine ferment, prepared by [illegible] of a rabbit, the animal [illegible]. Prof. Schoute proved that the [illegible] of the regular system in a space of n dimensions is $2^n - 1$.—Prof. Kamerlingh Onnes communicated the results of investigations by Mr. A. Lebret in the Leyden laboratory: (1) compensation method of the observation of Hall's effect; (2) on the dissymmetry of Hall's effect in bismuth when the directions of the magnetic field are opposite to each other. In every plate there are two perpendicular directions of great importance. The primary electrodes being attached in accordance with these directions, there is no dissymmetry. When they are attached in a direction making an angle α with one of them, the Hall effect is given by $H \pm \frac{1}{2}(K_1 - K_2) \sin 2\alpha$. It is explained by a difference between the variations of resistance through magnetisation K_1 and K_2 in two perpendicular directions.

BOOKS AND SERIALS RECEIVED.

BOOKS.—Dairy Bacteriology: Dr. E. von Freudenreich, translated by Prof. J. R. A. Davis (Methuen).—Petrology for Students: A. Harker (Cambridge University Press).—A Text-Book of Zoogeography: F. E. Beddard (Cambridge University Press).—Hydrodynamics: Prof. H. Lamb (Cambridge University Press).—Museums Association. Report of Proceedings, &c., at the Fifth Annual General Meeting, held in Dublin, June 26 to 29, 1894 (Sheffield).—The Horticulturist's Rule-Book: L. H. Bailey, 3rd edition (Macmillan).—Agriculture: R. H. Wallace (Chambers).—Off the Mill: Bishop G. F. Browne (Smith, Elder).—Bibliotheca Geographica, Band 1 (Berlin, Kühl).

SERIALS.—Journal of the Anthropological Institute, May (K. Paul).—Bulletin of the American Mathematical Society, May (New York, Macmillan).—Proceedings of the Physical Society of London, June (Taylor).—Report of the Marlborough College Natural History Society, 1894 (Marlborough).—Journal of the Chemical Society, June (Gurney).—Geological Magazine, June (Dulau).—Phycological Memoirs, Part 3 (Dulau).—Ethnographische Beiträge zur Kenntnis des Karolinen Archipels, 3 Heft (Leiden, Trap).—Natural History of Plants, Part 13; Kerner and Oliver (Blackie).—American Journal of Science, June (New Haven).—Materials for a Flora of the Malayan Peninsula, No. 7: Dr. G. King (Calcutta).—Journal of the Asiatic Society of Bengal, Vol. lxiii. Part 2, No. 4 (Calcutta).—Ditto Vol. lxiv. Part 2, No. 1 (Calcutta).—Science Progress, June (Scientific Press, Ltd.).—Strand Magazine, June (Newnes).—Picture Magazine, June (Newnes).—Engineering Magazine, June (Tucker).

CONTENTS.

THURSDAY, JUNE 20, 1895.

THE ATOMIC THEORY AND ITS AUTHOR.

John Dalton and the Rise of Modern Chemistry. By Sir Henry E. Roscoe, D.C.L., LL.D., F.R.S. Century Science Series. Pp. 212. (London: Cassell and Company, Ltd., 1895.)

WE have read through this little book from beginning to end with a great deal of pleasure. It tells the story of a life which has already been told more than once, but it tells it in a pleasant style, while at the same time it is fairly complete and, what is equally important in these days, not too long.

John Dalton was born at Eaglesfield, near Cockermouth in Cumberland, in 1766, about September 6; but as no register containing a record of his birth has been found, the exact date is not known. John is supposed to have been the second son of his parents, Joseph and Deborah Dalton, but, for the same reason, this statement cannot now be verified. According to his own account he attended the village schools in the neighbourhood, and was fortunate in attracting the notice of Mr. Elihu Robinson, a Quaker like his parents; but while Joseph Dalton was but a humble hand-loom weaver, Robinson was a man of independent means and considerable scientific ability. Under the influence of Mr. Robinson, John made such progress, especially in mathematics, that at the age of twelve he set up school teaching on his own account. When he was about fifteen he left his native place, in order to join his elder brother Jonathan in the conduct of a school at Kendal. Four years later, in 1785, George Bewley, the proprietor of the school retired from the management, and John became his brother's partner. A quaint card, reproduced photographically in the book, announced to their friends and the public that youth would be "carefully instructed in English, Latin, Greek, and French, also writing, arithmetic, merchants' accompts, and the mathematics."

All this time John was diligently occupied in self-improvement. His active mind, however, could not be contented with mere acquisition of knowledge, and we find that his first attempts at scientific investigations were made here. Meteorological observations occupied him in the first instance, and the requisite barometers and thermometers were made with his own hands. This was the beginning of the long series of daily observations which were continued without a break until the evening before his death in 1844.

In 1793 Dalton left Kendal for Manchester, having undertaken for the modest stipend of £80 a year to teach mathematics, mechanics, geometry, book-keeping, natural philosophy, and chemistry, and we are told that in 1794 he had twenty-four students in these subjects. In this position of college tutor Dalton remained six years, and then resigned his post in order to obtain time for his researches, supporting himself by private tuition. When he left the college, he lived first in a house in Faulkner Street, then with John Cockbain, a member of the Society of Friends; but, after a time, joined the family of the Rev. William Johns, with whom he remained nearly thirty years. It was here that his most important original work in physics and chemistry was accomplished, here he brought out his system of chemical philosophy, and here he attained to that celebrity which brought him honours from abroad, as well as the friendship of the most distinguished of his own countrymen.

To the pages of the book we must refer our readers for many of the details of Dalton's subsequent career: how he delivered courses of lectures in Edinburgh and Glasgow (1807), and twice at the Royal Institution in Albemarle Street (1803–4 and 1809–10); how he was made a corresponding member of the French Academy of Sciences (1816), and a Fellow of the Royal Society (1822); how he visited Paris (1822), and subsequently, after the death of Davy, was elected a Foreign Associate of the Academy (1830); how he received honorary degrees from many universities, among the rest, from Oxford (1832); and, finally, was assigned a pension out of the funds of the Civil List by King William the Fourth.

Dalton died on July 27, 1844. Since 1837, when he had a paralytic stroke, his vigour had very seriously declined; and of this decline it is obvious that he was conscious. Old people are usually parsimonious, especially if in their younger days they have been obliged to practise economy. Dalton was no exception to this, and an amusing account, which will not bear condensation, is given of a transaction of his with Dr. Lyon Playfair, in January 1844, only a few months before his death.

Dalton seems to have been a great smoker. In a letter quoted on p. 166, he says (January 10, 1804):

"I was introduced to Mr. Davy, who has rooms adjoining mine in the Royal Institution. He is a very agreeable and intelligent young man, and we have interesting conversations in an evening. The principal failing in his character is that he does not smoke."

Wrapt as he was from early youth in his scientific and philosophical pursuits, it is perhaps not surprising that he should have declared that his head was "too full of triangles, chemical processes, and electrical experiments, &c., to think much of marriage." Nevertheless, it appears that the Quaker philosopher had at least one or two affairs of the heart, and even when past the age of giddy youth he seems to have been accessible to the charm of female beauty: for in a letter in which he describes "the belles of New Bond Street," he admits that he is "more taken up with their faces than their dresses," and ends with the remark, "I do not know how it happens, but I fancy pretty women look well anyhow."

Every one has heard of Dalton's peculiarities of vision. It seems remarkable that he should have grown to manhood without becoming aware of his defect, but it appears that it was not till about the age of six-and-twenty that he found out that his notions of green and red were different from those of other people. This evidently caused him at first a good deal of perplexity, and brought down a certain amount of "chaff," for he writes to his old friend Elihu Robinson, that "the young women tell me they will never suffer me to go into the gallery of the meeting-house with a *green* coat; and I tell them I have no objection to their going in with me in a *crimson* (that is, dark drab) gown." Dalton had a notion that his defect of vision was due to the existence of a coloured medium in one of the humours of the eye. It is almost needless to

say that this was a mistake, and that the fact has now long been recognised that many persons are unable to distinguish red and green, though the true physiological explanation is still unknown.

We must now turn to a brief consideration of the chief subject of Dalton's scientific investigations. In connection with the history of the evolution of the atomic theory, Sir Henry Roscoe has been so fortunate as to make an interesting discovery. Among the "Dalton Papers" in the possession of the Manchester Literary and Philosophical Society, he has found the manuscript notes prepared by Dalton for the course of lectures delivered at the Royal Institution in the winter of 1809–10. In these notes he gives an account of the train of thought which led him to adopt the atomic hypothesis for the explanation of chemical phenomena. Contrary to the commonly received account, which appears to have originated with a statement by Dr. Thomas Thomson in his "History of Chemistry," the atomic theory did not first occur to him during his investigation of olefiant gas and carburetted hydrogen gas. From the newly-discovered manuscript it appears that Dalton's atomistic ideas arose in the course of his study of the atmosphere, and in speculating as to how a mixture of two or more elastic fluids could constitute a homogeneous mass. A reader of his "Chemical Philosophy" would perceive how thoroughly he was imbued with the Newtonian doctrine of particles, and in Henry's "Life" this is clearly pointed out.

By whatever process Dalton arrived at the adoption of the atomic hypothesis, it is certain that his great merit consisted in the application of a commonly accepted (see "Chemical Philosophy," part i. p. 141), but vaguely conceived, notion to the explanation of chemical phenomena. It was "for the development of the chemical theory of definite proportions, usually called the 'Atomic Theory,'" more especially, that he received the first awarded Royal Medal in 1826. This is the point upon which emphasis was particularly placed by the president, Sir Humphry Davy, in presenting the medal.

In the course of reading this little book we have met with only one passage which seems to require correction. The statement (p. 153) that Dalton's "great achievement was that he was the first to introduce the idea of quantity into chemistry" is not only erroneous but is inconsistent with the writer's own text, which on p. 161 contains a reference to the names of Wenzel and Richter.

We shall look forward with pleasure to the other volumes of the series. W. A. T.

HYDRAULIC AND OTHER POWERS.

Hydraulic Motors, Turbines, and Pressure Engines. By G. R. Bodmer, A.M.Inst.C.E. Pp. 540. (London: Whittaker and Co., and George Bell and Sons, 1895.)

Motive Powers and their Practical Selection. By Reginald Bolton, A.M.Inst.C.E. Pp. 250. (London and New York: Longmans, Green, and Co., 1895.)

THE first of these works is a second and enlarged edition of an excellent treatise on a subject seldom dealt with in English text books. The question of the application of water power to useful purposes is becoming more and more of importance, and the study of the design and construction of the necessary machinery naturally follows. Continental engineers are in advance of us in this matter, they having long studied the problem successfully. This difference, however, is rapidly disappearing, and will be greatly assisted by the issue of this work.

The author has consulted to a greater or less extent many works and publications, and appears to have brought together much valuable information; this, combined with his own experience, makes the work an important one. Historical matter has been purposely avoided, as well as descriptions of obsolete forms of motors. The author rather jocularly observes in his preface that he is sure to be criticised, one way or the other, as to the use of mathematics in his work. On the question of the free use of mathematical methods we are entirely of his opinion, viz. that formulæ afford the readiest means of accurately stating facts which in the simplest cases can only be verbally defined in elaborate phraseology. The description of the Niagara Falls installation is concise and to the point. This installation is designed for utilising 10,200 cubic feet of water per second, with an available head of 140 feet, which is equivalent, with an assumed efficiency of turbine of 75 per cent. to about 120,000 horse power. The turbines were designed by Messrs. Faesch and Piccard, of Geneva, and made by the I.P. Morris Company of Philadelphia; each of these wheels is to develop 5000-horse power, with a mean head of 136 feet. Other interesting descriptions of recent installations are added, but we miss an account of the Worcester Electric Lighting Station. This is to be regretted, because the installation is an example of a considerable application of water-power under somewhat adverse conditions. The ground covered in this book, both theoretical and practical, is of considerable extent. The author handles the subject in a sensible manner, and arranges it in such a way that the student can have little difficulty in mastering it. For the engineer who looks for theoretical considerations, there is ample food for reflection. The descriptions of the general theory of various turbines are remarkably clear, and are assisted by diagrams and woodcuts. To those engaged in the design of turbines the volume must be invaluable.

Mr. Bolton's book on "Motive Powers" is of a very different nature, and belongs to that large number of text-books written under the impression that a mere stringing together of "facts, formulæ, and data" is of service to the non-technical reader. The choice of a motor for any particular duty, of course, largely depends on various circumstances, and these must be considered by a qualified engineer. It is questionable whether any amount of study can qualify a non-technical man to make a suitable choice in such a matter. The book, however, contains a large store of information suitable for engineers, and it is arranged in a way that easy reference is possible, which is an important consideration. The author very properly falls foul of the term "nominal horse power," a useless term, and one very likely to mislead. It is quite time that steam and other engines were sold as representing the available power, or "brake horse-power." Under the head of power defined and compared, the author might have been more explicit in his definition of the relation between "the watt" and the "horse-power"; 746 watts are equal to

one *electrical* horse-power. In the chapter on the power of the wind, there is an interesting description of an electrical plant for lighting, which was used in London some time ago, the motive power being a windmill on the top of the building. There appears to be an opening for this type of motor. The author gives rules and tables for their design and construction.

Water-wheels, turbines, and hydraulic motors generally come in for a good deal of notice. The information given concerning these motors is very much condensed, but is in a useful form. "Molesworth's Pocket-book" is quoted for rules for the actual construction of turbines; Bodmer's book can be added with advantage. The question of labour and attendance has to be carefully considered in connection with the adoption of steam-power; a type of motor which, for small powers, is being discarded in favour of oil and gas engines. The steam engine, however, has points in its favour, simplicity of parts being not the least of them. The author gives a table showing relative values for heating purposes of various fuels; this is of value, and may prove of use to many steam users.

Under the heading of liquid fuel, no observations are to be found describing "Holden's System" for burning oil, tar, &c.: this should be added in a future edition. An essential feature of this work is a statement of the probable cost of the machinery described, thus rendering a comparison possible of alternate schemes. The condensation of exhaust steam from engines in large towns is an important question, because in some cases it may become a nuisance. The author describes the usual methods in vogue, but omits to mention the atmospheric condenser used to condense the steam, and so get rid of it. Steam engines of various kinds are fully dealt with, including those suitable for dynamo driving. Under the latter class we find no description of the Willan's central valve engine, probably the most efficient of any. If chapter xxi. is intended to include this engine, why not say so?

The author has much to say on the subject of different types of boilers. On page 179 we find a table giving the pitch of stays in flat surfaces in locomotive fire-boxes. This requires considerable alteration. The pitches given for the higher pressures and ½-inch plates are ridiculous; no locomotive builder exceeds 4¼ inches pitch with copper fire boxes. The usual hydraulic test for boilers is stated to be twice the working pressure. This is so in many cases, and we agree with the author that the boiler is needlessly strained. One and a half times the working pressure is ample, and is quite sufficient to test the workmanship. As to the general essentials for good boiler work, given on page 181, we cordially agree, but would add that machine flanging should, if possible, be done at one heat.

Much has been said of late about the virtues of the tubulous boiler. No doubt its convenience of transport is great, repairs are easily effected, and steam can be rapidly raised. The author gives some interesting data on these boilers, including the Belleville type now being adopted in this country.

Users of small power motors will be interested in chapter xxx. *et seq.* These deal with gas and oil engines, and contain some interesting information. Taken as a whole, this book contains a serviceable collection of data on various subjects. The volume should prove of use to engineers, who will find in it much information relative to motive powers.

N. J. L.

TRAVELS IN TIBET.

Diary of a Journey through Mongolia and Tibet in 1891 *and* 1892. By William Woodville Rockhill. 8vo. Pp. xx. and 414. Illustrations. (Washington: published by the Smithsonian Institution, 1894.)

Mr. Rockhill is no stranger to the British public; his admirable work on Tibet—"The Land of the Lamas," published in 1891—has been widely read, and his second great journey, described in the book now before us, earned for him the gold medal of the Royal Geographical Society, the highest geographical prize in the world. The book, as now published, differs from "The Land of the Lamas" by being less a piece of literature for general reading than a compendious record of observations suited for serious students of Central Asia.

Tibet is peculiar amongst the regions of the world by possessing almost every possible barrier to discourage the would-be explorer. Its physical conditions, lying in the centre of the largest continent, raised, though just without the tropic, into the frigid zone of altitude, composed in large part of rainless arid plains, girdled by the most stupendous mountains of the earth, conspire with the fanatical exclusiveness of its governing body to keep the land in seclusion. There have been fewer travellers in Tibet than in almost any other area of the known world. In his preface Mr. Rockhill recalls the deeds of his predecessors from Friar Oderic in 1325 to the Russian, French, and British travellers of the last decade. The last Europeans to reach the capital city of Lhasa were the Lazarist fathers, Huc and Gabet, in 1846. Since then the Indian native surveyor, Sarat Chandra Das, has succeeded in disguise in making a survey of the town, but every European has been successfully stopped and turned back at the entrance to Lhasa territory. Mr. Rockhill was no more fortunate in evading this fate than his predecessors were, or than his successor, Miss Annie Taylor, has been; but he was fortunate in being able to give an excellent account of the portions of the country which he visited. Mr. Rockhill has the almost unique attainment of knowing both the Chinese and the Tibetan literary languages perfectly; consequently he was able to make his own negotiations with the natives, and to obtain information from them at first hand. It is gratifying to find that one result of his careful study of Tibet is to vindicate the general truthfulness of the Abbé Huc's picturesque description of the country and the people, which is really responsible for such popular knowledge of Tibet as exists in European literature, and on which some recent travellers, misled by bad interpreters, had cast serious doubts.

Mr. Rockhill describes his journey in the form of a diary, a form which throws all the details into equal and somewhat undue prominence, demanding very careful reading, and many references to different passages, before the

general bearing can be understood. A series of appendices containing vocabularies of the Salar, and San-Ch'uan T'u-jen languages, a list of the plants met with, compiled by Mr. W. Botting Hemsley, a table of latitudes and altitudes, and a few meteorological statistics, in some measure makes up for the defects of the diary form. The index, which is all-important in a book of this kind, is unsatisfactory: the entries are numerous enough, but they are not descriptive. The mere facts that snow is referred to on twenty-eight specified pages, and sandstone on forty, does not assist the reader in the way a well-arranged index should. On the other hand, the illustrations are excellent, and leave nothing to be desired, except indeed that they were more numerous.

A map, on the generous scale of thirty-two miles to an inch, gives details of the route, but it is confined to Mr. Rockhill's own surveys, all outside being left blank.

Mr. Rockhill left Pekin in the hope of crossing Tibet from north to south, by a road leading to India, without touching Lhasa territory. He accordingly made his way through Mongolia, passing by Ordos and Alashan, up the valley of the Yellow River to Hsi-ning, and collecting the necessary material for a long desert journey, he left Lusar (Kumbum) on February 17, 1892, passed westward through the marshes of Tsaidam, and at the Naichi Gol, on May 17, turned south-westward with guides who had agreed to take him across the mountains to the Tengri-nor. It was a severe journey: grass for the horses and mules was often scarce; snow fell at midsummer, and herds of wild-yaks and wild-asses were the only living creatures to be seen. The snow-line appeared to be about 17,000 feet, but no glaciers were to be seen on any of the mountains. At length, on July 6, after three days' travelling without food, supporting life only on tea, the party sighted the tents of the Namru Tibetans, about two days' journey from the Tengri-nor. Here there was safety from starvation, but the tribe being under the government of Lhasa, the inevitable result followed. The tribe mustered sixty or eighty armed men, and with the utmost courtesy the head men, reinforced by officials from Lhasa, forbade any advance southward. After much talking, Mr. Rockhill secured the alternative of returning as he came, or going eastward to China *via* Ta-chien-lu, which was reached on October 1. By avoiding the high road, Mr. Rockhill succeeded in surveying a good deal of new country, and he made many most interesting observations on the people, who in south-eastern Tibet are much more liberal and enlightened than in the neighbourhood of Lhasa.

On returning to Shanghai the traveller found that in the eleven months since he had left it he had travelled 8000 miles, of which he had surveyed 3400 miles, and crossed 69 passes, all more than 14,500 feet above the sea. Three hundred photographs were taken, and between three and four hundred ethnological specimens collected. The journey was in fact a great and a successful one, though it led to no sensational discoveries; and we believe that the work of the American traveller from the east will bear the closest comparison with that of the Russian explorers from the north, and the British and Indian surveyors from the south.

Hugh Robert Mill.

MIND AND BODY.

The Philosophy of Mind; an Essay in the Metaphysics of Psychology. By G. T. Ladd, Professor of Philosophy in the Yale University. (Longmans, Green, and Co., 1895.)

PROF. LADD'S latest book opens with two excellent chapters on the connection between psychology and the philosophy of mind, which lead one to hope great things of the rest of the work. It is refreshing to find an author deliver an energetic and effective protest against the "water-tight compartment" theory—that science, and even the science of psychology, can get on without metaphysics—and then turn round and declare in favour of a good healthy realism. It is a psychological fact which is well worth keeping in mind, that we all naturally are, and, even in spite of philosophic training, in our ordinary life remain, dualistic realists. This metaphysical position is implied in all the language of science; so that, in particular, it is well-nigh impossible to interpret the results of psycho-physics in any other sense. His arguments against the view of consciousness as a mere series of passive states, which he attributes to Prof. James, are well worthy of attention, and further great expectations will be raised in the mind of the reader by the heading of the fifth chapter—"The consciousness of identity, and so-called double consciousness." For surely it is time that professed psychologists should give up ignoring the alleged facts of multiple personality and the various phenomena connected with "suggestion" and "hypnotism." Whence are we to learn about the psychological import of these things if not from them? But the expectation is unfortunately doomed to disappointment. After making some show of attacking the question, and expressing a pious belief that "the explanation of double-consciousness, when the facts are ascertained and the explanation is made, will be found in extension rather than reversal of the principles already known to apply to the normal activity of body and mind" (p. 168), he "feels obliged for the present to maintain a position of reserve." He admits, indeed, that if an individual should alternate from one condition to another, between which no actual connection by way of self-consciousness, memory, or thought could be traced (and, presumably, *à fortiori*, if both conditions should co-exist and manifest themthemselves by different channels, *e.g.* by speech and so-called "automatic" writing), we should have a true case of "double Ego." But he goes on to declare that "no such case, so far as the evidence is as yet sifted and understood, has ever occurred." It cannot be supposed that a professor of psychology has never come across the evidence; we can, therefore, only suppose that he relies upon the efficacy of his saving clause; for such cases have certainly been reported in abundance, though it may be that the evidence with respect to them is not yet thoroughly "sifted and understood."

The main thesis of the book, however, is the duality of body and mind; or, at least, the defence of natural dualism against such rival theories as Prof. Ladd conceives to be arrayed against it. It may, however, fairly be doubted whether any materialist, spiritualist, or monist would recognise his own theory among the dummies which Prof. Ladd puts up to knock down again.

He admits, in a note, that it is not likely that any one could be found to espouse the cause of what he calls materialism. The most effective answer he has to give to "monistic spiritualism," that if consistently argued out it would lead to solipsism, applies rather to idealism than to the animism against which the rest of his argument is directed. To his polemic against monism it might be objected, as to that against materialism, that no one would be found to defend the views attacked—at least, surely no one who believed, not only in body and mind, but in a third entity also, which is neither (even if this entity is "unknown and unknowable"), could call himself a *Monist*. Monism, as ordinarily understood, is the view, or hypothesis, that the *Träger* of conscious states is just the brain, and nothing else, and conversely that consciousness is a manifestation or aspect of certain brain activities. No third being is required where not even two are postulated. The rest of the argument against monism is to the effect that the supposed psycho-physical parallelism is not completely proven which may be admitted—and even that in some cases it can be shown not to exist, a point on which Prof. Ladd's arguments hardly seem conclusive. The weakest part of the argument, however, is the implied idea, so common in philosophical discussions, that a metaphysical theory to be accepted ought to be capable of rigid demonstration, instead of being of the nature of an hypothesis postulated to explain the facts of consciousness, which can never be absolutely proved, but may be believed in with greater or less strength of conviction. It is therefore no argument against the monistic hypothesis to say we cannot yet, and probably never will be able to, trace the psycho-physical parallelism everywhere.

The most curious thing in the book remains, however, to be told. In its last pages the author admits not only that "this dualism is not the final word," but that "it must undoubtedly be dissolved in some ultimate monistic solution"! And it must be a little annoying to the monists, whom he has so bitterly attacked, to find that this is a problem which "this treatise hands over to the larger and all-inclusive domain of philosophy."

EDWARD T. DIXON.

OUR BOOK SHELF.

The Story of "Primitive" Man. By Edward Clodd. Pp. 206. (London: George Newnes, Limited, 1895.)

A BOOK such as this forms a useful stepping-stone to higher knowledge; it creates interest, and develops a desire for further information, therefore it possesses the chief qualities that go to make a good book for the average man. For the reader who wishes to know more about the subject than can be compressed in two hundred small pages, a list of books is given at the end of the volume. The illustrations are numerous, but some of these are badly printed. The text is very attractively written, scarcely a sentence being beyond the comprehension of the popular mind. Though the story is briefly told, we have no doubt it will prove interesting to a wide circle of readers. It may be well to point out that the remarks with reference to the chipped flints found in what was believed to be an Upper Miocene deposit in Further India (pp. 23, 24), will need modification when the book comes to a second edition, the bed in which the flints occur having been shown to be Pliocene (see NATURE, vol. li. p. 608).

Britain's Naval Power. By Hamilton Williams. (London: Macmillan, 1894.)

THIS little volume ought to prove very useful to those who wish to know the chief events in the rise of Britain's naval power, without having to plod through details of little consequence. All the great battles are described, and plans of the actions are supplied with them. Celebrated single actions are also mentioned, and although, as the author himself states, some parts require revision and slight corrections, the volume is altogether a light and readable history of the first line of defence, to be commended to every one who desires to know something about naval battles without undertaking a systematic study of the subject.

Portraits berühmter Naturforscher. (Wien und Leipzig: A. Pichler's Witwe and Sohn.)

THE forty-eight portraits which, with short biographical sketches, make up this album, represent well-known men of science of the past and the present. With one or two exceptions, the plates are finely engraved from good portraits. Among our own countrymen in the collection are Darwin, Faraday, Sir William Herschel, Newton, Lord Kelvin (who is given his old and better-known name), and Tyndall.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*]

Discovery of Aboriginal Indian Remains in Jamaica.

THE island of Jamaica, at the time of its discovery by Columbus in 1494, is estimated to have been inhabited by about 600,000 natives, belonging to the race of the Arawâks—a people of simple habits and of a peaceable disposition. The barbarous and cruel treatment of these Indians by their Spanish conquerors, so rapidly decreased their numbers, that in 1655, the date of the conquest of the island by the English, it is probable that not a single specimen of the original type of inhabitant remained alive. Very little was left behind as a record that ever such a race existed here. A few pieces of earthenware showing very primitive ornamentation, and a few flint implements and beads, are practically all that remain to represent their arts and manufactures. Parts of the interior of the country are formed of Miocene limestone, and in this, many caves are to be found. Most of them have, however, yielded little of interest. In one, at Pedro Bluff, the only two aboriginal skulls hitherto known were found. These were submitted to Sir William Flower, and show a frontal compression with corresponding lateral expansion, a deformation produced artifically during infancy by the former inhabitants of the West Indian islands. A kitchen-midden at Northbrook, investigated by Lady Blake, has yielded pieces of ancient pottery, flint implements, shells, and bones of the Jamaica coney, *Capromys brachyurus*, Hill.

Great interest has been aroused in the island within the past few weeks by the discovery of a cave containing the skeletons of at least twenty-four individuals; the ages varying from that of a child with the permanent dentition not yet appearing, to that of aged persons with the teeth-sockets obliterated. Many of the skulls in their depressed frontal region resemble those from Pedro Bluff, and are, no doubt, aboriginal in type. There is, however, considerable variation in the amount of compression. Four of the skulls have been taken to England by Mr. Cundall, the Secretary of the Jamaica Institute, to be submitted to Sir William Flower.

A somewhat shattered canoe, about 7 feet long and 1½ feet wide, made of cedar-wood, was lying above many of the skeletons. An outer portion of the trunk of an *arbor-vitæ*, probably serving at one time as a "mortar," scarcely shows any signs of decay, as a result of the three or four hundred years it may have been in the cave. Among the remains were also obtained the perfect skulls and other parts of the skeleton of two coneys; two large marine shells (*Fusus* and *Murex*), soft parts of which are still eaten by the natives; numerous land shells (*Helix*), and insect remains.

Two small, nearly perfect, earthenware vessels were also found, similar to those known to have been made by the Arawâks. One of these *sappooras* is oval in shape, 7 inches in length and 2 inches high, with a rude handle at each end: the other is round, with a small ledge below the upper margin. Along with these were fragments of pottery belonging to a much larger specimen.

The cave was discovered by the Rev. W. W. Rumsey on the Halberstadt estate belonging to Mr. Gossett. It is in a wild rocky part of the Port Royal Mountains, at a height of about 2000 feet above the sea. The narrow entrance in the face of the hill-side was blocked by boulders of limestone. On removing these, a cavern with waterworn sides, partially covered with stalactitic deposits, was displayed, penetrating into the rock for a distance of about 20 feet, and in some places two or three feet high. The floor is covered with a deposit about 12 inches thick, of a fine, light yellowish dust, but the remains were superficial.

The size of the cave is not such as could possibly contain the whole of the individuals when alive, so that it is probable that it must have been used at one time as a burial-place; while the presence of the canoe, mortar, earthenware, coney bones, marine shells, and a flint implement, is suggestive that some of the people may have lived or fled there for safety, and perhaps been immured by their destroyers, the Spaniards. Whatever may be the explanation of their occurrence, the acquisition of the remains, which have been presented to the Museum, will be a great addition to the archæology of Jamaica.

Museum, Jamaica, May 28. J. E. DUERDEN.

The Antiquity of the Medical Profession.

WITH reference to Mr. H. Spencer's article on the evolution of the medical profession, in the *Contemporary Review* for June, it may be inferred that his remarks should only apply to its historical state in Britain, and not to that in European countries.

It may be pointed out that the profession had existed many centuries before that epoch in the Roman and Grecian nations, as may be seen by any one in looking over Lemprière's Dictionaries.

We have their medical works handed down to us in Celsus (14 A.D.) and Hippocrates (422 B.C.); likewise the Greek army at Troy (1184 B.C.) had military surgeons (Machaon); and Prof. Simpson had discoursed on those in the Roman armies—papers indicated 1856.

See also Dr. Smith's Dictionary, "Greek and Roman Antiquities," for articles on the subjects under:—Art. Medicus, art. Medicina, art. Chirurgia, art. Physiologia.

The art of medicine seems to have been ushered off the stage in the Dark Ages, and to have been consigned to the care of the monasteries and monks for a long period.

It would seem then, from history, that the medical profession is quite as old as either that of theology or law.

Edinburgh, June 17. W. G. BLACK.

A History of British Earthquakes.

ON two or three occasions you have allowed me to ask the readers of NATURE for aid in studying recent British earthquakes, and I have gratefully to acknowledge the valuable assistance which I have thus obtained.

If I might trespass once more upon your space, I should be glad to mention that I am now preparing a history of British earthquakes during the nineteenth century, and would thankfully receive notices of any shocks, either past or future, which your readers may be able and willing to send me. Extracts from provincial newspapers, from private diaries, or from any other trustworthy source, would be most useful.

With a view to aiding in the more careful observation of earthquakes in the future, I have drawn up a short paper of suggestions, and this I shall be happy to send to any one who may wish for it on receipt of his name and address. Those who desire to examine accounts of recent earthquakes in this country, I may refer to the *Proceedings* of the Royal Society for 1894, the *Quarterly Journal* of the Geological Society for 1891, and the *Geological Magazine* for 1891–1893. CHARLES DAVISON.

373, Gillott Road, Birmingham, June 17.

TERMS OF IMPRISONMENT.

IT would have been expected that the various terms of imprisonment awarded by judges should fall into a continuous series. Such, however, is not the case, as is shown by Table I., which is derived from a Parliamentary Blue-book recently published under the title of "Part I.—Criminal Statistics," p. 215. The original has been considerably reduced in size; first, by limiting the extracted data to sentences passed on male prisoners without the option of a fine, and, secondly, by entering the number of sentences to the nearest tenth or hundredth, as stated in the headings to the columns. The material dealt with is thereby more homogeneous than in the original, and its significance is more easily seen. The number of cases is amply sufficient to afford a solid base for broad conclusions, there being in round numbers 830 sentences for various terms of years, 10,540 for various terms of months, and 43,300 for various terms of weeks. The diagram drawn from Table I. gives a still clearer view of the distribution of these sentences:—

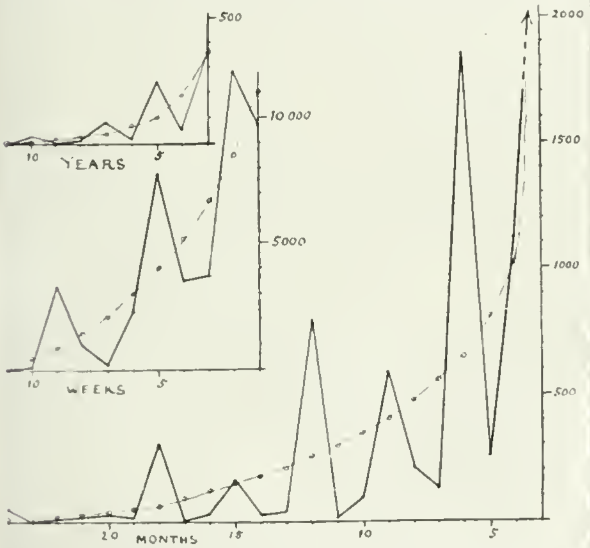
TABLE I. *Distribution of Sentences.*

Length of sentence.	One tenth (to nearest integer) of the number of sentences.		Length of sentence.	One tenth (to nearest integer) of the number of sentences.		Length of sentence.	One tenth (to nearest integer) of the number of sentences.		Length of sentence.	One hundredth (to nearest integer) of the number of sentences.	
Years.	Recorded.	Smoothed.	Months.	Recorded.	Smoothed.	Months.	Recorded.	Smoothed.	Weeks.	Recorded.	Smoothed.
16–	0		24–	5	1	10–	9	34	11–	0	0
15–	1		23–	0	2	9–	59	40	10–	1	5
14–	1		22–	1	2	8–	21	47	9–	33	9
13–	0		21–	2	3	7–	13	56	8–	10	14
12–	1		20–	3	4	6–	185	65	7–	2	21
11–	0	1	19–	2	5	5–	26	81	6–	23	30
10–	3	1	18–	30	6	4–	112	102	5–	77	40
9–	0	2	17–	0	9	3–	480	480	4–	35	52
8–	1	3	16–	3	12				3–	37	67
7–	8	4	15–	16	14				2–	118	85
6–	2	7	14–	3	17				1–	97	110
5–	24	10	13–	4	20						
4–	6	19	12–	79	25						
3–	36	36	11–	1	29						
	83	83		149	149		905	905		433	433

NOTE.—In reading the table, "16–" means "16 and above 15"; "15–" means "15, and above 14"; &c. The number of these intermediate cases being presumably insignificant, they are not noticed in the diagram, where all cases are referred to the upper of their limiting values.

The extreme irregularity of the frequency of the different terms of imprisonment forces itself on the attention. It is impossible to believe that a judicial system acts fairly, which, when it allots only 20 sentences to 6 years imprisonment, allots as many as 240 to 5 years, as few as 60 to 4 years, and as many as 360 to 3 years. Or that, while there are 20 sentences to 19 months, there should be 300 to 18, none to 17, 30 to 16, and 150 to 15. The terms of weeks are distributed just as irregularly. Runs of figures like these testify to some powerful cause of disturbance which interferes with the orderly distribution of punishment in conformity with penal deserts.

On examining the diagram we are struck with the apparent facility of drawing a smooth curve, that shall cut off as much from the hill-tops of the irregular trace as will fill their adjacent valleys. This has been done, by eye, in the diagram, the small circles indicating the smoothed values. Care has been taken that the sums of the ordinates drawn to the smooth curves should be equal to sums of those drawn to the traces, as is shown by the totals in the bottom line of Table I. The smoothed curves may therefore be accepted as an approximate rendering of the general drift of the intentions of the judges as a whole, and show that the sentences passed



by them severally, ought to be made more appropriate to the penal deserts of the prisoners than they are at present. The steep sweeps of the curves afford a strong testimony to the discriminative capacity of the judges, for if their discrimination had been *nil* and the sentences given at random, those steep curves would be replaced by horizontal lines. We have now to discuss the disturbing cause or causes that stand in the way of appropriate sentences.

The terms of imprisonment that are most frequently awarded, fall into rhythmic series. Beginning with the sentences reckoned in months, we see that their maxima of frequency are at 3, 6, 9, 12, 15, and 18 months, which are separated from one another by the uniform interval of 3 months, or a quarter of a year—a round figure that must commend itself to the judge by its simplicity. And we may in consequence be pretty sure that if the year had happened to be divided into 10 periods instead of 12, the exact equivalent of 3 months, which would then have been $2\frac{1}{2}$ periods, would not have been used in its place. If this supposition be correct, the same penal deserts would have been treated differently to what they are now.

Thus the precise position of the maxima has been apparently determined by numerical fancy, and it seems that the irregularity of the trace is mainly due to the award of sentences being usually in terms of the 3-monthly, but sometimes in that of the 1-monthly, series. The trustworthiness of this solution is tested by grouping the entries in sets of three, each set having one of the maxima for its middle member, as shown in Table II. (where, however, the first and last entries are perforce limited to sets of two). The agreement between the recorded and the smoothed entries is now passably good; it would become somewhat closer if the smoothed curve were revised by paying regard to the series of sets of three, thereby taking facts into account that were not utilised before.

TABLE II. (derived from Table I.).

Terms of sentence in months.	Number of sentences.	
	Recorded.	Smoothed.
24 and 23	5	3
22 — 20	6	9
19 — 17	32	20
16 — 14	22	43
13 — 11	84	74
10 — 8	89	121
7 — 5	224	202
4 and 3	592	582
	1054	1054

This solution does not, however, account for all the peculiarities of the irregular trace. For instance, in the original table in the Blue-book, absolutely not a single sentence of 17 months has been recorded, although there are 32 sentences of 16 months, and 340 of 18. I account for the absence of the number 17, by the undoubted fact that almost all persons have a disposition to dwell upon certain numbers, and an indisposition to use others, and that 17 is one of the latter. These curious whimsies become conspicuous whenever calculators, who are not forewarned, are set to record long series of measures, entering them *by estimation* to the nearest decimal of the divisions of the scale they use. Each figure from 0 to 9, in the decimal place, ought then to occur with equal frequency, but they never do; there is always a run upon some figures, while others are hardly, if ever, introduced. The fancies in this respect of different persons differ widely; the biblical Jews, for example, were fond of 40, apparently employing it as a noun of indefinite multitude, but it has no preferential use with us. On the other hand, it is probable that a large and awkward prime number, such as 17, would be generally in disfavour.

As regards the sentences reckoned in years, they range from 3 years upwards (those between 2 and 3 years being here reckoned as 3 years, while those below 2 years are reckoned, as above, in months). The maxima of frequency in this group are at 3, 5, 7, and 10 years, showing a tendency to a unit of 2 years at first, and then, presumably guided by the habit of decimal notation, to jump from 7 to 10. The bias due to decimal notation is forcibly shown by some entries in the original table which fall outside the limits of Table I. It there appears that 7 sentences were awarded for 20 years, and 6 for 15 years, but absolutely none for the 4 intermediate years, 19, 18, 17, 16. It should be added that there were also 8 sentences for 14 and for 12 years respectively. Had these appeared in Table I., they would have been entered to their nearest tenths, that is as 1 in each case, but I did not care to enlarge the table for the sake of including these, comparatively few, additional cases.

The sentences in terms of weeks have their maxima at 2, 5, and 9, for reasons which I do not as yet understand sufficiently to write about.

The general result is that if the judges were to act on uniform rules, the curve of distribution of terms of sentence would be mainly dependent on two sets of causes only, and would become much smoother in consequence. These are: (1) The distribution of true penal deserts; (2) errors of estimation, which would be distributed about each point in the true curve, according to the ordinary law of frequency of error, and with a modulus that might perhaps be determined.

It would be interesting to tabulate the sentences passed by the several judges since their appointments, to discover their respective peculiarities and personal equations, all who exercise extensive jurisdiction in criminal cases being included under the title of judge. We test the acquirements of youths by repeated examinations, but do not as yet employ the methods of statistics to test the performances of professional men. Examiners, for example, should themselves be tested in this way, and I have a fancy that a discussion of the clinical reports at the various large hospitals might enable a cautious statistician to express with some accuracy the curative capacities of different medical men, in numerical terms. Before putting oneself into the hands of any new professional adviser, it would certainly be a grateful help to know the indexes of capacity of those among whom the choice lay, not merely such as might be inferred from their performances in school and undergraduate days, or by their unchecked professional repute, but as they really are in their mature and practical life.

I will conclude by moralising on the large effects upon the durance of a prisoner, that flow from such irrelevant influences as the associations connected with decimal or duodecimal habits and the unconscious favour or disfavour felt for particular numbers. These trifles have been now shown on fairly trustworthy evidence to determine the choice of such widely different sentences as imprisonment for 3 or 5 years, of 5 or 7, and of 7 or 10, for crimes whose penal deserts would otherwise be rated at 4, 6, and 8 or 9 years respectively.

FRANCIS GALTON.

PROFESSOR FRANZ NEUMANN.

As already announced (p. 133) Prof. Neumann, the eminent physicist and mathematician, died on May 23 at Königsberg at the age of ninety-seven. At a recent meeting of the Paris Academy, the Secretary, M. Bertrand, in announcing the loss the Academy had sustained by the death of such a distinguished Correspondent in the Geometry Section, pronounced the following short *éloge* on Prof. Neumann's contributions to knowledge:—

"Franz Neumann, Professor of Physics and Mineralogy at the University of Königsberg, made his *début* in science more than seventy years ago, by some beautiful works on mineralogy. Soon after he directed his studies towards physics, and by an admirable 'Mémoire sur la Théorie des Ondulations,' which was presented to the Berlin Academy in 1835, he took his place among the masters of science. Neumann, like Cauchy, but by very different means, was led to consider luminous vibrations as taking place in the plane of polarisation, while Fresnel thought them perpendicular; he knew how to follow in the most minute details, always in accordance with the observation, the mathematical consequences of his hypothesis. But Fresnel's theory is not contradicted by any of the experiments, so doubt continues, and the ever renewed discussions, whatever their conclusion may be, will remain a noble homage to the man of science and profound physicist who was the first to start them.

"Neumann's memoir on induction showed again the great mathematical skill of its author. In it Neumann translated, by general formulæ, the discoveries of Faraday and Lenz's laws; it is to him that we owe the expression of the potential of a system of two closed currents, of which merely the existence, independently of the very elegant form which he has given it, has played such a great part in science.

"Franz Neumann was a great Professor. Even at the age of ninety he attracted numerous auditors; his lessons, received and written out by learned students, have been studied in all the universities of Europe. The study of physics was his aim; but when he came across a fine mathematical problem, he excelled in interesting his auditors by initiating them occasionally into the highest theories of analysis. It is with justice that in 1863 the Section of Geometry, making amends for a long neglect, elected this illustrious physicist into the Academy."

NOTES.

The annual meeting of the Royal Society for the election of Fellows was held on Thursday last, when the following gentlemen were elected into the Society:—Mr. J. Wolfe Barry, C.B., Prof. A. G. Bourne, Mr. G. H. Bryan, Mr. John Eliot, Prof. J. R. Green, Mr. E. H. Griffiths, Mr. C. T. Heycock, Prof. S. J. Hickson, Major H. C. L. Holden, Dr. Frank McClean, Prof. William MacEwen, Dr. Sidney Martin, Prof. G. M. Minchin, Mr. W. H. Power, Prof. T. Purdie.

Mr. C. C. Harrison has presented a sum of £100,000 to the University of Pennsylvania, in memory of his father, Dr. George Lieb Harrison. The fund is to be known as the "George L. Harrison Foundation for the Encouragement of Liberal Studies and the Advancement of Knowledge." Only the income from the fund can ever be used, and it must be devoted to the establishment of scholarships and fellowships intended solely for men of exceptional ability; to increasing the library of the University, particularly by the acquisition of works of permanent use and of lasting reference to and by the scholar; to the temporary relief from routine work of professors of ability in order that they may devote themselves to some special work; or to securing men of distinction to lecture and for a term to reside at the University.

Science gives the following as the preliminary arrangements for the forty-fourth meeting of the American Association for the Advancement of Science, to be held in Springfield, Mass., from August 28 to September 7, 1895:—At the first general session the President-elect, Prof. E. W. Morley, will be introduced by the retiring President, Prof. D. G. Brinton, who will afterwards give an address on "The Aims of Anthropology." The Presidents of the sections, and the subjects of some of their addresses, are as follows:—Section of Physics: "The Problem of Aerial Locomotion," W. Le Conte Stevens. Section of Anthropology: F. H. Cushing. Section of Geology and Geography: "The Geological Survey of Virginia, 1835–1841—its History and Influence in the Advancement of Geologic Science," Jed. Hotchkiss. Section of Economic Science and Statistics: "The Providential Function of Government in Relation to Natural Resources," B. E. Fernow. Section of Chemistry: McMurtie. Section of Botany: "The Development of Vegetable Physiology," J. C. Arthur. Section of Mechanical Science and Engineering: William Kent. The affiliated societies meeting in conjunction with the Association are:—The Geological Society of America: Prof. N. S. Shaler, President; Prof. H. L. Fairchild, Secretary. Society for Promotion of Agricultural Science: Prof. William Saunders, President; Prof. William Frear, Secretary. Association of Economic Entomologists. Association of State Weather Service: Major H. H. C. Dun-

woody, President; James Berry, Secretary. Society for Promoting Engineering Education: Geo. F. Swain, President; Prof. J. B. Johnson, Secretary. American Chemical Society: Edgar F. Smith, President; Prof. Albert C. Hale, Secretary. American Forestry Association: Hon. J. Sterling Morton, President; F. H. Newell, Secretary. Applications relating to membership and papers should be sent to Prof. F. W. Putnam, Permanent Secretary, Salem, Mass. For all matters relating to local arrangements, hotels, railway rates and certificates, Mr. W. A. Webster, Local Secretary. A. A. A. S., Springfield, Mass., should be addressed.

Mr. R. F. Stupart has succeeded the late Mr. C. Carpmael, as Director of the Meteorological Service of Canada.

The Grocers' Company have renewed the research scholarship held by Mr. Leonard Hill, and have elected Dr. J. Haldane and Prof. Waymouth Reid to the places vacated by Dr. Vaughan Harley and Dr. E. Stirling. The scholarships are each of the value of £250 a year.

At the annual meeting of the London Library, held on Friday last, Mr. Herbert Spencer was elected a vice-president, and Prof. Huxley was elected a member of the committee. A scheme for the reconstruction and extension of the premises, at an estimated cost of £17,000, was discussed and adopted, and it was decided to commence the work when a sum of £5000 has been obtained by means of donations.

The Organising Committee of the International Congress of Applied Chemistry, to be held in Paris next year, met a few days ago to make preliminary arrangements. The Congress will be divided into ten sections, referring respectively to sugar refineries, distilleries and brewing industries, agricultural industries, agricultural chemistry, alimentation and public hygiene, chemical industries, chemical apparatus, metallurgical chemistry, photographic chemistry, and electro-chemistry.

The fifth annual conference of representatives of authorities under the Sea Fisheries Act was held on Friday last, under the presidency of Sir Courtenay Boyle. In the course of a few remarks upon the establishment of hatcheries for sea-fish by committees, or out of Imperial funds, Mr. Bryce pointed out that a great deal had been done by marine laboratories and stations for observation, to determine more fully the habits of the fish, and remarked that only by means of hatcheries, and by prohibiting the taking of undersized fish, was it possible to recreate the diminishing supply of our soles and other flat fish.

We notice with regret that Dr. Valentine Ball, C.B., F.R.S., Director of the National Museum, Dublin, died on Saturday, after a short illness. Dr. Ball was for seventeen years connected with the Geological Survey of India. On the resignation of the chair of Geology in the University of Dublin by Dr. Haughton, he was appointed to it, and twelve years ago he accepted the position which he held at the time of his death. He was the author of several valuable treatises, and while Director of the National Museum, he greatly added to the value of the collections.

Several exhibitions and congresses of scientific interest are noted in the *Board of Trade Journal* as having been lately projected. In connection with the thirteenth International Exhibition to be held at Bordeaux in September next, the Société Philomathique of the town will organise a congress of technical, industrial, and commercial instruction similar to that held in 1886, at which the English Government was officially represented. An international exhibition of articles of food, clothing, hygienic appliances, sport, and inventions of all kinds will be held at the "Parkhaus," Bremen, in the course of this year. It will be open from September 14 to October 6. An international exhibition will also be held in Montreal, Canada, next year. The exhibition will open in May, and close in October. It will be held on the site of the present exhibition grounds and on adjoining land of the Mount Royal Park, embracing altogether about 120 acres. The buildings will be twenty-seven in number, and will be devoted to fine arts, manufactures, and liberal arts, electricity, machinery, fisheries, forestry, horticulture, agriculture, &c. Finally, according to latest advices from Denver, the plans for the holding of a mining and industrial exhibition in that city, in the fall of next year, are being advanced with vigour and success.

The New York State Bridge Commission have approved the plan of Engineer Charles MacDonald for a steel suspension bridge from New Jersey to New York City. The bridge will be 5600 feet long, with a length of 3110 feet between piers; 125 feet wide, with room for six railroad tracks; and 150 feet above mean tidewater. The piers will be 557 feet high, supported by 125 feet of solid masonry. The cost is guaranteed not to exceed 25,000,000 dollars. The bridge will be much the largest suspension bridge ever attempted.

One of the most remarkable features of earthquake-pulsations is their great duration. The originating earthquake may last but a few seconds or minutes, while the ground at a distance may rock gently through a very small angle for several or many hours. Dr. E. Oddone, of the geodynamic observatory at Pavia, has recently contributed an interesting paper on this subject (*Rend. della R. Acc. dei Lincei*, iv., 1895, pp. 425-430). Making use of the records of distant earthquakes during the years 1893-94 by delicate seismometrographs at Rocca di Papa, Rome and Siena, he arrives at the important conclusion that the duration of the pulsations increases with the distance from the epicentre.

Some singular curves showing the distribution of daily wind velocities in the United States, are published by Mr. F. Waldo in the current number of the *American Journal of Science*. The stations chosen range from the Atlantic to the Pacific and Mexican coasts, and include Block Island, New York, Cleveland, San Francisco, San Diego, North Platte, Fort Apache, Salt Lake City, and Roseburgh, among others. The months of January and July are selected as typical months for average daily variation. The daily variations are always greater in summer than in winter, except for Fort Apache, on the great plateau, where the excursions are about equal. At this place the velocities vary from 9·2 to 3·3 miles per hour in January, and from 10·1 to 2·9 miles per hour in July, the maximum in each case taking place at about 4 p.m., and the minimum at 8 a.m. The greatest variation of all is shown by the San Francisco curve for July. About 4 p.m. the wind blows with a speed of some 18 miles per hour, which falls to 7 miles per hour in the forenoon. Tatoosh Island shows a minimum at 2 p.m. in January, but its variations in July are similar to those at Block Island in the Atlantic, which shows the same sequence as the continental stations referred to, but with smaller amplitudes.

Two observations recorded by Mr. W. C. J. Butterfield, in the *Zoologist*, give support to the view that individual female Cuckoos only introduce their eggs into the nests of one particular species of birds, and not indiscriminately into those of any of the birds usually selected as foster-parents. Mr. Butterfield took a Cuckoo's egg from a Wren's nest in the early part of May, and three weeks later found another Wren's nest within a few yards of the former one, also containing a Cuckoo's egg. The two eggs were exactly alike, both as to size, and as to the manner in which the colouring matters and markings were disposed. It is therefore most probable that the eggs were laid by the same bird; for it is well known that a strong family likeness exists between the eggs laid by the same individual, although the eggs of different individuals of the same species

may vary considerably. The observation thus affords another instance of a Cuckoo placing its egg in the nest of a particular species of bird, although there were numerous nests of Hedge-Sparrows and other dupes of the bird in the vicinity, into which the egg could have been put with much less difficulty.

A story to the effect that a new breed of cats had been produced in the cold-storage warehouses of Pittsburg went the rounds of the newspapers some months ago, and was reprinted in most of our scientific contemporaries. It has even found its way into Mr. Lydekker's recent volume on "Cats." A letter received from the Secretary of the Cold Storage Co., and published in the June number of the *American Naturalist*, shows that the story has but a slight foundation in fact. The letter reads as follows:—"While there is some foundation for the newspaper article, it is somewhat exaggerated. Our cold-storage house is separated into rooms of various sizes, varying from 10° to 40° above zero. About a year ago we discovered mice in one of the rooms of the cold-storage house. We removed one of the cats from the general warehouse to the room referred to in the cold-storage house. While there, she had a litter of several kittens; four of these were transferred into one of the general warehouses, leaving three in the cold-storage house. After the kittens were old enough to take care of themselves, we put the old cat back into the house we had taken her from. The change of climate or temperature seemed to affect her almost immediately. She got very weak and languid. We placed her again in the cold-storage room, when she immediately revived. While the feelers of the cats in the cold-storage room are of the usual length, the fur is thick and the cats are larger, stronger, and healthier than the cats in any of the other warehouses." Thus, it is pointed out, the only result of the change of environment was the usual one which ensues on the advent of winter in extra-tropical latitudes generally.

Herr H. Schinz reprints from Engler's *Botanisches Jahrbuch*, vol. xxi., a synopsis of the African Amaranthaceæ, in which a number of new species are described.

The most recent part published (No. 7) of Dr. George King's "Materials for a Flora of the Malayan Peninsula," published in the *Journal* of the Asiatic Society of Bengal, is occupied by the orders *Meliaceæ*, *Olacineæ*, and *Ilicineæ*. A large number of new species are described, and a new genus, *Bracea*, belonging to the Olacineæ.

In an article reprinted from the *Ann. de la Société belge de Microscopie*, M. E. Marchal discusses the microbiological processes which take part in the ripening of soft cheeses, especially those known as "fromage de Herve" and "fromage Casette." While a large number of microbes appear to assist in the process, he states that the essential part is played by the fungus known as *Oospora lactis*, Sacc.

In a previous note (vol. li. p. 540), we have given a brief account of the Vicentini microseismograph erected in the University of Siena. A full description of the instrument, illustrated with three figures, has now been published by the inventor (*Bull. Soc. Veneto-Trentina di Sci. Nat.* vi., 1895), and well deserves the attention of seismologists.

We are glad to observe that the South London Entomological and Natural History Society reports a prosperous condition, in the volume of *Proceedings* for the year 1894. The Society dates back to 1872, and has been a centre of scientific energies ever since its foundation.

The papers read at the fifth annual meeting of the Museums Association, held at Dublin a year ago, have just been published in a report of the proceedings at the meeting. The report, which is edited by Mr. E. Howarth and Mr. H. M. Platnauer, should be in the hands of all curators of museums.

The first number of a bimonthly journal for sanitary engineers will be published at Brussels on August 1, under the title *La Technologie Sanitaire*. It will be under the direction of an editorial committee, the secretary of which is M. Victor J. Van Lint, 115 rue Joseph II., Bruxelles. The journal will deal with all questions relating to public health.

A full abstract of a paper on "The Psychologic Development of Medicine," read by Dr. J. H. McCormick before the Johns Hopkins Hospital Historical Club, on April 8, appears in the *Johns Hopkins Hospital Bulletin*, No. 49. The paper follows almost exactly the same lines as Mr. Herbert Spencer's paper in the current number of the *Contemporary Review*.

The latest addition to the Encyclopédie Scientifique des Aide-Mémoire is "Transmissions par Cables Métalliques," by M. M. H. Léauté and A. Bérard. The transmission of power by metallic cables has given rise to important mathematical developments which are considered in this Aide-Mémoire. The authors confine themselves to the theoretical points which ought to be known to every engineer concerned with cable transmission.

To the series of Economic Classics in course of publication by Messrs. Macmillan, has just been added Thomas Mun's important treatise, "England's Treasure by Forraign Trade," written about 1630, and printed for the first time in 1664. The treatise marks an important period in the history of economic thought, and its author is regarded by political economists as the founder of the mercantile system. In the present reprint of the first edition of the book, the title-page is reproduced in facsimile, and the original spelling and punctuation are followed throughout.

The third part of "Phycological Memoirs," edited by Mr. George Murray, has just been published by Messrs. Dulau and Co. The memoirs are devoted to researches made in the Botanical Department of the British Museum (Natural History), and the present part contains papers on "A New Part of *Pachytheca*," "Calcareous Pebbles formed by Algæ," "The Sori of *Macrocystis* and *Postelsia*," and "A Comparison of the Arctic and Antarctic Marine Floras." Four very fine lithographed plates illustrate the papers.

The colours exhibited by the artificial spectrum-top, described and discussed in these columns some months ago, are shown much more distinctly, and in greater variety, by a "Betts's Chromoscope," sent to us by Messrs. George Philip and Son. The instrument consists of an ingenious whirling table, by means of which heart-shaped pieces of cardboard, having arcs of different thicknesses variously disposed upon them, are put in rotation. A moderate speed of rotation produces a very definite impression of coloured rings, and when some of the more complicated designs are used, secondary tints are clearly seen.

Messrs. J. and A. Churchill have published an eighth edition of the well-known "Bloxam's Chemistry, Inorganic and Organic," rewritten and revised by Prof. J. M. Thomson and Mr. A. G. Bloxam. Several new woodcuts have been added, and some obsolete ones have been omitted. Considerable changes have been made in the arrangement of the subject-matter, and a large portion of the book has been rewritten, while the whole of it has been well revised. The changes all appear to have been in the direction of improvement; hence the book will hold its place as a good text-book and a handy work of reference.

We have received from Dr. L. Palazzo an account of a meteorological station recently attached to the laboratories of the Public Health Department in Rome. The authorities, recognising the important connection between various diseases and atmospheric conditions, have provided the station with a full

set of instruments, and intend to instruct students belonging to the school annexed to the laboratories in their use, and to include, among other studies, a short course of meteorology as applied to hygiene. The results of the observations will be regularly published in a special bulletin, with a view to determining more particularly the medico-climatology of that city.

MISS E. A. ORMEROD has sent us a leaflet referring to the Forest Fly (*Hippobosca equina*, Linn.), a well-known trouble in the New Forest of Hampshire and its neighbourhood. This fly is to be found on various kinds of animals, as horses, donkeys, cattle, dogs, and cats, to all of which its presence in the hair is a severe annoyance. According to general belief, the fly feeds by blood-sucking; it is also said to find nourishment in the perspiration given off by cattle, but further investigation as to how far this occurs is required. The method adopted to prevent the attacks is to wipe the horses over with a cloth moistened with paraffin, or with some dilute sanitary solution.

WE have received a copy of Mr. W. E. Plummer's Report of the Observations made, under his direction, at the Liverpool Observatory, Birkenhead, during 1894. From observations of twenty-two stars, the latitude of the Observatory, for the mean epoch 1894·7, was found to be 54° 24′ 4″·8. A new longitude determination has also been made; exchange of signals with Greenwich Observatory on thirteen nights gave the value 12m. 17·33s. West of Greenwich. The long series of photograph records accumulated at the Observatory, has been used by Mr. Plummer for the derivation of the diurnal inequality of barometric pressure. The results of his investigation are stated in an appendix to the Report, and are clearly exhibited by means of curves representing the diurnal changes of the barometer in each month, and also for the year.

ONE after another, scientific societies are beginning to organise their literature. Quite recently, under the title "Bibliotheca Geographica," the first volume of a geographical bibliography has been published by the Berlin Gesellschaft für Erdkunde. The volume contains the titles of all the geographical publications during 1891 and 1892, classified into subjects, and each section arranged alphabetically according to the author's names. In general geography there are different classes for text-books, historical geography, mathematical and physical geography, biological geography, and anthropological geography (which covers colonisation and the distribution of disease). The classification adopted for purely geographical papers is very elaborate, and the work done in any region during the years covered by the bibliography can be very easily found. It is proposed to issue annual bibliographies similar to the present volume. The editor of the series is Herr Otto Baschin, and the first volume has been prepared with the assistance of Dr. Ernst Wagner.

THE Belgian Society of Geology, Palæontology and Hydrology, aided by Government and other subsidies, has published the first part of an elaborate rainfall investigation of that country, prepared by A. Lancaster, of the Royal Observatory of Brussels. The author is well known to men of science by various valuable publications, and it was entirely due to his efforts that the rainfall service in its present complete form was established in the year 1882. The complete publication will consist of two or three volumes, the first of which contains 224 octavo pages, accompanied by a map drawn by the Military Cartographical Institute, to the 400,000th of the true scale. The number of rainfall stations dealt with is 282, and the monthly sums and means are given for the whole period, together with a series of tables showing the geographical distribution according to catchment basins, and tinted charts showing various annual rainfall zones. The second part will contain various supplementary tables, such as the distribution of rainfall according to seasons, variability of rainfall, &c.; the expense of this part is to be defrayed from the proceeds of the sale of the first part, which is issued at cost price.

FROM the point of view of stereochemistry, the supposed impossibility of preparing optically active halogen compounds from the corresponding active hydroxy-acids has been a serious defect in the strong array of evidence which has compelled the acceptance of van't Hoff's hypothesis of the asymmetric carbon atom. This defect has at last been remedied by P. Walden, who describes a series of active halogen substitution products in the current number of the *Berichte*. Inquiring whether the inactivity of the halogen derivatives prepared by replacement of the hydroxyl group in active compounds by bromine or by chlorine, were due to an inherent quality of the halogen atom, or rather due to the racemisation of the compounds under the conditions hitherto employed in their production, the author undertook the task of examining the methods used in preparing these compounds. Working on the active hydroxy-acids: malic, tartaric, sarcolactic, and mandelic acids, the substitution of chlorine and bromine for hydroxyl was accomplished by means of phosphorus pentachloride and pentabromide respectively Under the conditions detailed by the author, this substitution was carried out without the racemisation which appears hitherto to have always occurred when these halogen derivatives have been prepared. He has shown that (1) dextro-rotatory chlor- or brom-succinic acid may be prepared from the ordinary Levo-rotatory malic acid; (2) lævo-rotatory tartaric acid yields Levo-rotatory derivatives of its esters, containing a halogen atom in place of a hydroxyl group, which retain the optical activity due to the presence of the asymmetric carbon atom; (3) similarly, dextro-rotatory derivatives of α-chloropropionic acid and α-bromopropionic acid can be obtained from the lævo-rotatory sarcolactic acid; and (4) lævo-rotatory mandelic acid (from amygdalin) yields dextro-rotatory phenylchloracetic acid and phenylbromacetic acid. These active compounds have hitherto only been prepared in the racemic form. Their observed inactivity when so prepared was not due to any accidental limitation of the generality of van't Hoff's theory, but only to the racemisation they had undergone during the process of preparation. It is probably quite generally possible to substitute halogen atoms for hydroxyl groups in combination with active asymmetric carbon atoms without destruction of their optical activity. The activity of the compound depends only on the fact of four different atoms or atomic groups being connected with one and the same carbon atom, while the amount and direction of the rotation produced is unquestionably related to the specific nature of these atoms and groups.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*, ♀ ♀) from India, presented respectively by Mr. Charles Roberts and Miss Wieldt; a Leopard (*Felis pardus*, ♀) from India, presented by Mr. Edward Langworthy; a Common Otter (*Lutra vulgaris*, ♂), British, presented by Mr. M. P. Clarke; a Northern Mocking Bird (*Mimus polyglottus*) from North America, presented by Mr. Henry J. Fulljames: a Yellow-throated Sparrow (*Gymnorhinus flavicollis*), a Double-banded Pigeon (*Treron bicincta*), two Chinese Quails (*Coturnix chinensis*), two White-breasted Gallinules (*Gallinula phœnicura*) from India, presented by Mr. Frank Finn; two Weka Rails (*Ocydromus australis*) from New Zealand, presented by Mr. Reginald Moorhouse; two Southern River Hogs (*Potamochœrus africanus*, ♂ ♀) from East Africa, presented by the late Mr. B. Ward; a European Pond Tortoise (*Emys europæa*), European, presented by Miss Laura Bedford; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Jamaica, presented by Lady Blake; a Black-spotted Teguexin (*Tupinambis nigro-punctatus*) from South America, deposited; a Ring-tailed Phalanger (*Pseudo-*

hirus peregrinus) from Australia, two Nicobar Pigeons (*Calænas nicobarica*) from the Indian Archipelago, purchased; a Reticulated Python (*Python reticulata*) from Malacca, received in exchange; a Thar (*Capra jemlaica*, ♀), a Red Deer (*Cervus elaphus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OCCULTATION OF REGULUS.—On June 26 there will be an occultation of Regulus, magnitude 1·5. The disappearance will take place at 8.4 p.m., while the sun is still above the horizon, and the star will reappear at 8.56—that is, about 37 minutes after sunset at Greenwich. The point of disappearance will be at an angle of 147° from the north point towards the east, and of reappearance at 275 reckoned in the same direction. The age of the moon will be a little less than 4 days.

THE RECURRENCE OF ECLIPSES.—A new period of the recurrence of eclipses, which promises to be of great use in the discussion of ancient eclipses, has been investigated by Prof. J. M. Stockwell. (*Astronomical Journal*, No. 346.) He points out that 372 tropical years are very nearly equal to 4601 lunations, and also very nearly equal to twenty revolutions of the moon's node; thus:

372 tropical years	=	135870·10348 days.
4601 lunations	=	135870·23425 ,,
20 revolutions of node	=	135870·700 ,,

During this period, the change of mean longitude of the sun and moon at the time of new moon is $-5°·057$, of the longitude of the moon's perigee $+11°·464$, and of the longitude of the ascending node $+0°·021$. The precession of the equinoxes during 4601 lunations amounts to $5°·1368$, so that the mean longitude of the sun and moon when referred to the movable equinox only changes by $0°·0797$ in a period of 372 years. From this it follows that if an eclipse happened on a given day of the tropical year, there would be another eclipse on the same day of the tropical year 372 years afterwards.

As an example of the application of this new cycle, Prof. Stockwell gives particulars of an inquiry into an eclipse of the sun which is said to have been observed in China on the day of the autumnal equinox during the twenty-second century B.C. According to Oppolzer, an eclipse occurred at the autumnal equinox in the year B.C. 1039, October 3, and going back three periods of 372 years, the year 2155 B.C. is deduced; other eclipses about this time are found by adding multiples of nineteen years to that date. The discussion of the conditions shows that the eclipse which satisfies the tradition occurred on October 10, 2136 B.C.; this would be visible as a partial eclipse over nearly the whole of China. According to a well-known story, the astronomers Ho and Hi were put to death for having failed to predict this eclipse.

VARIABILITY OF NEBULÆ.—One of the best authenticated cases of a variable nebula is that discovered by Hind in 1852 in the constellation Taurus. The nebula was then easily seen in ordinary telescopes, but D'Arrest was quite unable to see it in October 1861, though it was detected shortly after as an exceedingly faint object in the Pulkowa refractor, and in the following year was seen a little brighter with the same telescope. In 1868, however, the nebula was invisible to Struve, but another nebula was discovered 4' preceding. Struve's nebula was subsequently observed by D'Arrest, who testified to its absence in previous observations of the neighbourhood; it was seen also by Tempel in November 1877, but was not visible to him a month later. The interest attaching to this region was revived when, in 1890, Mr. Burnham found that τ Tauri was involved in nebulosity; this was confirmed by Prof. Barnard, who also observed that Hind's nebula was only just visible with the Lick telescope, while Struve's nebula was not perceptible. In a paper recently communicated to the Royal Astronomical Society, Prof. Barnard states that on February 25 of the present year he found Hind's nebula to be an easy object, while Struve's nebula was unseen, and the nebulosity round τ Tauri had practically faded from visibility. Further observations on March 24 showed that Hind's nebula was again scarcely visible, while τ Tauri was distinctly nebulous, and a faint nebula was suspected in the position assigned to Struve's nebula (*Observatory*, June).

It thus appears that there are really three variable nebulæ in this region, and the observations rather suggest that there is a connection between them. In 1890, Prof. Keeler found that the nebulosity round τ Tauri was probably of the bright-line type, but nothing seems to be at present known as to the spectra of Hind's and Struve's nebulæ. On the meteoritic hypothesis, changes in the brightness of nebulæ are due to the interpenetration of nebulous streams and sheets.

THE ZI-KA-WEI OBSERVATORY.—The Zi-ka-wei (or Sicawei) Observatory, near Shanghai, was founded in 1873 by the French Roman Catholic Mission of Kiang-nan, and provided with the instruments necessary for the study of meteorology and terrestrial magnetism. Since that time, excellent service to commerce and to science has been rendered by the Observatory, by the daily publication of weather bulletins, and the issue of a number of important memoirs. Up to the present, however, astronomy has received little attention at Zi-ka-wei. Twelve years ago, the Municipal Council of the French Settlement furnished the Observatory with a small transit instrument for time determinations in connection with the time-ball service then established, but that instrument represents the whole astronomical outfit. Recognising this deficiency, Father Chevalier, the Director of the Observatory, has made an appeal for funds to purchase a good equatorial telescope. The English Settlement at Shanghai has voted a sum of £400 towards the cost of the instrument, and the French Settlement has granted a like amount. The shipping companies at Shanghai have also promised a sum of about £400, so that £1200 may be taken to be already available. But Father Chevalier wishes to have an instrument with an aperture of about twenty inches, and for this the money already subscribed is insufficient. He has therefore appealed to friends of science in France, America, and England for a sum of about £1000 more. If this is contributed, he hopes to have erected a great equatorial, and to accomplish valuable work with it.

THE ROYAL SOCIETY CONVERSAZIONE.

THE rooms of the Royal Society at Burlington House were filled last Wednesday evening, when the annual conversazione to which ladies are admitted took place. Some of the exhibits were shown at the conversazione on May 1, and have already been described in these columns. Following our usual custom, we only give descriptions of new exhibits.

Perhaps the most striking feature of the evening was the telephonic communication with Edinburgh, Glasgow, Belfast, and Dublin, practically shown by the Postmaster-General. The line used is the first link of the great Trunk Telephone System erected by the Post Office, which will eventually place the chief towns in the British Isles in direct communication with each other. The wires to Ireland extend through Leeds and Carlisle to Portpatrick, thence by cable across the North Channel to Donaghadee, and thence to Belfast and Dublin, the distance by this route from London to Dublin being 467 miles. The lines are so carefully laid that it was easy to converse with persons at the places connected by them, without being disturbed by the foreign sounds usually associated with telephonic communications.

An electograph for indelible linen marking was shown by Messrs. Nalder Bros. and Co. The instrument is used as follows: the fabric is damped and a current is passed for about two seconds from a silver die, carrying silver into the fabric wherever the die touches. The current is then reversed for three seconds, which reduces the silver in the fabric; the final result being the same as with ordinary marking-ink, viz. that metallic silver is deposited in the tissue. Plain water can be used, but a salt solution is preferable, as the result is much more quickly obtained.

Models illustrating Lewis and Hunter's patent coal shipping system, as in use at the Bute Docks, Cardiff, were exhibited by the Bute Docks Company. With this system the coal is shipped in very much better condition than with the old systems, and owing to the construction of the carrying-boxes, with a cone valve or bottom, which is only released to let the load out when it is lowered down into the hold of the vessel, within some 18 inches of the flooring of the ship or the cargo, as the case may be, the breakage is greatly reduced. Each crane is capable of loading 300 tons per hour.

Prof. C. V. Boys illustrated the projection of ripples, and showed a logarithmic chart of wave and ripple velocities and frequencies. Ripples produced by tuning-forks are so small, and travel so quickly, as to be invisible unless illuminated either instantaneously or intermittently at the proper rate. They are then visible, and the relations of velocity and frequency can be

illustrated. Both tuning-forks and a mechanical device were employed to produce the ripples. By the use of "scale lines," the logarithmic chart was made more comprehensive than usual. The lines were employed to illustrate the effect of all possible variations of gravity and of surface tension divided by density upon velocities and frequencies of waves and ripples.

Mr. J. Norman Lockyer, C.B., had three exhibits. One was a photograph of apparatus employed for collecting the gases obtained from minerals by the distillation method. A small retort containing the mineral is connected with an end-on spectrum tube joined on to a Sprengel pump. After exhaustion, the mineral is heated to redness, and the spectra of the gases evolved at the various stages, as exhibited by the spectrum tube, are both observed and photographed. The gases are collected in a "steeple" at the foot of the fall tube of the pump, and they can then be observed at atmospheric pressure. The second exhibit consisted of photographs of the spectra of Bellatrix, and of a part of the solar chromosphere, showing coincidences with the lines photographed in the spectra of the gases obtained from uraninite. The photographs showed a close relation of the new gas or gases to solar and stellar phenomena. They appear to point to the *vera causa*, not of two or three, but of many of the lines which so far have been classed as "unknown." The spectrum of Bellatrix was photographed at South Kensington with a 6-inch prism of 45°, and that of the solar chromosphere with the same instrument during the total eclipse of the sun, 1893. Mr. Lockyer also exhibited photographs of the spectra of the new gases. In the preliminary experiments, the new gases have not been separated from the known gases which come over with them, so that the spectra exhibited contained many known lines. The photographs illustrated: (*a*) The presence of the yellow line (D_3) in some instances *with* the blue line 4471, and in others *without* it. (*b*) The presence of the yellow line in some spectra *with* an ultra-violet line at 3889, and in others *without* it.

Dr. A. A. Common exhibited the following silvered glass mirrors: (1) 21-inch convex mirror, 54-inch radius, being the small mirror of an oblique Cassegrain reflecting telescope. (2) 20-inch concave mirror, 90-inch radius, spherical curve. (3) Two 16-inch plane mirrors for heliostats to be used at the 1896 total solar eclipse.

Mr. A. E. Tutton exhibited an instrument for cutting, grinding, and polishing accurately orientated plates and prisms of crystals of every degree of hardness. The instrument combines an accurate reflecting goniometer with a diamond-edged cutting disc and grinding and polishing laps. The adjusting segments of the goniometer are graduated, in order that the crystal may be adjusted so that the desired direction in it can immediately be brought parallel to the cutting disc or grinding lap. Numerous interchangeable laps are provided suitable for all classes of crystals, and the interchange may be effected with great readiness. A counterpoising arrangement is also provided which enables the pressure with which the crystal bears upon the lap to be nicely adjusted, according to the strength of the crystal. The instrument may either be driven by hand or by means of any form of small motor.

Mr. A. P. Trotter showed a model illustrating the relation of volts, amperes, and length of electric arc. The model was made from the diagrams in Prof. Ayrton's paper, read before the Chicago Congress, and described in Mrs. Ayrton's article in *The Electrician*. Drawings of the electric arc were shown by Mrs. Ayrton. The drawings were in sepia, and ten times the full size. They showed the form of the arc produced with a current of 20 amperes between a positive carbon 18 millimetres in diameter, and a negative one 15 millimetres in diameter, when the arc was respectively 4, 7 and 18 millimetres long. From the drawings it could be seen that using a *cored* positive carbon *diminishes* the visible part of the arc, and, when the arc is long, causes the central portion to become gourd-shaped.

The Applied Mathematics Department of University College showed a series of diagrams, calculated and prepared by Miss Alice Lee, to illustrate the time-decay of the field due to a Hertzian oscillator. The late Prof. Hertz prepared four diagrams to illustrate the nature of the field in the neighbourhood of an oscillator giving a stable wave train. His theory requires modification, owing to every Hertzian oscillator really giving a rapidly damped wave train. Miss Lee's diagrams illustrated the changes in the field during 6½ complete oscillations. Four systems of curves gave the points of the field with relative strengths 50, 30, 10 and 1. The decadence of the field was represented not only by the gradual change of shape of the curves, but by the complete disappearance of the curves of greater strength. When the series is complete, it is proposed to reduce it by photography and use it in a "wheel of life," to illustrate the decadence of an oscillator-field.

A curious model for showing the gyroscopic properties of a wheel was exhibited by Mr. Killingworth Hedges. The wheel was represented by a rim, having within it a heavy inner disc which could be made to revolve rapidly on the axis of the wheel. When the wheel was allowed to roll slowly down an inclined plane, and the inner disc was made to revolve in the same direction as the wheel, they both assisted to keep the system upright. When, however, the inner disc revolved in the opposite direction to the wheel, the system was in a state of unstable equilibrium which caused a rapid revolution through 180°, when both the wheel and the inner disc revolved in the same direction, and so produced a state of stable equilibrium.

Four photographic views taken by Mr. W. Bartier, and illustrating the accumulation of ice on the river near the Beckton Gas Works, North Woolwich, in February of this year, were shown by Mr. G. J. Symons.

Photographs of curvilinear crystals of water were exhibited by Dr. Gladstone, F.R.S. The photographs were taken during the severe frost of last February, and showed the forms assumed by the vapour when frozen upon a shop window, and the glass roof of a photographer's studio. All the lines of the crystals were curved. Another exhibit by Dr. Gladstone consisted of a blue photograph showing the way in which a solution of sodium salts mixed with some earthy matter and water may be made to crystallise on evaporation. This specimen showed many spiral forms. It, and the original specimens, were prepared by Mrs. M. Watts Hughes.

Prof. A. G. Greenhill and Mr. T. I. Dewar exhibited an algebraical spherical catenary. By a special choice of the constants, depending upon the quinquisection of the period of the associated elliptic functions, the general equations of the spherical catenary, considered by Clebsch in Crelle, 57 were shown reduced so as to make the projection of the chain on a horizontal plane a closed algebraical curve of the tenth degree.

A number of interesting Japanese pictures, selected to illustrate the effects of time on the pigments used by the old painters of Japan (A.D. 1322 to the early part of the 19th century), were exhibited by Mr. W. Gowland. The chief pigments used in these pictures were as follows:—Greens and blues: carbonates of copper. Permanent blue: the mineral *Lapis lazuli*. Reds: red oxide of iron, vermilion, carmine. Permanent white: levigated oyster-shells. Black: soot prepared from the oil of *Sesamum Indicum*.

Tropical American butterflies, selected to show the existence of common colour-types among species associated in the same areas, were exhibited by Mr. W. F. H. Blandford. The phenomenon (*Homœochromatism*) is observed chiefly among species of the sub-families *Danainæ* and *Heliconiinæ*, but frequently species of other sub-families conform to the prevailing colour-type. To a particular class of cases of colour-resemblance the term "mimicry" has been applied. The series shown comprised:—(1) Species of *Heliconius* associated in pairs, the colour-type varying with the distribution from north to south. (2) Species of different genera (*Tithorea* and *Heliconius*) associated in pairs, and sometimes mimicked by butterflies of other families. (3) Homœochromatic types from various districts represented by numerous species in different families, sub-families and genera.

Minutiæ in finger-prints formed the exhibit of Mr. Francis Galton. The exhibit furnished an illustration of the exceptional trustworthiness of the finger-print method in determining questions of identity. It demonstrated that in a case of twins, whose portraits, classificatory measures, and finger-print formulæ were closely alike, the finger-print minutiæ were quite different. A second exhibit of Mr. Galton's was the print of the hand of a child eighty-six days old. An enlargement of this print showed the development of the ridges at that early age.

Mr. B. Harrison exhibited Eolithic implements from the chalk plateau of Kent. The implements were found by the exhibitor in pits, dug under the auspices of the British Association. Stones were shown which were thought to bear evidence of use as tools, naturally of suitable shapes, but improved upon by chipping round the edges where required.

The Curator of the Maidstone Museum showed a series of nine photographs (with map, ground plan, and section) of a

supposed Roman Mithræum or Mithraic temple discovered on the east bank of the river Medway at Wouldham, near Maidstone. The temple, or "cave," was found by workmen while engaged in removing sand for ballast, and excavated under the superintendence of the exhibitor. It had apparently been built into the bank, standing east and west, measuring 40 feet in length and 20 feet in width. Numerous fragments of tiles, samian and other pottery, animal bones, and a coin of Constantinopolis were found in the *filling*, but no statuary or inscriptions. So far this "cave" is the only one found south of the river Tyne.

Mr. G. F. Scott Elliot had on view photographs and objects illustrating his recent expedition to Ruwenzori. The photographs showed characteristic trees and shrubs of Taru, view of Kagera River, and of Ruwenzori. The objects consisted of Wandorobbo costume, sword, quiver, fire-stick, and arrows; Uganda pottery, bark cloths, &c.; banana meal, &c., in form, ready for export.

Mrs. Ellis Rowan exhibited Australian wild flowers in water-colours. The examples were from Northern Queensland and Western Australia.

A letter and original manuscript of Emin Pasha's last Ornithological Journal formed an interesting exhibit by Sir William H. Flower. The objects were found by the officers of the Congo Free State, after Emin had been murdered by the Arabs at Kinena, on or about October 28, 1892.

A series of cultures of various forms of the bacteria which had been isolated from the river Thames, and then cultivated by the methods employed in the laboratory, formed Prof. Marshall Ward's exhibit. The bacteria were grouped in sections corresponding to the different types, and characterised by differences as to the pigment-production, temperature of growth, capacity of forming spores, behaviour in different media, sizes, shapes, and power of movement, &c. Some of them belonged to common species; others were rare, or unknown, and not classified.

An instrument for describing parabolas by means of a combined sliding and link motion was exhibited by the inventor, Mr. H. Thomson Lyon.

Sir David Salomons showed new forms of "top" slides for the lantern, selenite and hot-water slide heated electrically; and illustrated the behaviour of a glow lamp in the magnetic field, &c.

Mr. F. Enock exhibited a living aquatic hymenopterous insect, *Polynema natans* (Lubbock), *Caraphractus cinctus* (Haliday), described by him in these columns a few weeks ago. This minute and most beautiful Hymenopteron was observed by Sir John Lubbock swimming or flying under water, crawling about weed, &c. The *Mymarida* (Hal.) all oviposit in the eggs of other insects: *Polynema natans*, according to Ganin, having been bred from eggs of a dragon fly, *Æschna*. The smallest of this family, *Camptoptera papaveris*, is but one eighty-fifth of an inch in length.

The bone structure in the dentary bone of *Gomphognathus*, a South African reptile, was illustrated by one of Prof. Seeley's exhibits. The bone structure in this fossil, which is of Permian age, is not distinguishable from the bone structure of a mammal, in the arrangement of the haversian canals and the lacunæ. Prof. Seeley also showed vertical sections through the maxillary and mandibular teeth from the same skull. This exhibit consisted of three vertical sections of the skull of *Gomphognathus* taken at the hinder termination of the hard palate, showing the conical forms of the single roots to the molar teeth, the flat transverse crowns to the teeth, and the way in which the mandibular teeth are opposed to those in the skull.

A sacred bone trumpet, drum, and flute were exhibited by Dr. George Harley. The trumpet and tom-tom drum were from the temple of a Buddhist monastery in Thibet. They were made from the bones of priests, from their being supposed to be more religiously effectual. The trumpet when blown emits a rising and falling mournful wailing sound. The drum, when the knobs attached to its strings are rattled against the skins, gives a disagreeable harsh noise which is thought to drive the evil spirit out of the temple. The flute is a Carib Indian's, from Guiana, made from the tibia of a deer (*Coassus rufinus*). From it can be got the notes 1, 2, and 3, in the natural harmonic ratios of 6, 7, and 8, as in the French flageolet.

The following exhibits, with demonstrations by means of the electric lantern, took place in the meeting room of the Society.

Lantern slides, illustrating the ethnography of British New Guinea, by Prof. A. C. Haddon. The slides illustrated the physical characters of different tribes inhabiting British New Guinea, some of the occupations of the people, several kinds of dances, and the distribution of dance-masks. A series of dwellings from one end of the Protectorate to the other was shown, and two types of canoes. Finally, illustrations of the decorative art of various districts were thrown upon the screen. Evidence was given in support of the view that British New Guinea is inhabited by true dark Papuans, and by two distinct lighter Melanesian peoples, one of whom may have come from the New Hebrides, and the other from the Solomon Islands.

Dr. J. Joly exhibited examples of colour photography, and described his method of obtaining them. The photographs were a realisation of composite heliochromy in a single image. The method of composite heliochromy requires three images superimposed by projection. In Dr. Joly's photographs the colour analysis and synthesis are carried out in the one image. The colours are the natural colours as they registered themselves upon the plate, and in no case altered after reproduction. The specimens shown were first attempts, produced with rough apparatus. The images showed a slightly grained appearance, but this is avoidable with proper appliances. The process of taking and reproducing the photographs differs in no way from ordinary photography upon the dry plate, save that the sensitive plate is exposed in the camera behind a screen lined in particular colours. The positive is subsequently viewed through a screen lined with three other colours; the three "fundamental colours," which upon the three-colour theory of vision are supposed to give rise to all our colour sensations.

ON THE TEMPERATURE VARIATION OF THE THERMAL CONDUCTIVITY OF ROCKS.[1]

§ 1. THE experiments described in this communication were undertaken for the purpose of finding temperature variation of thermal conductivity of some of the more important rocks of the earth's crust.

§ 2. The method which we adopted was to measure, by aid of thermoelectric junctions, the temperatures at different points of a flux line in a solid, kept unequally heated by sources (positive and negative) applied to its surface, and maintained uniform for a sufficiently long time to cause the temperature to be as nearly constant at every point as we could arrange for. The shape of the solid and the thermal sources were arranged to cause the flux lines to be, as nearly as possible, parallel straight lines; so that, according to Fourier's elementary theory and definition of thermal conductivity, we should have

$$\frac{k(\mathrm{M},\ \mathrm{B})}{k(\mathrm{T},\ \mathrm{M})}=\frac{[v(\mathrm{M})-v(\mathrm{T})]\div\mathrm{MT}}{[v(\mathrm{B})-v(\mathrm{M})]\div\mathrm{BM}},$$

where T, M, B denote three points in a stream line (respectively next to the top, at the middle, and next to the bottom in the slabs and columns which we used); $v(\mathrm{T})$, $v(\mathrm{M})$, $v(\mathrm{B})$ denote the steady temperatures at these points; and $k(\mathrm{T},\ \mathrm{M})$, $k(\mathrm{M},\ \mathrm{B})$, the mean conductivities between T and M, and between M and B respectively.

§ 3. The rock experimented on in each case consisted of two equal and similar rectangular pieces, pressed with similar faces together. In one of these faces three straight parallel grooves are cut, just deep enough to allow the thermoelectric wires and junctions to be embedded in them, and no wider than to admit the wires and junctions (see diagram, § 8 below). Thus, when the two pieces of rock are pressed together, and when heat is so applied that the flux lines are parallel to the faces of the two parts, we had the same result, so far as thermal conduction is concerned, as if we had taken a single slab of the same size as the two together, with long fine perforations to receive the electric junctions. The compound slab was placed with the perforations horizontal, and their plane vertical. Its lower side, when thus placed, was immersed under a bath of tin, kept melted by a lamp below it. Its upper side was flooded over with mercury in our later experiments (§§ 6, 7, 8), as in Hopkins' experiments on the thermal conductivity of rock. Heat was carried off from the mercury by a measured quantity of cold water poured upon it once a minute, allowed to remain till the end of a minute, and then drawn off and immediately replaced

[1] A paper by Lord Kelvin, P.R.S., and J. R. Erskine Murray, read at the Royal Society on May 30.

by another equal quantity of cold water. The chief difficulty in respect to steadiness of temperature was the keeping of the gas lamp below the bath of melted tin uniform. If more experiments are to be made on the same plan, whether for rocks or metals, or other solids, it will, no doubt, be advisable to use an automatically regulated gas flame, keeping the temperature of the hot bath in which the lower face of the slab or column is immersed at as nearly constant a temperature as possible, and to arrange for a perfectly steady flow of cold water to carry away heat from the upper surface of the mercury resting on the upper side of the slab or column. It will also be advisable to avoid the complication of having the slab or column in two parts, when the material and the dimensions of the solid allow fine perforations to be bored through it, instead of the grooves which we found more readily made with the appliances available to us.

§ 4. Our first experiments were made on the slate slab, 25 cm. square and 5 cm. thick, in two halves, pressed together, each 25 cm. by 12·5, and 5 cm. thick. One of these parts cracked with a loud noise in an early experiment, with the lower face of the composite square resting on an iron plate heated by a powerful gas burner, and the upper face kept cool by ice in a metal vessel resting upon it. The experiment indicated, very decidedly, less conductivity in the hotter part below the middle than in the cooler part above the middle of the composite square slab. We supposed this might possibly be due to the crack, which we found to be horizontal and below the middle, and to be complete across the whole area of 12½ cm. by 5, across which the heat was conducted in that part of the composite slab, and to give rise to palpably imperfect fitting together of the solid above and below it. We therefore repeated the experiment with the composite slab turned upside down, so as to bring the crack in one half of it now to be above the middle, instead of below the middle, as at first. We still found, for the composite slab, less conductivity in the hot part below the middle than in the cool part above the middle. We inferred that, in respect to thermal conduction through slate across the natural cleavage planes, the thermal conductivity diminishes with increase of temperature.

§ 5. We next tried a composite square slab of sandstone of the same dimensions as the slate, and we found for it also decisive proof of diminution of thermal conductivity with increase of temperature. We were not troubled by any cracking of the sandstone, with its upper side kept cool by an ice-cold metal plate resting on it, and its lower side heated to probably as much as 300° or 400° C.

§ 6. After that we made a composite piece, of two small slate columns, each 3·5 cm. square and 6·2 cm. high, with natural cleavage planes vertical, pressed together with thermoelectric junctions as before; but with appliances (see § 10) for preventing loss or gain of heat across the vertical sides, which the smaller horizontal dimensions (7 cm., 3·5 cm.) might require, but which were manifestly unnecessary with the larger horizontal dimensions (25 cm., 25 cm.) of the slabs of slate and sandstone used in our former experiments. The thermal flux lines in the former experiments on slate were perpendicular to the natural cleavage planes, but now, with the thermal flux lines parallel to the cleavage planes, we still find the same result, smaller thermal conductivity at the higher temperatures. Numerical results will be stated in § 12 below.

§ 7. Our last experiments were made on a composite piece of Aberdeen granite, made up of two columns, each 6 cm. high and 7·6 cm. square, pressed together, with appliances similar to those described in § 6; and, as in all our previous experiments on slate and sandstone, we found less thermal conductivity at higher temperatures. The numerical results are given in § 12.

§ 8. The accompanying diagram (Fig. 1) represents the thermal appliances and thermoelectric arrangement of §§ 6, 7. The columns of slate or granite were placed on supports in a bath of melted tin with about 0·2 cm. of their lower ends immersed. The top of each column was kept cool by mercury, and water changed once a minute, as described in § 3 above, contained in a tank having the top of the stone column for its bottom, and completed by four vertical metal walls fitted into grooves in the stone, and made tight against wet mercury by marine glue.

§ 9. The temperatures v(B), v(M), v(T) of B, M, T, the hot, intermediate, and cool points in the stone, were determined by equalising to them successively the temperature of the mercury thermometer placed in the oil-tank, by aid of thermoelectric circuits and a galvanometer used to test equality of temperature by nullity of current through its coil when placed in the proper circuit, all as shown in the diagram. The steadiness of temperature in the stone was tested by keeping the temperature of the thermometer constant, and observing the galvanometer reading for current when the junction in the oil-tank and one or other of the three junctions in the stone were placed in circuit. We also helped ourselves to attaining constancy of temperature in the stone by observing the current through the galvanometer, due to differences of temperature between any two of the three junctions B, M, T placed in circuit with it.

§ 10. We made many experiments to test what appliances might be necessary to secure against gain or loss of heat by the stone across its vertical faces, and found that *kieselguhr*, loosely packed round the columns and contained by a metal case surrounding them at a distance of 2 cm. or 3 cm., prevented any appreciable disturbance due to this cause. This allowed us to feel sure that the thermal flux lines through the stone were very approximately parallel straight lines on all sides of the central line BMT.

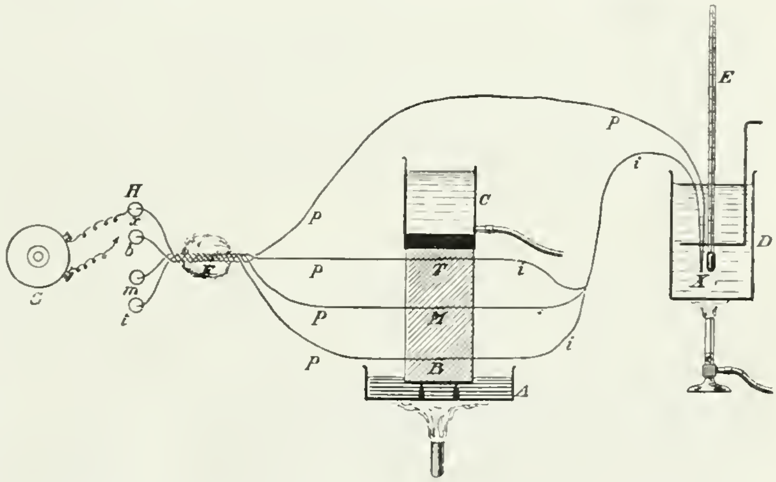


FIG. 1.—Iron wires are marked *i*. Platinoid wires are marked *p*. B, M, T. Thermoelectric junctions in slab. X. Thermoelectric junctions in oil bath. A. Bath of molten tin. C. Tank of cold water. D. Oil bath. E. Thermometer. F. Junctions of platinoid and copper wires. The wires are insulated from one another, and wrapped altogether in cotton wool at this part, to secure equality of temperature between these four junctions, in order that the current through the galvanometer shall depend solely on differences of temperature between whatever two of the four junctions, X, T, M, B, is put in circuit with the galvanometer. G. Galvanometer. H. Four mercury cups, for convenience in connecting the galvanometer to any pair of thermoelectric junctions. *x*, *b*, *m*, *t*, are connected, through copper and platinoid, with X, B, M, T, respectively.

§ 11. The thermometer which we used was one of Casella's (No. 64,168) with Kew certificate (No. 48,471) for temperature from 0° to 100°, and for equality in volume of the divisions above 100°. We standardised it by comparison with the constant volume air thermometer[1] of Dr. Bottomley with the following result. This is satisfactory as showing that when the zero error is corrected the greatest error of the mercury thermometer, which is at 211° C., is only 0·3°.

Reading.		
Air thermometer.	Mercury thermometer.	Correction to be subtracted from reading of mercury thermometer.
°	°	
0	1·9	1·9
120·2	122·2	2·0
166·8	168·6	1·8
211·1	212·7	1·6
265·7	267·5	1·8

[1] *Phil. Mag.*, August 1888, and *Roy. Soc. Edin. Proc.*, January 6, 1888.

§ 12. Each experiment on the slate and granite columns lasted about two hours from the first application of heat and cold: and we generally found that after the first hour we could keep the temperatures of the three junctions very nearly constant. Choosing a time of best constancy in our experiments on each of the two substances, slate and granite, we found the following results:—

Slate: flux lines parallel to cleavage.

$$\tau(T) = 50^\circ{\cdot}2 \text{ C.}$$
$$\tau(M) = 123^\circ{\cdot}3.$$
$$\tau(B) = 202^\circ{\cdot}3.$$

The distances between the junctions were BM = 2·57 cm. and MT = 2·6 cm. Hence by the formula of § 2.

$$\frac{k(M, B)}{k(T, M)} = \frac{73{\cdot}1 \div 2{\cdot}6}{79{\cdot}0 \div 2{\cdot}57} = \frac{28{\cdot}1}{30{\cdot}7} = 0{\cdot}91.$$

Aberdeen granite:

$$\tau(T) = 81^\circ{\cdot}1.$$
$$\tau(M) = 145^\circ{\cdot}6.$$
$$\tau(B) = 214^\circ{\cdot}6.$$

The distances between the junctions were BM = 1·9 cm. and MT = 2·0 cm.

$$\frac{k(MB)}{k(TM)} = \frac{64{\cdot}5 \div 2{\cdot}0}{69{\cdot}0 \div 1{\cdot}9} = \frac{32{\cdot}2}{36{\cdot}3} = 0{\cdot}88.$$

§ 13. Thus we see, that for slate, with lines of flux parallel to cleavage planes, the mean conductivity in the range from 123° C. to 202° C. is 91 per cent. of the mean conductivity in the range from 50° C. to 123° C., and for granite, the mean conductivity in the range from 145° C. to 214° C. is 88 per cent. of the mean conductivity in the range from 81° C. to 145° C. The general plan of apparatus, described above, which we have used only for comparing the conductivities at different temperatures, will, we believe, be found readily applicable to the determination of conductivities in absolute measure.

THE RELATION BETWEEN THE MOVEMENTS OF THE EYES AND THE MOVEMENTS OF THE HEAD.[1]

WE all know that it was a long time before mankind found out that the earth moves. For ages the apparent motion of the heavenly bodies was supposed to be their real motion, the earth being fixed. We, who know something of the truth in this matter, do not, however, any more than our ancestors did, see or feel the earth move. We believe that it does so either because we have been told by some one who, we think, knows about such things, or because we have reasoned the matter out from data observed by ourselves or reported by credible observers. But in habitual thought and speech we go back to the old assumption which, for our practical, terrestrial purposes, answers well enough, and is perfectly in accordance with our sensations.

When we turn from the great Cosmos to the microcosm: when we compare the motion of our own body among the various fixed (terrestrially fixed) and moving bodies around us, with the motion of the earth among the stars, we find quite a different state of matters. It never occurs to us that our own body is at rest, and that the trees, houses, &c., move. When we really move we not only know, but feel and see that we are moving, and every one learned or ignorant, old or young—if only he is sober—feels and sees that the solid earth is fixed, except on the rare occasion of an earthquake, and in the case of some illusions which we shall have to consider. I wish to discuss the cause of this *sensation* of the fixedness of the earth, and also incidentally of the exception implied in the words I have just used, "if only he is sober."

If we keep our head fixed and look at any really fixed scene—say, a room in which there is nothing moving—or a landscape, if we can find one without railway trains, ships, moving beasts, or flying birds, we can allow our eyes to run over it in as uniform or as irregular a way as we please, and see that the scene remains fixed. We might have supposed that, as we move our eyes from right to left the whole scene, like a moving panorama, would seem to move from left to right, but it does not do so. It remains visibly at rest, and we know, without any reasoning about it, that the changes of view were produced by the motion of our eyes.

We fancy that we can move our eyes uniformly, that by a continuous motion like that of a telescope we can move our eyes along the sky-line in the landscape or the cornice of the room, but we are wrong in this. However determinedly we try to do so, what actually happens is, that our eyes move like the seconds hand of a watch, a jerk and a little pause, another jerk and so on; only our eyes are not so regular, the jerks are sometimes of greater, sometimes of less, angular amount, and the pauses vary in duration, although, unless we make an effort, they are always short. During the jerks we practically do not see at all, so that we have before us not a moving panorama, but a series of fixed pictures of the same fixed things, which succeed one another rapidly. It is not difficult to understand how this gives rise to a sensation of the fixedness of the external scene. If, in the otherwise fixed scene, there is a really moving object, we see it move, because during the pauses, short as they are, the moving object has visibly changed its place, and in each of our fixed pictures the moving object is seen to move. If it moves too slowly for this, then we do not *see* it move, but only infer its motion from comparison of its position at different times. If we keep our eyes fixed on the moving object, and this is possible if it does not move too fast or too irregularly, then we see it fixed and the really fixed things moving, an illusion we have all observed when the pier seems to move and the steamer remain at rest.

That the eyes jerk in the way now stated can be made plain by means of a simple experiment. If we have in the field of view a bright object, such as an incandescent electric lamp, and after running our eyes over the scene before us, shut our eyes, we see secondary images of the bright object.[1] Now if the eyes move continuously from one position to another, we should see between the two secondary images of the bright object corresponding to these two positions, a bright band composed of an infinite number of images each infinitely near its two neighbours. But we see no such band, but a finite number of sharp individual images, each of which corresponds to the position of the eyes during a pause between jerks; unless the bright object is very bright, there is nothing in the secondary image to represent the positions of the eyes during the jerk. If for a bright object we take the sun, then we do see bands joining the sharp secondary images. These bands are fainter than the sharp images, and die away sooner. They are the impressions made on the retina by the image of the sun passing rapidly across it during the jerk. But, if with the fixed bright object in the field we follow with our eyes a really moving thing, then on shutting the eyes we see a band of light, because the image of the bright object passed not very rapidly across the retina.

This habit of jerking the eyes from one position of vision to another, as fast as the light, well-poised globes can be swung round by the quick-working, straight-fibred muscles which move them, may be an innate habit, or it may have been acquired by our looking at things and turning quickly from one object of interest to another: at all events, it is now the way in which alone we can move them, unless we fix them on a moving object.

So far I have supposed the head fixed and the eyes alone moving. Let us now attend to what happens when we move our head.[2]

The movement of the head, unless it is very rapid, makes no difference at all in the phenomena just described.

If we call the line along which we look during the pause between the two jerks a glance-line, we may describe the whole phenomenon by saying that the glance-lines are fixed relatively to fixed external objects, whether the head is rotated or not. This, of course, means that, during a pause, the eyes are rotated relatively to the head about the axis about which the head is really rotated, in the opposite sense and through the same angle as the head.

It might, for all that has been yet said, be supposed that this fixedness of the glance-lines, when the head is rotated, depends on the habit of looking at things; but that this is not the cause, or, at all events, not the only cause, is plain from the fact that the same relative movements of the eyes take place when we

[1] Being the Fourth "Robert Boyle" Lecture, delivered before the Oxford University Junior Scientific Club, at the University Museum, Oxford, on May [illegible], by Prof. A. Crum Brown, F.R.S.

[1] The secondary images are better seen if we look at a white surface and wink rapidly.

[2] By "moving the head," I mean moving the head either alone or along with the body or any part of it.

look at an objectless field of view, such as the clear, cloudless sky, or, as was, I believe, first noticed by Dr. Breuer, when the eyes are shut. By placing the fingers lightly over the closed eyelids we can feel the motion of the prominent cornea. If, with eyes shut and fingers so placed on the eyelids, we turn the head or turn head and body round, we feel the eyes twitch. As the head turns round the eyes retain for a little a fixed orientation in respect to external fixed things, and then jerk so as to make up for lost time, again pause, and again jerk, and so on. So that while the head turns uniformly, the eyes, which must, of course, on the whole make one full turn, while the head makes one full turn, do their rotation intermittently, being, so to speak, left behind by the head, and then making up by a rapid jerk.

Another proof that these compensatory movements, as they may be called, of the eyeballs are not, or, at least, not wholly, caused by the effort of looking at things, is afforded by observing what happens when the head is rotated about a fore and aft axis, about an axis coinciding with a glance-line. If we keep our eyes fixed on a particular point and rotate the head about the line along which we look,[1] we still see things fixed, the world does not seem to revolve about our fore and aft axis. Here also we can show by means of secondary images that we see a series of fixed pictures.

If, with a bright object in the field of vision, we fix our eyes and keep them fixed on a point, about 15° distant from the bright object (if we keep both eyes open, about as far from our eyes as the bright object is, so as to avoid double vision), and then rotate the head about a fore and aft axis through, say, 30° by inclining the head towards one shoulder, and shut the eyes after this performance, we see a number of sharp secondary images of the bright object arranged upon an arc of a circle, the radius of which is the angular distance of the bright object from the point fixed.

If I have rotated my head through about 30°, I see about five secondary images, so that what I call the *angle of rotatory nystagmus* is, in my case, about 6°. Here we have been looking all the time at the same point, and it is not easy to suppose that the very slight attention we pay to objects seen indirectly, or, as we sometimes say, "with the tail of the eye," could lead to a habit, so fixed that we cannot escape it, of moving the eyeballs in the way described.

I have said that the movement of the head, *unless it is very rapid*, does not affect the fixedness of the glance-lines. Translatory motion of our body may be so rapid, as in a railway train, that the eyes cannot twitch so fast as to keep the glance-lines fixed relatively to near fixed objects.

The eyes do their best, they twitch but not enough, unless the train is moving slowly, and near objects seem to fly backwards. We succeed with fixed objects at a greater distance from us; we can see them fixed, and all objects between us and such visibly fixed objects are seen to move backwards, fixed things beyond them seem to move forward with us. Of course if, by keeping our attention on our carriage and its contents, our glance-lines become fixed in reference to these really moving things, they seem fixed, and the whole world outside of the carriage is seen to move in the direction opposite to that of our real motion. It is also obvious that rotation of the head, if it is more rapid than the quickest possible rotation of the eyeball in the head, must affect the position of a glance-line, for, in order that the glance-line may remain fixed, the eyeball must rotate in reference to the head as fast in one sense as the head rotates in reference to external things in the other sense; but in the case supposed, the eyeball cannot do so. We can try this experiment without having recourse to mechanical means of rotating our body and head, which, of course, we could do as fast as we please, and a great deal faster than would be either pleasant or safe. The most rapid rotation of our head which we can produce by the direct action of our muscles is what is known as wagging, that is, a rotation about a vertical axis upon the joint between the first two vertebræ. In this way we can give the head an angular velocity considerably greater than the maximum angular velocity of the eyeball. When we do this as fast as we can, we see that external things do not appear steady. When we wag our head to the right we see the world wag to the left, and *vice versa*. But the external really fixed things do not appear to us to describe nearly so large an angle as the head really does, the eyes make an effort to compensate the rotation of the head, an effort only partially successful, the angle through which external things seem to move being the difference between the actual angular rate of movement of the head, and the maximum possible angular rate of movement of the eyeball in its socket. This difference can best be observed and, indeed, can be approximately measured by observing a distant light on a dark night, while we wag the head. The point of light seems drawn out into a horizontal line of light, the apparent length of which is the angular difference in question. As we can wag our head much faster than we can nod it, the apparent length of the vertical line of light into which a bright point is drawn out when we look at it and nod as rapidly as we can, is much less than that of the horizontal line of light just spoken of: but I find that I can, by nodding, rotate my head about a right and left axis a little faster than I can rotate my eyes about the same axis, so that the luminous point does appear drawn out into a short vertical line.

Such violent movements of the head occur sometimes in our ordinary (not experimental) use of our eyes, but they are rare and isolated, so that the disturbance of the fixedness of the glance-lines which they cause does not really affect our sense of the fixedness of the world. The illusion of the moving pier and fixed steamer, which we have all also observed when there is a train alongside that in which we happen to be, and we see the moving train fixed and the fixed train moving, is corrected by looking at the shore or the railway station. For a moment these also seem to move, but our glance-lines almost instantly become fixed in reference to these things which we know are fixed, and it is then difficult to recall the illusion. Another similar case is that of the moon and the clouds. We sometimes see the moon moving and the clouds fixed, sometimes the clouds moving and the moon fixed, as our glance-lines are fixed relatively to the clouds or to the moon, and a little practice enables us to change from the one sensation to the other at will.

What has been said seems to show that our immediate sense that the earth and what we call fixed objects on it are fixed is a consequence of the way in which we move our eyes, and, in particular, of the way in which, by a suitable movement of the eyeballs, we involuntarily and unconsciously compensate movements of the head, voluntary or involuntary, conscious or unconscious.[1]

That such an immediate sense of the fixedness of external fixed things is of great use to us in moving about among them is plainly shown when we observe the trouble which a drunken man, who has lost this sense, has in guiding himself.

I now turn to the question. What is the cause of this prompt and wonderfully accurate compensatory movement of the eyeballs?

There are three sources from which we can obtain information leading to an answer. (1) Experiments on ourselves, (2) anatomical observations and measurements, and (3) observations of the effects of injuries to the labyrinth of the internal ear.

I shall consider these in their order.

By experiments on ourselves I mean the study of the effect on the motion of the eyes and on our sense of the fixedness of external things, of movements of our head (in this case, always along with the rest of our body) which we do not make, as a rule, for any other purpose.

I have already stated that if we shut our eyes, place our fingers on the eyelids, and turn round about a vertical axis, we feel with our fingers the jerking motion of the eyeballs. If instead of turning once round, we turn round several times, still better if we seat ourselves on a turning-table and get some one else to turn it and us round at a uniform rate, we find that the jerks become less and less frequent, and after two or three turns cease altogether. Another thing which we observe is, that although the turn-table is being turned round at a perfectly uniform rate, we feel the rotation becoming slower and slower, and when the jerks of the eyeballs have quite ceased we feel ourselves at rest, and have no sensation of rotation. Let us for convenience call the sense in which the rotation is still going on positive. This uniform positive rotation has become to us imperceptible (as long as we keep our head in the same position in respect to the vertical), and is what we may call a new zero of rotation. If the rate of rotation is now increased, we feel this increase as a positive rotation; if it is diminished, we feel the diminution as a negative

[1] If we take a sufficiently distant object as the thing to be looked at, we may neglect the want of coincidence of the two glance-lines belonging to the two eyes, and, moreover, all that is here described is seen as well, though not so conveniently with one eye shut.

[1] I need hardly repeat that, by movements of the head, I mean movements of the head whether accompanied or not by movements of the body.

rotation—a rotation the other way about. What we really perceive then is *acceleration* of rotation, using the word acceleration in its technical sense. If the turn-table is stopped, this is a negative acceleration, and what we feel is that we are being turned round in a negative sense, and at the same time we feel our eyeballs jerk. The sense of rotation and the jerking die away in this as in the former case.

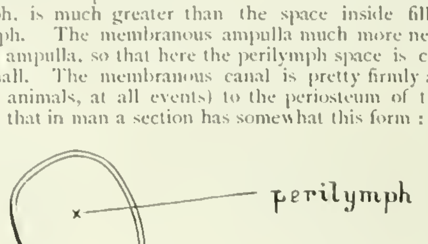
If, while we are being turned round with uniform angular velocity, but after all sense of rotation and all jerking of the eyeballs have ceased, we open our eyes, we still feel ourselves quite at rest, but we see all external objects turning round us; as has been well said by Prof. Mach, the external world seems to turn round inside an outer unseen fixed world. It is in reference to this imaginary fixed world that our glance-lines are now fixed. If the rate of rotation is changed while the eyes are open, the sensation of rotation is exactly the same as if they were shut, we feel the acceleration—positive or negative—as a rotation in the one or in the other sense, and the jerks of the eyeballs take place as if the real external world were not there, and we were looking beyond it at the unseen fixed world outside of it, that imaginary world in reference to which our glance-lines are now fixed.

If while the experiment I have described is going on, we move so as to change the direction, in our head, of the axis of rotation—for instance, if, after uniform rotation about a vertical axis has gone on, with the head in its usual upright position, until the sense of rotation has ceased, we bow our head forwards so that the axis of rotation is now parallel to a line from the occiput to the chin, a very striking, and somewhat alarming, but most instructive sensation is experienced. What we feel is that we are being turned round with a rotation which is the resultant of two rotations of equal angular velocity—one the real rotation about what is now the vertical, the other the imaginary (but equally perceived) rotation in the opposite sense about the line in the head which was vertical. If the angular movement of the head is small, so that the angle between what is the vertical and what was the vertical is small, then the two component rotations nearly neutralise one another, and the strange and alarming resultant is slight; but if the head is bent so that the old and new verticals are at right angles to one another, the real and the imaginary components are both felt in full, and the effect is very startling. If the rate of rotation is changed simultaneously with the change of position of the head, we have a resultant of two rotations of different angular velocity. The most easily observed case of this kind is when the rotation is stopped altogether at the moment of change of position of the head. Here the real component is zero, and we have only the imaginary one. This is the case of the well-known practical joke: a man is asked to plant the poker before him on the floor, place his forehead on the end of it, walk round it three times, and then rise and walk to the door. The preliminary part of this experiment presents no difficulty: the victim plants the poker, puts his forehead on it, walks round it with the greatest ease and with no sense of anything unusual. But when he rises, the line in his head which was vertical is now horizontal, and he feels himself turned round about that horizontal line. The external world he also sees turning round this line, objects on the one side rising up and objects on the other side sinking down. In this visibly swaying world he has to guide his sensibly rotating body, and if his friends do not catch hold of him he is pretty sure to fall. All these experiments are most conveniently made on a smoothly working turn-table of such a size that one can comfortably lie down upon it. By the kindness of Messrs. Dove, lighthouse engineers, I had the use of a large turn-table made for the revolving lantern of a lighthouse. It could be turned round smoothly and uniformly, at the moderate speed that is most suitable for experiments of the kind in question. A few experiments with such an apparatus will convince any one that we have here to do with a perfectly definite sense, and not with any vague sensations caused by the inertia of the soft parts of the body.

This is one of the ways in which the phenomena have been explained by those who hesitate to believe that there can be a definite special sense only discovered within the last few years. That the origin of the sensation is not in the soft parts of the body generally, but in the head, is made perfectly plain by the fact that the position of the head and the changes of that position alone determine the sensations. We must therefore look in the head for the organ of this sense.

In close proximity to the cochlea, which is universally regarded as the organ of hearing, there is an organ of very striking, and we might say mysterious, form. It occurs in all vertebrates, and occurs in them fully developed, except in the lowest forms of fish. It is contained in a bony or cartilaginous cavity, which communicates with the cochlea or lagena. This cavity may be divided into the vestibule and the three semicircular canals. The canals open at both ends into the vestibule, and each has at one end an enlargement called the ampulla.[1] Within this bony case is contained a membranous structure, consisting of the utricle, situated in the vestibule, and three membranous canals, each in one of the bony canals, each with an ampulla in the bony ampulla, and each opening at both ends into the utricle. The vestibule contains, besides the utricle, the saccule, a membranous bag continuous with the cochlear duct, and has in the side next the tympanic cavity a hole in the bony wall filled in by a membrane, and known as the *fenestra ovalis*. The saccule and the utricle have each a spot on the lower wall supplied with nerves ending in hair-cells, and known as the *macula acustica*.

The *maculæ acusticæ* are probably, as suggested by Mach and Breuer, organs fitted to perceive acceleration of translatory motion, and are not connected directly with the function of the semicircular canals. The *fenestra ovalis* belongs to the organ of hearing, which may thus be said to have a right of way through the vestibule. We need not therefore here consider any further these organs, but confine ourselves to the semicircular canals and the utricle in its relation to them. As already stated, each bony canal contains a membranous canal. The membranous canal is, except at the ampulla, much smaller in bore than the bony canal, so that the space outside the membranous canal filled with perilymph, is much greater than the space inside filled with endolymph. The membranous ampulla much more nearly fills the bony ampulla, so that here the perilymph space is comparatively small. The membranous canal is pretty firmly attached (in some animals, at all events) to the periosteum of the bony canal, so that in man a section has somewhat this form:



Each canal is, in all animals I have examined, approximately in a plane, and it is important to consider the relations of these planes to one another and to the mesial plane of the head.

As I have brought part of the apparatus with me, I may shortly describe the method I used to measure the angles which these planes make with one another, and also an improved method, of which I have not yet had time to make any very full trial.

[The method illustrated by the human skull shown is fully described, with woodcuts from photographs, in Prof. McKendrick's "Text-book of Physiology," vol. ii. pp. 697–699, and therefore need not be reprinted here. The other method will, I hope, give more accurate measurements.]

It consists in attaching the preparation—either a cast of the canals, or, in the case of a bird, the dissected and cleaned bony canals—to one arm of a branched rod, and a lump of wax to the other. The rod is then fixed to the large apparatus already referred to. The canals are successively made horizontal, and a small plate of glass fixed horizontally in each case—parallel therefore to each canal—to the lump of wax. We can also attach a glass plate parallel to the mesial plane. We can thus have, on a comparatively small piece of wax, glass plates parallel to all the planes, the relations of which to one another are to be measured. The lump of wax is then removed from the rod, and the angles between the planes of the glass plates measured by means of an ordinary reflexion goniometer.

The general results are:

(1) The canals do not lie rigorously in planes, but sufficiently nearly so to give closely accordant results.

(2) The external canals are very nearly at right angles to the mesial plane, and therefore, from the bilateral symmetry, the two external canals are very nearly in one plane.

(3) The superior and posterior canals of the same side make

[1] In all animals the non-ampullary ends of the superior and the posterior canal have a common opening into the vestibule.

approximately equal angles with the mesial plane. In all cases which I have examined, the angle between the posterior canal and the mesial plane is somewhat larger than that between the superior canal and the mesial plane.

From the bilateral symmetry, therefore, the superior canal of the one side is nearly, but not quite, parallel to the posterior canal of the other side. In the discussion of the way in which the system of canals may be supposed to act, I shall for convenience assume that these canals are parallel, as the deviation from exact parallelism only complicates, but does not at all vitiate, the argument.

(4) In man, and in a large number of other animals, the three canals are very nearly at right angles to one another. But, in a good many of the animals I have looked at, the superior and posterior canals make with one another an angle considerably greater than a right angle.

Looking at the six canals as forming one system, we see that we have three axes, that at right angles to each axis there are two canals, one in the one internal ear, the other in the other; these two canals having their ampullæ at opposite ends, so that if rotation takes place about the axis, the ampulla in the one case precedes the canal, in the other follows it. The vertical axis, as we may call that at right angles to the two external (or horizontal) canals, is pretty nearly vertical in most animals, in the usual position of the head when the animal looks to the horizon; in man it is not exactly so, we must bow our head a little to make this axis vertical. If we suppose we are looking north, the other two axes are north-east and south-west and north-west and south-east respectively. In man they pass from the eye of one side to the mastoid process of the other side, and are nearly at right angles to one another. As already stated, in some animals they are inclined and are nearer the right and left than the fore and aft line in the head.

In order to see how such a system can work as a hydrodynamical instrument, let us first consider one canal.

Here we have two watery liquids, the endolymph within the membranous canal, its ampulla and the utricle, the perilymph between these and the bony case. How will these behave when rotation takes place about an axis normal to the plane of the canal? The inertia of the liquids will tend to produce a flow through the canal in the sense opposite to that of the rotation.

Let the rotation take place so that the ampulla precedes the canal. Here the endolymph will tend to flow from the utricle into the ampulla, and thence through the canal to the utricle again. But, as Mach has pointed out, the canal has too small a bore to allow of any sensible flow through it, so that the effect of this rotation will be to increase the pressure within the membranous ampulla. But (and this is a point to which, as far as I know, no one has hitherto called attention) as there will also be a tendency of the perilymph to circulate, so in its circle there is also a narrow place, namely at the ampulla; for as the membranous ampulla nearly fills its bony case, there is not much room there for the perilymph to pass from the vestibule into the space surrounding the membranous canal. There will, therefore, be a diminution of pressure of perilymph at the ampullary end of the canal, so that the ampullary walls will be stretched by the increase of pressure within and the diminution of pressure without. Of course when the rotation is kept up uniformly for some time the pressure inside and outside of the membranous ampulla is soon equalised, and the stretching or relaxation ceases. With the cessation of the stretching the sensation must also cease.

If now the rotation is stopped the perilymph and endolymph will tend to move on, and pressure will be produced inside the membranous ampulla of that canal, which during the rotation moved with ampulla following the canal.

All this will of course be reversed when the rotation takes place with the ampulla following the canal; the pressure inside the membranous ampulla will be diminished, that without increased, and the walls will become flaccid.

In each membranous ampulla there is a so-called *crista acustica* where nerves terminate in hair-cells, and it is not difficult to suppose that stretching of the ampullary walls will irritate these nerve-endings, while a relaxation of the ampullary walls will produce no irritation. If this be so, then we have three axes each with an organ sensitive to rotation about it in either sense, and capable of discriminating between the two; and as every rotation of the head can be resolved into component rotations about these three axes, we have the means of perceiving the axis and what we may call the intensity of the rotation, or perhaps more correctly the rotational acceleration.

This hydrokinetic theory of the function of the semicircular canals was propounded at very nearly the same time by Prof. Mach of Prague, Dr. Breuer of Vienna, and myself. I give the names in the order of publication. The views expressed by us were not exactly the same, and the statement of the theory I have just given is any one of them with additions and corrections from the other two.

I have not thought it necessary to refer to the hydrostatic theory of Goltz, or, indeed, to give any details of the literature of the subject. A very full and accurate digest of almost every thing that has been written on the functions of the several parts of the labyrinth of the ear has been published in Russian by Dr. Stanislaus von Stein, and translated into German by Dr. C. von Krzywicki.

The theory as I have just described it might perhaps have been developed, as I have here developed it, from a consideration of the structure and position of the canals. But, as a matter of fact, this was not the historical order. It was the experiments of Flourens that first directed attention to these organs as having something to do with the equilibrium of the body.

In reference to these experiments and those made since by many able physiologists and skilled operators, I shall only say that the results seem to me to be consistent with the hydrokinetic theory. Certain of de Cyon's experiments, in which he increased the pressure in the canals by inserting in them small tangle plugs without producing any nystagmus or rotatory movements of the head, appear to contradict the theory. But increase of pressure in the bony canal can have no tendency to stretch the walls of the membranous ampulla, and therefore could not be expected, if the theory as I have stated it is correct, to produce a sensation of rotation; what is required, is that the pressure inside the membranous ampulla should be greater than that outside of it.

The symptoms observed in cases of disease of the internal ear also appear to support this hydrokinetic theory.

But the position of the canals in close anatomical relation to the organ of hearing had impressed on the minds of physiologists so obstinate an opinion that they must be connected with the perception of sound in some way or other, that even now many will not admit that they are the peripheral organs of a sense of rotation.

A favourite theory was (and there are still some who hold it) that the semicircular canals give us information as to the direction in which sound comes to us. There are two ways in which we can show that this view is erroneous.

(1) By considering the physical conditions.

The shortest sound wave which we can hear is so long compared with the dimension of the ear, that every part of the ear must be at any instant in the same phase of the wave. We must assume that, as far as the effect of such sound waves is concerned, the liquid contents of the internal ear are incompressible. It is as absurd to speak of sound-waves travelling round one of the canals as to say that it is high water at one end of a dock and low water at the other, at the same time.

(2) By experiments on the way in which we really do perceive the direction of sound. I shall describe two such experiments. (*a*) Let the observer close his eyes—for security it is best to bandage them—seat himself in a chair, and keep his head steady. Now let an assistant produce a sharp short sound. In showing this experiment to Section D of the British Association, at its meeting at Belfast in 1874, I used three coins in the way I show you now. The observer can tell with really astonishing accuracy whether the sound comes from the right or from the left, because he hears it louder in the nearer ear, but he is without any knowledge at all as to whether it comes from above or below, from the front or the back. He forms a judgment indeed on this point, but his judgment is usually wrong, often very ludicrously so.

The experiment is most striking when the click is produced in the mesial plane of his head, in which case he has not the binaural effect to help him. In this connection I may say that I know no experiment which illustrates so well the marvellous delicacy of our sense of relative loudness of sound, a very small deviation from the mesial plane being quite certainly recognised.

We have then with one ear no means of ascertaining the direction of sound if we keep the head fixed. How then do we ascertain the direction of sound? for we all know that we

can do so with very considerable accuracy. This leads me to the second experiment. (*b*) Let the observer, still with eyes closed and bandaged, stand up and be at liberty to move his head. Let the assistant produce the clicking sound, not once only, but again and again at short intervals, always in the same place. The observer turns round until he faces the source of sound. He knows that he is facing it when he hears it equally loud in both ears, and hears it to the right when he turns a little to the left, and to the left when he turns a little to the right, that is the criterion of whether the source is behind or before him. Having now got the azimuth, he seeks the altitude. Moving his head about a right and left axis, he seeks the position in which he hears the sound best. He is now looking towards the source of the sound.

The concha of the external ear acts as a screen, and it is remarkable how much difference there is in the quality as well as in the loudness of most sounds with different altitudes.

Stand in front of a pipe from which water is rushing, and move the head round a right and left axis, bow, in fact, to the pipe, and a striking difference in the quality and loudness of the sound will be observed in the different positions of the head.

It may be said birds have no concha, and yet they perceive as well as we do the direction of sound. But there is a method by which, without any use of the action of the concha, and by purely binaural observations, we can ascertain the direction of sound. By one observation, as already described, we can find a plane containing the line along which the sound reaches us. That plane is at right angles to the line joining our two ears. By moving the head we can shift the line joining our two ears, and then by another similar observation obtain the plane at right angles to the new position of the line joining the two ears and containing the direction of sound. The direction of sound is the intersection of these two planes.

I do not think *we* use this method (although I have tried it and found it work), but we often see birds incline their heads when listening in such a way as to suggest that they use it.

There is another objection which is often brought against the theory I have been explaining. It is said, "Is it conceivable that there should be a special sense, common to man and all vertebrate animals, which has remained unknown till about twenty-two years ago? This is a sense invented, not discovered by scientific men, otherwise we should all have known about its existence at least."

This objection is not one to be met by contempt: it has a real basis, and as I believe this sense to be a real one, I feel bound to look for the cause of the incredulity.

A special sense is popularly understood to be a gateway of knowledge. Information as to external things comes to us in various ways, and each of these ways has from ancient time been recognised and named as a special sense. But this is not exactly the physiological way of looking at things. I may illustrate the difference by a sort of analogy. In a large business establishment the manager sits in his room upstairs. He has various ways of getting information. The post brings him letters, he looks at them; some he carefully considers and answers, others he looks at and puts into the waste-paper basket—but he has looked at them all. So we *see* things; many of the things we consider, take note of, others we pay no attention to—do not an hour later remember anything about them. But there are many messages which come to the business establishment and never reach the manager's room at all. They are attended to by clerks in the office. They are not futile, they are real messages and serve their purpose, a purpose essential to the carrying on of the business. If these were not attended to downstairs, the manager would very soon hear of it. So with us. There are what we may call sensory impressions which do not make their way to the conscious *Ego*, but are all the same properly attended to by what in us corresponds to the clerks. If our clerks neglect their work, the conscious *Ego* very soon becomes aware that there is something wrong.

In the case of the sense of rotation, ordinarily we pay no attention to its messages; the clerks at the sensory centres of the ampullary nerves, and at the motor centres of the muscles of the eyeballs, do all that is necessary. We perceive the result of their work in our visual sense of the fixedness of the outside world, and we do not trouble ourselves as to how the office work has been done.

But—and here I come to a matter I referred to early in this lecture—the office work is sometimes not well done, and the visual sense of the fixedness of the outside world is lost. If this is due to disease, we send for the doctor and ask him to find out what is wrong in the office, and, if he can, put it right. But there is a far more common cause of the loss of the visual sense of the fixedness of the outside world, one which it has not been left for two or three scientific men to discover in the last quarter of the nineteenth century. The most characteristic effect of alcohol is to make all reflex actions sluggish. Under the influence of a moderate dose of alcohol, what I have called the office work, goes on all right but not quite so fast as with no alcohol. The message arrives, and the answer is sent, but not quite so promptly. The conscious *Ego* may not note anything wrong, but a quantity of alcohol, far short of a dangerously poisonous dose, may delay the transmission of the signal to the muscles of the eyeball so much as to affect quite perceptibly the compensation of the movements of the head. A perfectly sober man sees the world wag a little when he wags his head very vigorously—a point of light is perceptibly drawn out into a horizontal line of light—the office work fails a little under such extreme pressure. But a little alcohol makes the office work fail more readily, and as the dose is increased it fails altogether, and the sense of the fixedness of the world is wholly lost. Even in such an extreme case of intoxication, short of paralysis, the drunken man may see the world steady, if only he can keep himself steady. I dare say we have all seen very drunken men walking quite straight, but with a preternatural fixedness of the head. If anything makes them move their head, they totter and reel. They move the head a little; that happens to them in consequence of a small and slow rotation of the head, which happens to us when we wag our head violently, and they reel and stagger just as we should reel and stagger if we tried to walk, violently wagging our head all the time.

Just as there are blind men and deaf men, so there are men who have lost or never had the sense of rotation. Such persons are almost always deaf-mutes. The close anatomical relation of the organ of hearing and the organ of the sense of rotation has this effect, that imperfect development of pathological injury of the one is usually associated with similar defect in the other. And experiments on deaf-mutes have shown that a large proportion of them are defective in the sense of rotation. This is shown by the absence of the normal jerking of the eyeballs when they are rotated, and by a perceptible insecurity in their gait. They do not reel as drunken men do, just as blind men find their way about much better than we could do if our eyes were bandaged up; they have learned to get on fairly well with the help of experience and their other senses.

I am not sure whether in this account of the sense of rotation, of its organ, and of the use of it, I have carried all my hearers with me, and convinced you of the real existence and real practical use of this sense. I hope, however, I have made it clear that the subject is worthy of attention, and that we have here matter for the careful consideration of physicists, physiologists, and psychologists.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—In a Convocation, held on Tuesday last, the University, or at least a section of it, displayed itself in an unfavourable light. The Convocation House was crowded, not because of the Statute on Research Degrees, which came before the House, and passed its final stage without opposition, but because of the proposal, which seemed to be a modest one, that Anthropology should be included among the subjects of the Final School of Natural Science, not as an extra, but as an equivalent subject. This proposal was from the first strongly opposed by a few members of Congregation, but passed the two readings in that body by substantial majorities. The opponents of the subject, however, were not content to accept the results of these votes, and issued an urgent whip to members of Convocation, with the result that the statute was rejected by 68 votes against 60. Presumably the philosophers, historians, and divines who succeeded in throwing out the statute at its final stage are pleased with their performance. To the outside world, which is less than ever convinced that education is comprised within the limits of the subjects of the School of Literae Humaniores, their action will be but another instance of the incompetency of a section of the classical world to understand what is going on around them. The circular which was issued

by the opponents of the statute was so artfully worded as to rouse theological suspicions. Reference was made to the undesirability of the comparative study of religions, and it was obvious that a considerable proportion of those who attended to vote against the measure, had come in obedience to a summons to resist the enemy, and were in no way qualified to form a judgment on educational matters. The larger proportion, however, consisted of those classical teachers whose belief it is that science may safely be ignored in a nineteenth century education, and that a "good general education" means only a training in the Greek and Latin languages, with a smattering of ancient history and philosophy. The result of the vote was a great disappointment to those who had hoped that the work of Prof. Tylor, Prof. Arthur Thomson, and Mr. H. Balfour, would find its fruition in a small but earnest school of anthropologists in Oxford.

The National Association for the Promotion of Technical and Secondary Education has made arrangements for a Conference of the representatives of Technical Education Committees to be held at the Royal United Service Institution, on July 11, when the Duke of Devonshire, President of the Association, will take the chair. The object of this Conference is to discuss means whereby the various authorities charged with the provision of technical education may be brought into closer relationship, and may be enabled to avail themselves of the results of the experience of others as regards many important details of their work. Among the subjects which it is proposed to deal with are (*a*) scholarships (local conditions and uniformity in respect to award and tenure), (*b*) evening continuation schools (the co-ordination of their work with that of evening science, art, and technical classes), (*c*) trade and technology classes and their relation to the various trades.

The chemical and engineering societies formed by the members of many of our polytechnic institutes might emulate, with advantage, the Engineering Society of the School of Practical Science, Toronto. We have lately received a volume of 253 pages containing the papers read before the Society during the session 1894-95. The papers refer to both the theoretical and practical sides of engineering, and their publication cannot but encourage investigation among the students. A plan adopted by this Society, and by a number of American societies of a similar kind, is worth noting. Before a paper is read, 150 proofs of it are distributed among engineers and specialists interested in the subject with which it deals, and their opinions upon any particular point are invited. The replies received are read after the paper, and help to make the discussion more general and of greater value than it otherwise would be.

The Corporation of the Massachusetts Institute of Technology, Boston, have a good understanding of what technical education means. The following paragraph, from the *Calendar* of the Institute received a few days ago, should be borne in mind by the organisers of technical education in this country:—"The foundation of all sound technological education requires not only thorough theoretical training, but also prolonged, well-directed laboratory drill which shall first give the student the power of close and accurate observation, and then bring him into direct contact with the material problems of his future profession." It is by acting upon this educational principle that the Massachusetts Institute has gained such a large measure of success.

Tables showing the number and proportion of pupils attending secondary schools in London are given in the *Technical Education Gazette*. The returns obtained show that the number of pupils receiving education in 84 public endowed and public proprietary schools is 19,072, and the number receiving education ni 126 private or semi-private schools is 7107. The proportion which pupils attending secondary schools bear to those attending public elementary schools, may be gathered from the fact that the number per 100,000 of the population attending secondary schools is 623, while the number per 100,000 of the population attending public elementary schools is 16,904.

SCIENTIFIC SERIALS.

Bulletin of the American Mathematical Society, vol. i. No. 8 (May 1895).—Kinetic stability of central orbits, by Prof. Woolsey Johnson, contains an investigation, of an elementary character, of a problem not discussed in the fourth edition (p. 125) of Tait and Steel's "Dynamics of a Particle." It is a satisfactory discussion of the problem so far as it relates to central orbits. The note was read before the Society at its April meeting.—Dr. J. Pierpont, in a short paper, read before the Yale Mathematical Club, entitled "Lagrange's place in the Theory of Substitution," though he cannot vindicate Lagrange's right to the title of creator of the theory of substitutions, presents a few examples of his methods in order to show the importance of considering him from this point of view. "Lagrange was led to the study of this theory by his attempts to solve equations of degree higher than the fourth."—Gauss's third proof of the fundamental theorem of algebra, by Prof. Bôcher, indicates the connection between Gauss's third proof that every algebraic equation has a root and those branches of mathematics which have since been developed under the names of the theory of functions and the theory of the potential. The notes, among other details, give the different courses of lectures in mathematics at American and European colleges.—There is the usual long list of new publications.

Wiedemann's Annalen der Physik und Chemie, No. 5.—Wave-lengths of ultra-violet aluminium lines, by C. Runge. The lines of the spark spectrum near 186 μ wave-length are of great intensity, and may be used as standards of reference. They were therefore carefully determined by means of a Rowland concave grating and sensitive plates prepared by Schumann's method. They were compared with the spectrum of iron, and referred to Rowland's standard wave-lengths for that substance. The figures for the four lines at 760 mm. pressure and 20° C. were 1854·09, 1862·20, 1935·29, and 1989·90. The wave-lengths reduced to a vacuum would be about 0·6 units greater.—On the dichroism of calcspar, quartz, and tourmaline for infra-red rays, by Ernest Merritt. The absorption of the infra-red rays in these substances depends upon the plane of polarisation. Especially in calcspar and in tourmaline the two curves representing the transmittency for the ordinary and the extraordinary ray, respectively, are quite different, so that they appear to be independent of each other. The following absorption bands were observed in these curves: Calcspar, at 2·44 μ and 2·74 μ for the ordinary ray. These are very sharp. Some broad bands also appear at 3·4 μ, 4 μ, and 4·6 μ. The extraordinary ray is absorbed at wave-lengths of 3·28, 3·75, and 4·66 μ. Quartz shows an absorption band for the ordinary ray at 2·9 μ. When the wave-length exceeds 4·75 μ the substance is practically opaque for both rays. Tourmaline absorbs the ordinary ray of wave-length 2·82 μ. The two curves intersect at 2·30 μ and again at 3·84 μ, so that between these two points the dichroism of tourmaline is reversed.—On the transmittency of solid bodies for the luminiferous ether, by L. Zehnder (see p. 153).—On the measurement of high temperatures with the thermo-element and the melting-points of some inorganic salts, by John McCrae. The melting-points of a number of salts, chiefly alkaline haloids, were determined by means of a platinum and platinum-rhodium couple, whose E.M.F. is proportional to the temperature between 300° and 1400°. The temperature of the alcohol flame, as shown by the same couple, was 1488°, and that of the Bunsen flame at its hottest part, 1725° C.—On electric resonance, by V. Bjerknes. This is an important contribution to the theory of Hertzian oscillations. The author considers the effect of the periods of the oscillator and the resonator, and their logarithmic decrements, together with a constant measuring the intensity of the oscillations. He thus arrives at several fundamental laws, such as: The secondary spark potential is proportional to the square of the period of the resonator, the magnetic or thermal integral effect to its cube, and the electric integral effect to its fifth power.

SOCIETIES AND ACADEMIES.

London.

Physical Society, June 14.—Captain W. de W. Abney, President, in the chair.—Mr. Burstall continued the reading of his paper on the measurement of a syclically varying temperature. Three sizes of platinum wire have been employed for the thermometers in order that some idea might be formed as to the magnitude of the error caused by the lag of the temperature of the wire behind that of the gases. The constants of the platinum thermometers were determined either by comparison with a standard Callendar platinum thermometer or by means of

[illegible], boiling water and boiling sulphur. In most cases the thermometer constants were determined after the wire had been exposed to the action of the hot gases for about half an hour. One wire, however was calibrated before being used, and an unusually high value was obtained for the coefficient δ. After this wire had been exposed to the hot gases, the value of δ fell, however, to the normal. The author thinks the abnormal value may have been due to the formation of a gold platinum alloy during the process of attaching the wire to the leads, and that this alloy was subsequently swept off by the hot gases. The paper includes a number of tables and curves which embody the numerical results, and show that concordant results can be obtained on different days and with different thermometers. Prof. Perry said that an instrument for quickly recording varying temperatures was greatly required by engineers. The temperature just inside the cylinder walls was, however, the most important to determine, and a knowledge of how the temperature from 1 to 2 m.m. inside the walls varied would be of the greatest importance. He would like to ask the author if the observed temperatures agreed with the values calculated on the assumption that the gases in the cylinder behaved as a perfect gas, and that $\frac{PV}{T}$ was constant during the whole stroke. Differences between the observed and calculated values might be due to dissociation, and not entirely to lag in the thermometers. It was astonishing that even the fine wires employed were able to follow the rapidly varying temperature, and he would like to see some special experiments made to test this point. Prof. Capper showed a diagram giving the values calculated on the assumption that $\frac{PV}{T}$ = constant. In such a calculation it was necessary to assume some temperature as a starting-point, and in general this temperature was obtained from an analysis of the exhaust gases, so that the calculated curve is most likely to depart from the truth at the commencement of the stroke. He, Prof. Capper, hoped that the author would be able to accurately determine the temperature of some one point of the stroke, and he suggested that the point where the observed curve crossed the theoretical curve would be the most suitable one for this purpose. Such a point must exist, since at the commencement of the stroke the lag causes the observed temperature to be too low, while at the end of the stroke the observed temperatures are too high. Mr. Burstall finds a curious bump in his curves, and it is curious that a similar bump exists in the calculated curves. From the constancy with which this bump appears, it would seem that it must have some physical meaning. It was important to remember that the expansion in the gas-engine cylinder is not adiabatic, for heat is both abstracted and generated during the stroke. Mr. Blakesley suggested that since the temperatures dealt with were sufficient to make the wire red-hot, the question arose whether lag might be investigated by the wire being examined by means of Becquerel's phosphoroscope, at a known interval after the removal of the source of heat. Mr. Griffiths said he considered an important source of error was the large thermal capacity of the leads when the working wire was so very short. He thought it would be possible to standardise the thermometers under conditions similar to those which occur in the engine cylinder. Thus perhaps alternate gushes of air at 0° and 100° C. might be used. The use of gold to attach the fine wire to the leads was objectionable, since the gold must permeate the platinum for quite an appreciable fraction of the whole length of the wire. He would like to know whether the change in δ alluded to by the author occurred with the first explosion, or whether it was a gradual one. Mr. Enright pointed out that the nature of the working substance in a gas engine varied during the stroke. Prof. Perry said that the change in the specific volume of the gases before and after combustion did not amount to more than 1·25 per cent. Mr. E. Wilson said he thought it was most important to shorten the time of contact, since at present the galvanometer readings corresponded to the mean temperature over a range of about 5 per cent. of the whole stroke. It might be possible to make use of a condenser to get over this difficulty. Prof. Rucker said that the Kew Observatory were making arrangements to undertake the testing of platinum thermometers. Mr. Enright suggested that with a very short contact induced currents might cause errors. Mr. [illegible] said that he had found that the method of determining the zero point of thermometers, by means of melting ice, was far from satisfactory, and that the results obtained could not be depended upon to within 0°·1 C. The author, in his reply, said the only chemical action on the wires he thought probable was the formation of a carbide. After several hours use, however, the wires appear quite bright and clean. Mr. N. E. Deerr read a paper on the thermal constants of the elements. The object of the paper is to establish the following laws: If T denote the melting-point on the absolute scale, C the mean coefficient of expansion between zero and the melting-point, S the mean specific heat, and L the latent heat of fusion, then, for any family in Mendeléef's periodic classification, the following relations will hold between metals and metals, and between non-metals and non-metals:

$$\left(T + \frac{L}{S}\right)C = \text{const.}$$

$$TC = \text{const.}$$

$$\frac{LC}{S} = \text{const.}$$

In the absence of other data, the mean values of C and S between 0 and 100° have been taken. Anomalous values are obtained in the case of gold and mercury, if these metals are included in their usual positions. The author considers that the thermal constants indicate that gold ought to be placed among the transition elements. He further proposes to place mercury in a new group to come before the lithium group. Such a group, he suggests, would contain hydrogen, argon and mercury. The paper concludes with an attempt to justify the expression

$$\left(T + \frac{L}{S}\right)C = \text{const.}$$

on theoretical grounds. Dr. Gladstone considered that the paper contained valuable numerical relationship, and that the second and third formulæ were much more strongly supported by the data given than the first formula. He, the speaker, had previously noticed that the elements of the transition group might be subdivided into sub-groups, and that the elements of each of these sub-groups were particularly closely related. He agreed with the author that gold ought not to be included in the first group. Mr. F. H. Neville said that since the author did not give the source of the data he had employed, most of the results given were rather indefinite. For example, while the author gives 870 as the melting-point of aluminium, Mr. Haycock and himself had found the value 927°. The value of the latent heat of aluminium given was 29·3, while Pionchon, in a recent paper in the *Comptes rendus*, gives the value 80. Theoretical considerations appear to indicate that 80 is the minimum value possible. The author assumes that when you heat a substance from the absolute zero to its melting point, all the energy supplied is expended in the work of expansion. Some of the heat, however, must be employed in changing the kinetic energy of the molecules, even in the case of a solid. Prof. Worthington said that in some cases the amount of work done against cohesive forces between 0 and 100° was much less than one ten-thousandth of the whole amount of energy supplied. Mr. Griffiths said he did not believe in any generalisation which depended on the values of the specific heats determined between 0 and 100°, the rate of change with temperature of specific heat being so great. The author in his reply said he had made every endeavour to obtain the most accurate data for his calculations. The value 29·3 for the latent heat of aluminium was obtained from a paper by J. G. Richards.—A paper on an electromagnetic effect, by Mr. F. W. Bowden, was postponed till the next meeting.

Entomological Society, June 5.—The Right Hon. Lord Walsingham, F.R.S., Vice-President, in the chair.—Dr. Sharp, F.R.S., exhibited, on behalf of Dr. G. D. Haviland, two species of *Calotermes* from Borneo, the individuals being alive and apparently in good health; one of the two small communities (which were contained in glass tubes) consisted of a few individuals of the immature sexual forms and of a neoteinic queen; this latter had increased somewhat in size during the eight months it had been in Dr. Haviland's possession, but no eggs had been deposited, neither had any of the immature individuals developed into winged forms. The second community exhibited consisted entirely of the immature sexual forms, and this community had produced numerous winged adults while it had been in Dr. Haviland's possession. Specimens were also exhibited to illustrate the neoteinic forms that were produced in Borneo after a community had been artificially orphaned. As regards these, Dr. Sharp expressed

the hope that Dr. Haviland would shortly publish the very valuable observations he had made. In the case of a species of fungus Termite, Dr. Haviland had found that the community had replaced a king and queen by normal, not by neoteinic forms.—Mr. McLachlan, F.R.S., exhibited examples of the female of *Pyrrhosoma minium*, Harris, having the abdomen incrusted with whitish mud through ovipositing in a ditch in which the water was nearly all dried up. He had noticed the same thing in other species of Agrionidæ.—Mr. Roland Trimen, F.R.S., exhibited some specimens of "Honey" Ants, discovered at Estcourt, in Natal, about a year ago, by Mr. J. M. Hutchinson. The species has not been identified, but is quite different from *Myrmecocystus* and *Camponotus*—the genera which have long been distinguished as containing species, some of whose workers are employed as living honey-pots for the benefit of the community. The specimens exhibited included six "globulars"—to use Mr. McCook's term in regard to the American species, *Myrmecocystus hortus-deorum*—all with the abdomen enormously distended with nectar; but other examples presented to the South African Museum by Mr. Hutchinson comprised various individuals exhibiting different gradations of distension, thus indicating that the condition of absolute repletion is arrived at gradually, and may possibly be reached by some few only of those individuals who feed, or are fed, up for the purpose. Certainly, in the nests examined by Mr. Hutchinson, in Natal, the number of "globulars" was very small in proportion to the population of ordinary workers; and it is somewhat difficult to understand of what particular value as a food reserve so very small a quantity of nectar so exceptionally stored can be. Mr. Trimen added that while the occurrence of "Honey" Ants in Southern, North America, South Australia, and he believed also in India, was well known, the Natal species now exhibited was the first African one that had come under his notice.—Dr. Sharp exhibited a series of Coleoptera, to illustrate variation in size.—Herr Brunner von Wattenwyl made a communication informing the Society that a most unfortunate error had crept into the table of genera in his Monograph of *Pseudophyllides*; on page 9, line 1, and on page 13, line 37, instead of "mesonotum" should be read "mesosternum."

Geological Society, June 5.—W. H. Hudleston, F.R.S., Vice-President, in the chair.—On a well-marked horizon of Radiolarian rocks in the Lower Culm Measures of Devon, Cornwall, and West Somerset, by Dr. G. J. Hinde and Howard Fox. In the Lower Culm Measures the basal *Posidonomya*-beds and the Waddon Barton beds with *Goniatites spiralis* consist of fine shales with thin limestones, and above these are the beds which form the subject of the present paper. The Upper Culm Measures consist of conglomerates, grits, sandstones, and shales, with occasional beds of culm. There is evidence of the partial denudation of the radiolarian rocks during the accumulation of the Upper Culm beds, as indicated by the presence of pebbles of the former in the latter. The radiolarian beds consist of a series of organic siliceous rocks—some of a very hard cherty character, others platy, and yet others of soft incoherent shales. The term "grits," which has been used in connection with these beds, is a misnomer; there are beds which are superficially like fine grits, but they are found to be radiolarian deposits. At present there are not sufficient data for estimating the thickness of the radiolarian deposits; but they are probably some hundreds of feet thick, though the whole does not consist of beds of organic origin. In a quarry in the Launceston district 50 feet of radiolarian cherty rock are seen without admixture of shale. A detailed description of the lithological characters of the rocks of the series was given, and analyses by Mr. J. Hort Player; a marked feature of their composition is the very general absence of carbonate of lime. The microscopic characters of the rocks were also described, and the small amount of detrital matter in the beds of the series was noted. The fossils tend to confirm the view that the Lower Culm Measures are the deep-water equivalents of the carboniferous limestone in other parts of the British Isles, and not shallow-water representatives of deeper beds occurring to the north, as was formerly supposed. In connection with this it was noted that the deep-sea character of the Lower Culm of Germany, which corresponds with our Lower Culm Measures, was maintained by Dr. Holzapfel even before the discovery of radiolaria in the beds of Kieselschiefer furnished such strong evidence in support of this view.—The geology of Mount Ruwenzori and some adjoining regions of Equatorial Africa, by G. F. Scott-Elliot and Dr. J. W. Gregory. Ruwenzori is a mountain between the Albert and Albert Edward Nyanzas. Topographically it is a narrow ridge which extends for about 50 miles in a direction from north-north-east to south-south-west. Its summit attains a height of 16,500 feet. The western slope is at an angle of 22°; the eastern slope at about one of 4°. The authors described sections across the ridge at right angles to its trend. These showed that Ruwenzori is not volcanic, nor is it a simple *massif* of diorite. Epidiorite occurs only as banded sheets in the schists on the flanks of the mountain, and is not the central rock of the ridge. The strike of the flanking schists seems to run concentrically round the ridge as though the central rock were intrusive into them. The highest rock collected, a coarse-grained granite or granitoid gneiss, may be an intrusive igneous rock, but it may be part of the old Archæan series faulted up; there is nothing in its microscopical characters to separate it from the Archæan rocks, and the authors thought it probable that this rock was raised into its present position by faulting. In this case Ruwenzori is simply composed of an orographic block or "scholl," which was at one time probably part of a wide plateau of Archæan rocks. There is abundant evidence of volcanic action around Ruwenzori, for the plains, especially to the east and south-east, are studded with small volcanic cones, arranged on lines which radiate from Ruwenzori. Evidence points to the former occupation of the Nyamwamba, Mubuku, and Batagu valleys by glaciers, *roches moutonnées* of typical character having been noted in the two former valleys. The country round Ruwenzori consists of rocks which may be conveniently grouped into two series—one composed of gneisses and schists, and the other of non-foliated sediments. The former (the Archæan series) are of the type that has an enormous extension in Equatorial Africa, and forms the main plateau on which all the sediments and volcanic rocks have been deposited. The sedimentary rocks are probably Palæozoic, possibly pre-carboniferous, but in the absence of fossils it would be unsafe to go beyond this statement.—On overthrusts of tertiary date in Dorset, by A. Strahan. The results given in this paper were obtained during a re-survey of South Dorset on the 6-inch scale. The disturbances can be divided into two groups—the one being mainly of Miocene date, and the other of intra-cretaceous (between Wealden and Gault) age. The former includes the Isle of Purbeck fold (which is the continuation of the Isle of Wight disturbance), the Ringstead fold, the Chaldon and Ridgeway disturbances, and the Litton Cheney fault. In the latter are placed the anticline of Osmington Mill, the syncline of Upton, and a part of the anticline of Chaldon; farther west the Broadway anticline and Upway syncline, a fault at Abbotsbury, and many other folds come into the same group. These earlier movements led to the well-known unconformity at the base of the Upper Cretaceous rocks.

Linnean Society, June 6.—Mr. W. Percy Sladen, Vice-President, in the chair.—The minutes of the last meeting having been read and confirmed, the Chairman, on behalf of the President, declared the following to be Vice-Presidents:—Messrs. J. G. Baker, F. Crisp, A. Lister, and W. P. Sladen. Mr. B. B. Woodward was elected a Fellow.—Mr. M. Buysman, who has laboured for many years to establish a garden at Middleburg for economic plants, exhibited specimens to show the excellence and completeness of his preparations.—On behalf of Mr. T. J. Mann, who had lately returned from Ceylon, Mr. Harting exhibited specimens of a butterfly, *Catophaga galena*, Felder, which had been observed migrating in thousands across the northern part of the island during March and April last, in a direction from north-east to south-west. The movement commenced about 7 a.m. and lasted until noon, when it decreased, and was resumed in the afternoon for another two hours. Mr. Harting referred to the remarks on this subject made by Sir J. Emerson Tennent ("Nat. Hist.," Ceylon, 1861, p. 404, note) to the observations of Darwin on the countless myriads of butterflies met with at sea some miles off the mouth of the Plata, and to a paper by Mr. R. McLachlan on the migratory habits of *Vanessa cardui* (*Entom. Mo. Mag.*, xvi. p. 49). He did not think that the movement was analogous to the migration of birds which migrated in opposite directions in spring and autumn, for the insects moved only in one direction, and did not return, vast numbers perishing *en route*. The phenomenon rather resembled what had been observed in the case of lemmings, locusts, and dragon-flies (Weissenborn, *Mag. Nat. Hist.*, n.s., vol. iii. p. 516), and might be explained as a sudden exodus from the birthplace, leading to a compensating reduction of the species after a season exceptionally favourable to its increase. His

remarks were criticised by Colonel Swinhoe, who was inclined to confirm this view, and by Mr. Kirby, who referred to the particular species which were found to take part in these so-called "migrations."—A new *Distomum* was described by Mr. G. West, whose observations were favourably criticised by Mr. W. P. Sladen and Prof. Howes.—On behalf of Mme. van der Bosse, Mr. George Murray communicated a description of a new genus of Algæ (*Pseudocodium*), the characters of which were minutely pointed out by means of specially-prepared lantern slides.—A paper was then read by Mr. A. Vaughan Jennings on the nature of *Mœbiusispongia parasitica*, on which critical remarks were made by Prof. Rupert Jones and Mr. F. Chapman.—A second paper by Mr. Vaughan Jennings contained a description of a new genus of Foraminifera of the family *Astrorhizidæ*.

PARIS.

Academy of Sciences, June 10.—M. Lœwy in the chair.—On the Meudon Physico-Astronomical Observatory, by M. J. Janssen. An account of the present state of the Observatory and of the difficulties through which it has passed on account of the reductions made in the State grants and appropriations, together with some details of the work done since 1876.—On the necessarily harmonic form of expression, for the displacements of each particle in an ocean roller, as a function of the time, by M. J. Boussinesq.—Note on the photographic surveys executed in 1894 by the Canadian engineers and the United States Coast and Geodetic Survey Service for the delimitation of Alaska and British Columbia, by M. A. Laussedat. This is an account of the spread of the Canadian method into the United States Service, and a review of the general adoption of similar processes in other countries.—Solar observations made at Lyons Observatory during the first quarter of 1895, by M. J. Guillaume.—On algebraical curves of constant twist and on algebraical minima surfaces inscribed in a sphere, by M. E. Cosserat.—New theorems in arithmetic, by P. Pepin.—On an explosive system capable of demonstrating the rotation of the terrestrial globe, by M. Jules Andrade.—Spectroscopic study of carbons from the electric furnace, by M. H. Deslandres. Two carbon poles used in M. Moissan's electric furnace were examined. Those parts of the carbon removed from the arc showed the usual spectra of impurities, whereas the parts in the arc were free from all impurities except calcium. The growths on the negative pole were of greatest purity, and furnished carbon spectra showing wave-lengths (cited) much fewer than those recorded for carbon by Hartley and others. The purification of the carbons by the passage of the current in the arc is due to the volatilisation of the more volatile constituents at the high temperature obtained.—On sensitive flames, by M. E. Bouty.—Physical properties of acetylene; acetylene hydrate, by M. P. Villard. A table of pressures corresponding to certain temperatures is given for acetylene, together with a table of dissociation pressures for the hydrate of acetylene. This hydrate resembles the hydrates of nitrous oxide and carbon dioxide, and is represented as $C_2H_2.6H_2O$. Its heat of combination is 15·4 Cal. per molecule, very near to the value, 15·0 Cal., obtained for carbon dioxide and nitrous oxide.—Synthetical production of nitro-alcohols, by M. Louis Henry.—Condensation of aldehydes and saturated ketones, by MM. Ph. Barbier and L. Bouveault. The researches detailed appear to establish the fact that only ordinary acetone can condense easily with aldehydes; on the other hand, as the molecular weight of the aldehydes increases, the aptitude for condensation with acetone diminishes, and the principal reaction becomes the condensation of the aldehyde itself.—On the causes of the colouration and the coagulation of milk by heat, by MM. P. Cazeneuve and Haddon. The conclusions are drawn: (1) That the yellowing of milk by heat is due to oxidation of lactose in the presence of the alkaline salts of the milk; (2) the oxidation of lactose produces acids, formic among others, and hence causes coagulation of the milk; (3) the coagulated casein is not itself altered, but is merely tinted by the decomposition products of lactose.—Esters of the active α-oxybutyric acids, by MM. Ph. A. Guye and Ch. Jordan.—On the history of the alkaloids of the Fumariaceæ and Papaveraceæ, by M. Battandier.—Contribution to the study of germination, by M. Th. Schlœsing. The germination of lupin seeds or wheat does not entail an appreciable loss of nitrogen in the gaseous state.—On amylase, by M. Effront.—The Cecidomyia of oats (*Cecidomyia avenæ*, nov. sp.), by M. Paul Marchal.—The epidermal cell of insects: its paraplasm and its nucleus, by M. Joannes Chatin.—On the relation of the thermal springs of Néris and Evaux with ancient faults of the Central Plateau, by M. L. de Launay.—On the succession of fauna of the Upper Lias and Lower Bajocien in Poitou, by M. Jules Welsch.—Researches on the modifications of nutrition in persons suffering from cancer, by MM. Simon Duplay and Savoire. The differences observed in amounts of urea and phosphoric acid excreted by cancerous patients, as compared with the normal healthy excretion, are due entirely to defective nutrition, and disappear when a suitable régime is used. These differences cannot be used for purposes of diagnosis.—On the use of chloride of lime and its mode of action against the bite of venomous serpents, by MM. C. Phisalix and G. Bertrand.—Storms of five days from May 20 to May 25, 1895, in Bohemia, by M. Ch. V. Zenger.

BOOKS, PAMPHLETS, SERIALS, &c., RECEIVED.

BOOKS.—A Chapter on Birds. Rare British Visitors: Dr. R. B. Sharpe (S.P.C.K.).—The Metallurgy of Iron and Steel. Vol. 1. The Metallurgy of Iron: T. Turner (Griffin).—The Story of the Plants: Grant Allen (Newnes).—England's Treasure by Foreign Trade: T. Mun, 1664 (Macmillan).—Natural History of Aquatic Insects: Prof. L. C. Miall (Macmillan).—Chemistry, Inorganic and Organic: C. L. Bloxam, 8th edition, rewritten and revised by Thomson and Bloxam (Churchill).—The Great Frozen Land: F. G. Jackson (Macmillan).

PAMPHLETS.—Report of the Director of the Observatory to the Marine Committee, Liverpool Observatory, Bidston, 1894 (Liverpool).—Les Variations Periodiques des Glaciers des Alpes, Report, 1894: Prof. Forel (Berne).—White Servitude in the Colony of Virginia: J. C. Ballagh (Baltimore).—Protection from Lightning: A. McAdie (Washington).

SERIALS.—American Naturalist, June (Philadelphia).—Journal of the Franklin Institute, June (Philadelphia).—Abstract of Proceedings of the South London Entomological and Natural History Society, 1894 (London).—Seismological Journal of Japan, Vol. 4 (Yokohama).—Mathematical Gazette, May (Macmillan).—Memoires de la Société de Physique et D'Histoire Naturelle de Genève, tome xxxii. Première Partie (Genève).—Kew Observatory, Richmond, Report for the Year 1894 (Harrison).—Bulletin of the Geological Institution of the University of Upsala, Vol. 2, Part 1, No. 3 (Upsala).—Massachusetts Institute of Technology, Boston, Annual Catalogue, 1894-95 (Cambridge, Mass.).

Betts's Chromoscope (Philip).

CONTENTS.

THURSDAY, JUNE 27, 1895.

"THE WIZARD OF MENLO PARK."

The Life and Inventions of Thomas Alva Edison. By W. K. L. Dickson and Antonia Dickson. (London: Chatto and Windus, 1894.

THE present rapid increase in the number of places where the Edison Kinetoscope is exhibited, leads one to glance through the account which was published towards the end of last year of the life and adventures of the American inventor. The career of one who started as a newsboy, and who has raised himself to fame and wealth by his quickness of perception, fertility of resource, and general shrewdness, has been too varied and exciting for the authors to succeed in rendering the narrative uninteresting.

But the pages of rhapsody with which this long quarto book is filled, combined with the extremely verbose and grandiloquent style in which it has been written, not only render the meaning well-nigh unintelligible in many places, but give a wholly false notion of Mr. Edison's character. For those who have met him must have been struck with his somewhat boyish character, his fondness for a joke, and his objection to black coats, tall hats, and formality. The Edison of this book would hardly be recognised as the Edison who, we remember, some years ago could not be induced to put on his coat or shoes to receive an English peer, well known to science, who happened to call at Menlo Park when the inventor was taking his afternoon nap.

We start, of course, with Edison's pedigree, and we are told that his father, "Samuel Edison, however, was not minded to stimulate the waning flames of patriotism by a libation of personal gore." We should have thought the father of an inventor would have known that gore was not a good sort of kindling. Then comes a description of "callow collegians dragged through an uncongenial course of study, boarding-school graduates steeped in a weak solution of accomplishments, ephemeral creatures on whose glossy plumage the dews of Parnassus have no power to rest"; but Edison, on the contrary, "despite his paucity of years," read through "fifteen feet of closely serried volumes." Then we come to an excellent portrait of Edison at fourteen years of age, which strikingly resembles the closely shaven Edison of to-day, and shows the same merry twinkle of the eye.

Facsimiles are given of pages of Edison's newspaper, the *Grand Trunk Herald*, started in 1862, the vast number of blots on which are explained, we suppose, by the fact that this newspaper was regularly composed and printed in a dilapidated freight car attached to a running train. His next venture in the newspaper line, *Paul Pry*, led to his being ducked by a subscriber, and, as his travelling railway printing establishment and laboratory were burnt, through the constant jolting of the springless car shaking the cork out of a bottle of phosphorus, he turned his attention to the construction of a telegraph line. This was not attended with success, since to produce an electric current, "Edison secured two Brobdingnagian cats, with volcanic tempers, attached a wire to their legs, administered a violent amount of friction to their backs, and breathlessly awaited developments."

Afterwards he became a real telegraph operator, and when on night duty in the service of the Grand Trunk Railway of Canada, he was, in common with the other night operators, required to signal the word *six* every half-hour to show that he was awake. Preferring, however, to wander about the town, he obtained a clock and converted it into an automatic telegraph key. This key, however, would do nothing more than periodically signal the word *six*, and declined to answer inquiries, so a detective operator was put on the track, and Edison had to make his escape into the United States.

During the severe winter which followed, the ice broke the telegraph cable under the river which separates Port Huron from the Canadian city of Sarnia, on the opposite bank a mile and a half away, and further rendered all traffic across the river impossible. Communication between the two cities was, however, restored by Edison using the alarm whistle of a locomotive engine to send Morse signals. This power of overcoming difficulties brought him into public notice, and he obtained in succession several good posts as a telegraph operator. His love of fun and of making experiments, however, led him into constant trouble; but he was rewarded at the age of seventeen by making his first invention of an instrument for automatically repeating a telegraphic message.

Edison's electric device for killing cockroaches "is told in the prosaic terms of the nineteenth century," and commences, "Curiosity betrayed our Mother Eve," and so on for many lines. Edison's first patent for a "Vote Recorder" was not commercially successful, as its employment in the Massachusetts Legislature was found to interfere with the power of the House to use "*filibustering.*" Then come his Universal Stock Printer and his employment as operator by the Law's Gold Reporting Company.

During the excitement connected with the operations of the Gould and Fisk ring to make a corner in gold, the stock quotation printer broke down, and Edison gave the very simple explanation that a contact spring had broken and fallen between two cog-wheels in the instrument. To describe this, however, the authors require several pages. "Inflamed by the lust of gold" (not Edison, however, for he was very poor at the time and owed 200 dollars), "and reduced to the semblance of insatiate brutes, the great sea of sentient humanity surged around the shrine of its desires," &c.

Chapter iv. commences with a description of "Edison's storm-tossed craft," and tells how "a steady gale blew from the Blessed Isles, wafting the adventurer into all tempting harbours of successful discovery." We much doubt the value of a wind blowing *from* an island, whether blest or not, to take a craft into its harbour.

In 1870 he was developing his automatic telegraph for transmitting a message by the use of a perforated strip of paper, and receiving it in Roman characters at the other end of the telegraph line; also instruments for automatically sending messages, using the Morse code, as in the well-known Wheatstone's Fast Speed instruments.

Next came the carbon button and the loud-speaking telephone. No reference is here made to Prof. Hughes' microphone, or to the controversy which was carried on

about 1876, as to who invented the carbon telephone transmitter, and we are told that the Edison carbon transmitter "held the monopoly of the telephone in England for many years." In the next chapter, "the pretensions of his rival" are touched on, and Edison's remark, that "one of the biggest steals ever made was filched directly from my telephone," is quoted.

"The individual mistress of Edison's heart until now had been science, but a new potency was at hand equally strong, but immeasurably more subtle and all-pervading." Then the authors drop into poetry, which they have a way of doing on all possible occasions. Later on we are told that "prior to his marriage Edison portioned out the hours of sleeping and waking by the ebb and flow of the Divine afflatus," and that his "blood after having served the purpose of stimulating the capillary vessels of the brain, and inducing inventive capacity, soon retreats quietly to its legitimate source." We note in this chapter references to "Mrs. Noah's superior faculties," the Roman Empire, Carthage and her glory, a Phœnician axiom, and a disquisition on "the sickly and mercurial sentimentality of the Oriental and Latinic races," "the Plutonian broths of Sparta," "the delicious pastoral flavour to the *Allegretto* and the *Lycidas*." We presume Milton's title "l'Allegro" was not long enough for the authors; and all this while Edison has been left gazing at a test-tube in a large photograph on page 95 of this book.

By 1876 forty-five of his distinct inventions were in different processes of completion; £100,000 had been realised from the manufacture and the sale of patents; and the throng of sight-seers to Edison's laboratory at Newark became so great that he moved to Menlo Park, twenty-four miles from New York, and stacked there his "cases of every ordinary and extraordinary device born of that prolific parent, necessity."

The first sketch of the phonograph, on p. 123, is of real interest, for we regard the phonograph as scientifically the greatest of Edison's achievements, in that Edison accomplished with its use, in an extremely simple way, what the previous elaborate talking-machines could not perform. But why the microscopic examination of the tin-foil showed that "the feminine members of the alphabet were less aggressive in their outlines than their masculine coadjutors," or why the "long E vindicated her rights to female enfranchisement," we know not.

Descriptions of various forms of phonographs, phonographic dolls, &c., take us to the end of chapter xi. Chapter xii. is devoted to telegraphing from trains in motion, a subject that is certainly worthy of more consideration than it has yet received, and to Edison's pyromagnetic motor, which, from its principle of construction, could never have been commercially successful.

The chapters on the development of the glow-lamp by Edison, and those associated with him, are some of the most interesting in this book. Phlegmatic indeed must be the reader who does not feel inspired by the enthusiasm which led Edison to despatch Mr. Moore to search through China and Japan, Mr. McGowan to explore the American continent from the Atlantic to the Pacific, and Mr. Ricalton to seek in India, Ceylon, and the neighbouring countries for a vegetable fibre suitable for being carbonised into a glow-lamp filament. But, if the reader be of a critical temperament, his pleasure at reading the account of these explorations will be diminished by the many faults which mar the description.

For example, the large picture on p. 217 of "Cingalese Women, photographed by Mr. Ricalton in his Search for Fibre," was never taken in Ceylon, since it is obviously a photograph of a group of *Japanese* girls posed in front of a theatrical back scene. One of these girls is sitting on a Western rustic garden-chair; so, perhaps, the photograph was taken in New York or Paris, on the principle followed by the special correspondent in the Soudan war, whose envelopes bore the St. John's Wood post-mark. Oddly enough, the book contains several other photographs of Cingalese people taken by Mr. Ricalton; but the authors do not seem to have been struck with the fact that a comparatively small island like Ceylon should have possessed inhabitants of such a variety of different types.

A great deal of tall talk follows about Edison's work on the dynamo machine. "Ah! potent wizard, you shame the records of the Arabian nights and the fabled glories of the East," &c., with the following surprising bit of information for the Englishman: "To-day there is not a hamlet in England, however insignificant, which is not in vital connection with the central sources of supply," that is, has electric energy supplied to it from a central electric light station. Passing over pages of grandiloquence, we come to a long description of Edison's factory and laboratories at Orange. The pictures remind us of what we ourselves saw when visiting Edison, but we have no recollection that in the laboratory "fragrant gums and spices recall memories of the fair Babe of Bethlehem." In fact, what we chiefly remember was our surprise at the large number of phonographs which we saw in course of manufacture, and Edison's sallies of laughter at the simplicity of the English in being so easily gulled by limited liability companies.

Although this book is in parts as silly as anything we have ever read, it is nevertheless full of interest; for it gives a graphic picture of the struggles and success of one who is certainly remarkable for his quickness of insight, originality, and capacity for long stretches of hard work, even if we do not agree with the authors that he is "the greatest genius of this or any other age." Even if we were not told on the title-page that the book was written by W. K. L. and Antonia Dickson, we should feel quite sure that it was a joint production, one of the authors being Edison's superintendent of the experimental department in New York, and the other a poetic rhapsodist who has never read her "Mark Twain." The illustrations are well executed, the printing and paper good, and the general get-up of the book all that can be desired of an expensive quarto volume to lie on the drawing-room table. But why was it not edited? asks the English reader. "P. D."

CRIMINAL IDENTIFICATION.

Finger-print Directories. By Francis Galton, F.R.S. (London: Macmillan and Co., 1895.)

IT will be remembered that the Departmental Committee which reported in the beginning of last year upon the best method of identifying habitual criminals, re-

commended the adoption of the Bertillon system of measurement conjointly with the plan of taking finger-prints now associated in this country with the name of Mr. Francis Galton. He loyally disclaims the honour of being the first to use it; that rests with Sir William Herschel, of the Indian Civil Service. But it is really from the unwearied labours of Mr. Galton that the scientific certainty of the system has been fully proved. He has so simplified the processes of taking and recording the impressions of the finger, has invented so complete and intelligible a series of indications and formulas, that the system can now be worked with the greatest facility and with mathematical precision. Of the supreme value of the finger-print as a means of identification, there can be no manner of doubt. It is, as Mr. Galton happily describes it, "an automatic sign-manual subject to no fault of observation or clerical error, and trustworthy throughout life." The Committee above quoted fully recognised this. "Finger-prints," they reported, "are an absolute impression taken direct from the body itself; if a print be taken at all, it must be necessarily correct." But they were met with the difficulty of classification as applied to any large collection of impressions. Where these were comparatively few, the index adopted by Mr. Galton was admirable and most effective. But where the numbers rose to many thousands, as would of course be the case in a criminal register, it might be a serious question whether searches could be made with reasonable facility and dispatch. It was for this reason that the double system of identification was recommended, for the strongest point in the Bertillon plan of measurement as practised in Paris was its perfect classification. There the particular card required, giving the name and antecedents of an individual, "could be found as certainly and almost as quickly as an accurately spelt word could be found in the dictionary."

Since then Mr. Francis Galton has devoted much time and very highly skilled intelligence to enlarging his methods of indexing and proving beyond all question the usefulness of the finger-prints. He now tells us, in his new work on "Finger-print Directories," how these indexes may be most easily and surely constructed, how the work of reference and search can be easily and quickly performed. Of course the result is largely dependent upon the size of the directory, the number of "sets" of impressions that have been collected to compose it. Mr. Galton's experiments were made with two collections, one of 300 complete sets of finger-prints, the other with 2632. In both, even with the largest, he was entirely successful. "The efficiency of a directory," as he says, "depends on its power of breaking up, with the maximum of surety and the minimum of labour, a collection of sets into groups of which even the largest shall be easily manageable, so that when a group is designated as that in which the set searched for must be, if it exists anywhere in the collection, it shall be quickly discovered." The collection that Mr. Galton finds most easily manageable is not necessarily the smallest, but that which lends itself best to search, in its character and its form. The one he has adopted is the card catalogue: "a collection of separate cards stacked behind one another in the separate order of their formula." Mr. Galton timed himself in his examination of 156 sets in his largest collection, which fell all under the same formula. Eight searches were made, during which a total of 373 cards were examined, and the time taken was a little over thirty-six minutes. Mr. Galton could therefore get through ten cards per minute, the trouble of opening the drawer or other receptacle having been done by an assistant. It is interesting to note that Mr. Galton in his inquiries first accepted the "whorl" as the basis of classification, thinking that from its almost endless variety of shape it would be the most useful of the three forms of impression; but as he went on he discarded it in favour of the "loop," the plainer forms of which could be "classed numerically by the simple expedient of recording the number of ridges in each of them that are crossed by an imaginary line drawn between two definite termini."

For a minute and detailed account of the primary and secondary classification of finger-prints, as well as for the best methods of taking them and studying their forms, we must refer the reader to Mr. Galton's new book. This most useful work contains a number of woodcuts and ample indications for the instruction and guidance of the student, with a specimen-book directory for three hundred sets. But whether the index is in the form of a book or of cards, Mr. Galton affirms, on perfectly good grounds, that it is quite possible to have "a finger-print directory, even of three thousand sets or more, that shall discriminate to within two or three sets." There can be no question, therefore, but that the whole system has passed out of the academic stage into one of real practical usefulness; and we may expect to see it applied for other purposes than that of criminal identification. Now that it has been made really manageable, it may be strongly recommended, for instance, to the military authorities as an infallible method of checking desertion and fraudulent re-enlistment. It appears that out of 35,000 men who enlist annually, 5000 desert, and only half are recaptured. Of the other half many, undoubtedly, re-enlist. Although the exact number cannot be positively fixed, it is estimated at 600, all of whom defraud the exchequer to the value of their second bounty and outfit. If, however, the finger-prints of all recruits were taken on attestation, and a register formed on the plan of the directories constructed by Mr. Galton, indisputable evidence would be afforded which would certainly convict the re-enlisted deserter of his original offence

BIRDS, BEASTS AND FISHES OF THE NORFOLK BROADLAND.

Birds, Beasts and Fishes of the Norfolk Broadland. By P. H. Emerson. 8vo, pp. 396, illustrated. (London: David Nutt, 1895.)

AFTER reading the severe criticisms passed on the works of several eminent British ornithologists—especially as regards illustrations—in the introductory chapter to the volume before us, we hoped we were going to be rewarded by finding something that would eclipse all previous efforts, both in the way of letter-press and plates. But we do not hesitate to say that in both respects we are disappointed. After all the writing about the "caricatures" of Bewick, and the "monstrous and gaudy decorations" of Selby, Gould, and Dresser, we find

only a series of very ordinary photographs, many of which have evidently been done from mounted specimens, and, what is more, from badly mounted ones. As to the text, we fail to see the reason for interlarding it with a provokingly numerous series of provincialisms, which, although no doubt familiar enough to the dwellers in East Norfolk, are certainly not household words in other parts of Her Majesty's dominions. To Norfolk people the names of "Herring-Spink," "Reed-Pheasant," "Spinex," and "Draw-Water," doubtless have a meaning, but we should be somewhat surprised if all our readers are aware that they respectively indicate the gold-crest, bearded tit, chaffinch, and goldfinch. It is true that in most cases the author does introduce a better-known name in the course of his notices, but this is not so with the "reed-pheasant." In omitting all scientific names, we are by no means sure that Mr. Emerson is not right, seeing that these are constantly being changed, while *English* names are permanent; but then let us have *English* names, and not *Norfolk* ones.

In the introductory chapter the author says indirectly that not much has been left out in regard to the habits of British birds; and we cannot help adding that if any important omissions do occur, he has done but little in the way of supplying them. Writing of the wren, he observes that "the tomtit, as the Broadsmen call this pert, child-like little bird, always brings an affectionate smile to your face as you see his hopping, plump little body flitting over the bank, or running along the branches of a leafless tree, stopping every now and then to sing his loud-voiced song; for, though his is a little body, he has a mighty and pleasant song." This example cited is only one of many taken almost at random. The professed ornithologist surely does not want such descriptions, and if the book is intended for the eyes of ladies and young people, why are we treated on p. 211 *et seq.* to a very unnecessary anecdote concerning the amours of swans?

We will take it for granted that among the birds our author has correctly determined the species he notices, and has recorded all those found in the Broads; but in the case of the mammals he is far from exact. He states, for instance, that there are two kinds of bats found there, one of which is designated the common, and the other the large bat. By the former is doubtless meant the pipistrelle, but as to the species indicated by the latter title we have no clue; and surely there ought to be more than two species of bats in Norfolk. Among the voles, again, we have two species, respectively termed the "red mouse" and the "marsh mouse"; and, although the former may be the bank vole, we can scarcely recognise the common field vole under the latter inappropriate title, if so be that it is intended for that species. The Broadland rats which the author places a long distance after the mice and voles are likewise left in a state of hopeless confusion, and we quite fail to recognise what are the three kinds alluded to under the names of "big rat with [illegible] chest," "large brown rat," and "little red rat." If the author thinks he has got hold of new species, or even fashionable sub-species, why did he not submit his specimens to a specialist? But as it is, his notes are [illegible] to the scientific zoologist, and, we should think, of no great interest to the ordinary observer of nature.

In the chapter on frogs and toads, the author excels himself. Of these animals he recognises the following: viz. the "garden-toad," "water-toad," "running toad," "common frog," and "land-frog." To know what creatures are meant might perhaps tax the acumen even of Mr. Boulenger; but the notes on their habits are too naïve. The garden-toad, we are informed, "makes a form in the grass during the hot weather in which to shelter himself; and should you come upon him, he will squat with his bright eyes fixed upon you all the time." This merely records a fact known to every one; but what shall we say of the following concerning the running toad? "The chief thing in connection with this creature is the rockstaff that a man can quiet the most restive horse with the bone of a running toad, which, it is said, will swim against the stream. Yacht designers and others might well look into the matter." Apart from the grammar, what a rockstaff is, we do not know, and we are equally ignorant whether it is the toad or its bone that can swim against stream. A lack of information as regards species and habits is also displayed when the author comes to eels; and he seems to be totally unaware that some years ago the late Surgeon Day communicated an important paper on the breeding of these fishes to the *Proceedings* of the Cotteswold Naturalists' Field Club.

As to the literary style of the book, perhaps the less said the better; and although it may attain a popularity among the numerous frequenters of the Norfolk Broads, it is to be feared that it cannot take a high rank among zoological works.

R. Lydekker.

OUR BOOK SHELF.

Object-Lessons in Botany. Book ii., for Standards iii., iv. and v. Being a Teacher's Aid to a Systematic Course of One Hundred Lessons for Boys and Girls. By Edward Snelgrove, B.A. London: Jarrold and Sons.

It is not perhaps very often that elementary scientific books of the type to which the volume before us belongs, either meets with, or indeed deserves, much success. It is with the greater pleasure, then, that we feel that the author is to be congratulated on having succeeded in producing a really good series of lessons which will be most useful, either in guiding teachers in arranging their class work, or in enabling a student to acquire a knowledge of plants for himself. The series of lessons is progressively arranged, beginning with the simpler forms of leaves and stems, and passing on to the various types of flowers and fruits. The really excellent feature of the work is the method by which the student is led to examine actual plants. The book would probably be of little service to any one merely desirous of "getting up" the subject without troubling to form any practical acquaintance with the objects dealt with. The examples selected as types are well chosen, and the student or teacher receives plenty of hints as to other forms which he may usefully compare with them. Almost the only fault we have to find with the book is, after all, only a literary one; still, it seems a pity that the generic names of the plants should have been commenced with a *small* letter, especially in the chapters on botanical names. This, however, is a defect that can easily be remedied in a future edition, which soon should be needed, for we can cordially recommend the volume, both to the elementary teacher and student, as a thoroughly good one.

Dental Microscopy. By A. Hopewell Smith, L.R.C.P., L.D.S., &c. Pp. 119. (London: The Dental Manufacturing Company, Limited.)

STUDENTS of dental microscopy will find this work a valuable guide to the preparation, observation, and photography of microscopical sections of hard and soft dental tissues. The volume is practical throughout, and is illustrated by eight lithographed plates, from which typical structures may be readily recognised. It should prove of great assistance to workers in dental histology.

Organic Chemistry, Theoretical and Practical. By Prof. J. S. Scarf, F.I.C., F.C.S. Pp. 240. (London and Glasgow: W. Collins, Sons, and Co., Limited.)

WE find no feature which distinguishes this text-book from others "adapted to the requirements of the Science and Art Department, and of the London University." The book may assist students to pass the examinations for which it has been constructed, but it is not a desirable introduction to the science of organic chemistry.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Antiquity of the Medical Profession.

DR. BLACK displays a surprising facility of misapprehension greater than I should have supposed possible.

The final sentence of his letter runs thus:—"It would seem, then, from history, that the medical profession is quite as old as either that of theology or law."

Now since the first sentence of my essay contains the clause—"In rude tribes it is difficult to distinguish between the priest and the medicine-man"; and since various illustrations are then given of the union of the priestly and medical functions in the same individual; and since it is thereafter shown that this union long continues among early civilised peoples Egyptians, Babylonians, Hebrews, Hindus, Greeks it is a necessary implication that, as Dr. Black says, "the medical profession is quite as old as either that of theology or law." For if two professions are at first exercised by the same persons they are necessarily of equal antiquity. So that, strangely enough, Dr. Black points out to me a truth which it is one of the purposes of my essay to teach. I can only suppose either that he did not read the first part of the essay at all, or that before he had reached the end he had forgotten the beginning.

Westerham, Kent. HERBERT SPENCER.

Halley's Equal Variation Chart.

I HAVE read Mr. Ward's interesting letter on this topic in NATURE of May 30, p. 106. I embrace this opportunity to correct some typographical errors in my letter in the issue of May 23.

No. 974 (4) should be 977 (4).

In foot-note 3, p. 79, the title of atlas referred to should be 'Tabulæ Nautic*al* Variationes Magneticas Denot*antes*."

I have compared Mr. Ward's description of his own chart with my notes. He evidently is the lucky possessor of the exceedingly rare Halley chart 977 (4). I should be pleased to have him inform me if the word "Britanniæ" in the dedication is not spelt with two *t's*.

The size of the British Museum copy is about 48 × 57 cm., the shorter dimension being in an east-west direction: it is in a splendid condition.

The earliest mention made of Halley's Equal Variation Chart is found in "Histoire de l'Acad. de Paris," 1701, p. 9. The chart referred to there must be the above 977 (4), of which we now know that two copies exist the British Museum's and Mr. Ward's. L. A. BAUER.

The University of Chicago.

The Invention of the Net.

IN your number of February 28 (p. 417), Mr. R. I. Pocock suggests that the observation of a spider's web may have given rise to the art of netting. It is of interest to note that the following citation is found in a Chinese cyclopædia: "Yuen-kien Lui-han" (1701, tom. ccccxlix. *art.* "Chi-chu," 2):—"In 'Pau-puh-tsze' it is said, 'Tai-hau [or Pâo-hsî] made a spider his master and knitted nets.'"

In the "Yih-King," the oldest authority that ascribes to Pâohsî the invention of the net, no mention is made in this connection of spider (see Legge's translation, in the "Sacred Books of the East," vol. xvi. p. 383); but the above-quoted passage of "Pau-puh-tsze" is tantamount to prove such a view, as suggested by Mr. Pocock, to have already occurred among the Chinese in the fourth century, A.D., when the book was written by a Taôist recluse named Koh Hung.

June 17. KUMAGUSU MINAKATA.

The Bird of Paradise.

I DESIRE to call the attention of your readers to a fashion which in the month of May was at its height in London, and is now much patronised throughout the country. I refer to the wearing in hats and bonnets of a graceful spray of soft fine plumes with drooping or curly tips. These the milliners call Bird of Paradise feathers, the assurance being constantly given that they are *real.* They are often mixed with osprey tips, which, to the shame of womanhood, have so long been in fashion, and are still largely used. I may state on trustworthy authority that during the past season one warehouse alone has disposed of no less than *sixty thousand dozens* of these mixed sprays!

The Bird of Paradise most used in millinery is that obtained in the Papuan Islands and New Guinea. Mr. Wallace, in describing the *Paradisea apoda*, says: "From each side of the body, beneath the wings, springs a dense tuft of long and delicate plumes, sometimes two feet in length, of the most intense golden-orange colour and very glossy, but changing towards the tips into a pale brown. This tuft of plumes can be elevated and spread out at pleasure, so as almost to conceal the body of the bird." In his "Oiseaux dans la Mode" of October 20, 1894, M. Jules Forest bitterly deplores the destruction which has been going on during the last decade. He emphasises the fact that it is no longer possible to procure such perfect specimens as were common ten years ago, since the unfortunate birds are so hunted that none of them are allowed to live long enough to reach full maturity, the full plumage of the male bird requiring several years for its development! He further states that "the birds which now flood the Paris market are for the most part young ones, still clothed in their first plumage, which lacks the brilliancy displayed in the older bird, and are consequently of small commercial value." Since January 1, 1892, strict regulations for the preservation of the Bird of Paradise have been in force in German New Guinea, and M. Forest appeals to the English and Dutch Governments to follow their good example.

The common sense of every thoughtful woman must at once tell her that no comparatively rare tropical species, such as the Bird of Paradise, can long withstand this drain upon it, and that this ruthless destruction, merely to pander to the caprice of a passing fashion, will soon place one of the most beautiful denizens of our earth in the same category as the Great Auk and the Dodo.

The women of England are earnestly entreated not to countenance the sacrifice of this bird by encouraging the demand for its precious feathers. Let them resolve to do what they can to prevent the extermination of this "wonder of nature" by stoutly refusing to purchase or wear anything purporting to have once belonged to a Bird of Paradise.

MARGARETTA L. LEMON.

Redhill, Surrey, June 21.

THE TICK PEST IN THE TROPICS.

THOSE living in temperate climates have probably small idea of the virulence of insect and other pests in the tropics. A plague of caterpillars may destroy a season's crop in England, but there is the winter's frost to be passed through before a second attack need be feared. It is otherwise in the tropics. Vegetation is much more luxuriant, and the food supply is permanent; and, when once a plague has obtained a firm foothold

there is no apparent reason why it should cease its ravages before it has entirely destroyed its particular host. It is fortunate for agriculturists that the great increase of any particular parasite seems ultimately to work out its own destruction; and frequently when all hope seems over, the plague rapidly and unaccountably disappears.

Surprise has been expressed that ticks infesting cattle have received so little real study. Quite recently the statement appeared that these parasites formed the least known part of the tropical fauna. But a great deal has been done in this direction of recent years, and there seems some hope of real progress being made.

Taking the conditions into consideration, it is a matter of great wonder that so few ticks exist in many parts of the tropics. No real attempt has been made to decrease their numbers, and there appears to be no season of the year when the climate is fatal to them. Vegetation is rank, and we know now that they can live to a great extent upon vegetable matter; further, even where there is a scarcity of small indigenous mammals, there are plenty of horses and cattle. The multiplying powers of ticks are enormous. In one case I determined the number of eggs from one female as over 20,000 (see Fig. 3), and almost all of these were fertile and produced young ticks. The increase in numbers of ticks in most countries is not marked, however, and we are driven to the conclusion that there is here, in the animal kingdom, a waste of material analogous to that in the seeding of parasites and saprophytes among plants.

It is not surprising now and then to hear of a long-continued plague of ticks from one place or another where cattle-rearing is a staple industry. In Jamaica, it is by no means uncommon for the traveller to get covered with "grass-lice." On pushing aside the branches overhanging the riding path, I have been immediately covered with firmly attached young ticks which needed much care and patience to remove. The ticks of Jamaica are now a very serious source of anxiety in cattle-pens, and much loss is attributed to these parasites.

During my stay in Antigua, complaints were loud and frequent of the ravages of a large tick, which infested the cattle between the months of May and September. In the cattle and sheep farms of the Cape of Good Hope and Australia the "tick" matter is absorbing much attention. Specially large and annoying forms are described from parts of India, Central Africa and Central America; while extraordinary tales are told of the destruction caused by these parasites in cattle-rearing districts of South America. Elaborate and expensive researches have been conducted in the United States Southern Experimental Stations upon the life-history of the ticks and their relations to cattle; and the exhaustive reports, issued from the Bureau of Animal Industry, form by far the most valuable part of our economic literature on these pests.

The books of travellers teem with references to the annoyance caused by ticks. Sir Joseph Hooker, in his "Himalayan Journals," describes their abundance in the frontier regions between Sikkim and Nepaul, in pathless tracts destitute of animal life. He writes the following concerning the neighbourhood of Tonglo: "A large tick infests the small bamboo, and a more hateful insect I never encountered. The traveller cannot avoid these insects coming on his person (sometimes in great numbers) as he brushes through the forest; they get inside his dress, and insert the proboscis deeply without pain. Buried head and shoulders, and retained by a barbed lancet, the tick is only to be extracted by force which is very painful. I have devised many tortures, mechanical and chemical, to induce these disgusting intruders to withdraw their proboscis, but in vain."

Bates, on passing through the grassy lanes of the second-growth woods on the Amazons, often found himself covered by ticks. It occupied him, he says, a full hour after his day's work to clear himself of the parasites.

Belt refers to the "grass-lice" on the plains of Nicaragua, as quickly covering any one travelling through the country; so much so, that the herdsmen or "vacqueros" keep a ball of soft wax with which to rub themselves. The smaller ticks are thus removed from their skin, while the larger ones are picked off by hand.

Many a time, in walking through grass in the Leeward Islands, I have been conscious of the peculiar itching at the ankles caused by the attacks of "bête rouge." The bête rouge is not in reality a tick, although often confused with it. Horses seem to be particularly liable to its attacks, with the result that they lose all the hair about the face and eyes. In all probability the poor animals suffer a good deal, for the personal irritation is extreme. The bête rouge is exceedingly minute, and, as its name implies, is of a brilliant scarlet. At night, after retiring to rest, the warmth of the body seems to increase the irritation to the utmost pitch, and sleep becomes absolutely impossible. Rubbing or scratching the parts attacked merely intensifies the discomfort, the creature pushing itself deeper into the flesh. Most painful sores are the result if the greatest care is not taken. The one certain remedy seems to be to anoint the inflamed spots with vaseline. This substance not only soothes, but appears to destroy the bête rouge by stopping up its breathing pores. I have never succeeded in detecting the creature on the skin, but, when reading in or near an infested lawn, I have captured many by watching for the minute scarlet dots travelling over the white paper.

The damage done by ticks to cattle is undoubtedly very serious. According to observations by Leidy, the adult female tick is able to absorb 100 times its weight of blood, swelling during that time to an enormous extent. This food is rapidly changed into eggs. The adult male does not increase appreciably in size, but his demands upon the host have probably been greatly underrated. An account of tick-infested cattle in Queensland states that they were so completely covered that the branding-iron had to be burnt through the ticks before it was possible to reach the animals' skins. A case in Texas is mentioned where it was found impossible to lay a silver dollar upon the body of the animals without touching some ticks. Again in Texas, 100 full-grown ticks were collected from each ear of a pony, while many immature ones were left behind. The mere abstraction of blood must, in this case, be a very serious drain upon the system.

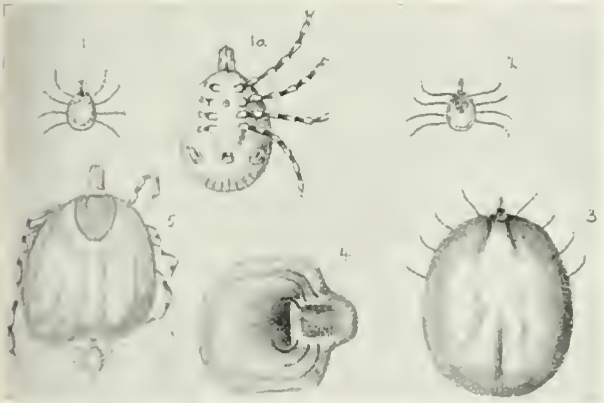
When one considers, further, the irritation experienced by travellers from the few ticks fixed upon them in their daily rambles, it may be safely concluded that the penetration of the countless proboscides into the skin of cattle must of itself be a source of great discomfort, especially as these animals are quite unable to get rid of them. Calves not uncommonly are destroyed by the formation of balls of hair in their stomachs; and in tick-regions this is undoubtedly due to an attempt to get rid of the parasites by licking and biting them off.

It is quite conceivable, then, that ticks do really cause the death of multitudes of cattle on the great estates where it is impossible to examine them closely. We should, however, approach this part of the subject with caution. Sickly cattle are usually covered by ticks, while the healthy ones have only a few; but it is questionable whether the ticks are the real cause of their emaciation. The case of ticks seems rather to be analogous to that of scale insects on plants. The latter pests appear in great quantities at any period of stress, when from lack of nutriment or other cause the plants become weakly. Thus, in Antigua, there is a marked disappearance of scale insects with the commencement of the rainy season. It seems probable that the prevalence of ticks upon certain cattle is rather due to conditions of the blood or skin of the animal, closely connected with its general nutrition. This is an exceedingly important matter for determination, for upon it, as will presently be shown,

depends the only means of freeing the cattle from these pests.

Thus far the direct effects of ticks upon cattle have been considered. Certain alarming facts have lately been brought to light with regard to the relations existing between ticks and different well-known cattle diseases. The subject is by no means new, having long been a fascinating one for cattle-breeders. The "louping-ill" or "trembling" of the north of Britain has been traced by some directly to the presence of ticks upon the sheep. The same may be said of a disease called "heart-water" at the Cape of Good Hope. Finally, the United States Department of Agriculture has for the last five or six years been conducting exhaustive experiments upon the connection between ticks and the Texas cattle fever, the results of which have appeared in the annual reports of the Bureau of Animal Industry already referred to.

There is, in this latter case, present in the blood of the cattle suffering from disease, an infusorian which quickly destroys the red blood corpuscles. This minute organism has also been detected in the body of the tick. It has been again and again transferred from diseased animals to healthy ones by means of the tick, and tick alone. The presence of this infusorian is regarded as diagnostic of



The Great Antigua Gold Tick.—*Hyalomma venustum*, Koch. (1) Mature male, natural size; colours, gold, scarlet, and black. (1*a*) Magnified ventral view. (2) Female, mature but not inflated; colours, shield black with flesh-coloured and gold spots; body dark green. (3) Female full of blood, natural size; colour, dark green. (4) The same female as in (3), after 20,000 eggs had been laid. (5) Female into which male had accidentally inserted his proboscis; both magnified.

the disease; and the effect of its corpuscle-destroying powers is seen all over the body, as well as in the red-coloured urine, which has won for the disease the colonial name of "red-water."

Ticks, then, are in certain cases connected with the transmission of deadly disease. In how many more cases this is so remains to be investigated. It is quite possible that some of the obscure cattle diseases in different parts of the world are caused by ticks, and that other countries will, in their turn, be forced to face this problem.

There is now and then an outbreak of a severe skin disease among cattle in Antigua; and this disease does not appear to be known in the neighbouring islands. Judging from the climate and peculiar conditions of Antigua, the scarcity of water and lack of nutritious food for part of the year might be considered sufficient to account for a local disease; but there is also a large tick present, which has not been recorded from the other islands of the group. A loose theory has thus arisen that this "gold tick" is connected with, if not the direct cause of, the cattle disease.

The evidence available does not tend to confirm this idea, but it is obviously impossible to solve the problem in the absence of proper appliances. I was led, however, to commence observations upon the gold tick, which may be of interest.

Mr. A. D. Michael has determined it to be *Hyalomma venustum*, which Koch described in 1847 from a single male specimen collected in Senegal. There is a local tradition in Antigua that the tick was introduced some thirty or forty years ago with some imported Senegal cattle; and this determination lends probability to the belief. The male is a very beautiful creature, decked in scarlet and gold, whence he obtains his name. The female is very large, one specimen being nearly an inch in length and weighing ·17 oz. I calculated the number of eggs laid by this female at over 20,000. She commenced laying on July 31, and finished, a shrunken mass, on September 10—a period of exactly six weeks. The accompanying life-size drawings are of Antigua gold ticks. The first is a mature male. He is not usually larger than this, and may be seen moving rapidly across the ground, or firmly attached to the skin of the cattle close to a female. The next three figures are of females, all mature, but at different stages. The first is undistended; the second gorged with blood, and commencing to lay its eggs; while the third is the same tick after the last egg was laid. There is also the drawing of a curious case, in which a male had by accident attached himself to a distending female—a mistake which resulted in the premature death of both.

The period of incubation observed in the tick's eggs varied from twenty-three to fifty-one days. The young ticks usually emerged in great numbers on the same day, and any eggs left unhatched quickly dried up. In Antigua the gold ticks appear upon cattle, in numbers, from May till September each year. It became important to determine what became of them in the meantime; and whether they passed the winter in the body of the parent, in the egg, or as young ticks. From experiments in the laboratory, it would appear that the little ticks pass the winter months huddled together in masses of several hundreds at the roots of the old dead grasses.

In considering the remedies for ticks, one is soon forced to the conclusion that direct measures against the parasite themselves will be of little avail. Methods of prevention are always preferable to those of cure, and in no case is this more so than with parasites of this class. Besides this, they are practically invisible at the most dangerous stage; and when we see the ugly, swollen, mature specimens, we know that their evil work is done. All large females should be carefully collected and burnt, however, as thus future attacks will be diminished.

The treatment of pastures is a very important matter. Here probably the parasite spends the greater part of his early life usually on the ragged bunches of old grass left from previous years. The proper feeding or cutting of the grass, and the liming and draining of the pastures, will destroy myriads of the infant ticks or "grass-lice." For the sake of the animals, there is every inducement to render the pastures as nutritious as possible; and ticks do not seem to trouble the sleek cattle of the herd. It is an undoubted fact, moreover, that the improvement in food, due to change of pasturage, does in certain cases cause all the ticks to drop off infested animals. The first class of remedies will aim at cutting off the supply of ticks by treating the pastures.

The second class—one might say almost the only one which is attempted in the tropics—is the destruction of ticks upon the cattle.

The common method of tying the legs of the animal together, hurling it to the ground, and smearing some tick-destroying compound over it, cannot be too strongly condemmed, especially as there is no need for it whatsoever. Cattle may be handled with impunity if some form of cattle-bail is employed; by this means they may be driven one by one into a small trap, where they

can be treated. But even this is hardly necessary if the application to the skin is in the liquid form; for with a powerful spraying machine, as many as one hundred cattle have been completely covered in the space of an hour.

Of pastes and powders and fluids recommended there is no end; and it will serve no useful purpose to give detailed lists discussing the merits of each. The points to be kept in view are that the liquid should be of an oily and non-poisonous nature, capable of clogging up the air-pores of the ticks. It should be cheap, and easily applicable without handling the cattle; it should, finally, not easily evaporate, or be washed off by the rains. A full discussion of remedies has recently been published by me, the following being taken from the summary at the end: "A number of types of washes for spraying are selected for description. All poisonous ones should be rejected, as there are non-poisonous preparations equally effective. Carbolic acid dips and other liquids, which evaporate quickly, need frequent applications, and should be discarded in favour of oily liquids or emulsions where the latter are equally effective. The best of all these is the kerosene emulsion regularly used for plants. There are many formulæ for the preparation of this; a useful one for ticks is given." The formula referred to is as follows: "In two quarts of boiling water dissolve half a pound of soap; remove from fire; immediately add one pint of kerosene, and agitate. In from three to five minutes the liquid becomes creamy. It may be stored in this form in bottles or barrels. For use, add three of water to one of emulsion; mix thoroughly, and apply with a spraying pump."[1]

The third and most important class of remedies is closely connected with the nutrition of the animal. If we can render the skin or blood of our cattle so distasteful to the tick that the latter will not attach itself, we have a solution of the whole matter. We should confer immunity upon our animals, and, at one stroke, do away with the necessity of all the laborious and expensive methods now in vogue for the destruction of these parasites.

The first step in this direction has been taken; and, in various parts of the world, most excellent results are recorded from the addition of small doses of sulphur to the animal's food.

It has already been noted that the food of animals has an influence upon their infestation by ticks. Cases are not uncommon among cattle-breeders where a mere change of pasture will cause all the ticks to drop off. This change is obviously felt through the animal's skin.

It has also been mentioned that the ticks seem to congregate upon cattle in poor condition, while those with sleek skins are more or less untouched. Dr. Cooper Curtice (late of the United States Bureau of Animal Industry) suggests, as an explanation of this, that there is in well-fed cattle an oily condition of the skin obnoxious to the ticks; and this suggestion is the more worthy of consideration when we remember the aversion of these creatures to grease of any kind.

It is certain that *sulphur taken internally* will render the skin evil-smelling, by the exhalation of sulphuretted hydrogen, a substance highly obnoxious to all parasites. The following seem to be the physiological changes which take place during the passage of the sulphur through the animal's body to the skin. Sulphur taken in with the food passes the stomach unaltered. In the intestines a small portion is changed into sulphides of hydrogen and the alkalies. Part of these sulphides pass into the blood, and into the tissues from the blood, and act chiefly upon the central nervous system. The sulphides in the tissues are variously excreted. By the kidneys they are excreted as sulphates; if in excess, part is also excreted in the form of sulphides. By the skin they escape as sulphides, giving the characteristic foul odour to the perspiration, and somewhat increasing its amount.

[1] For further details see paper in Leeward Islands *Agricultural Journal*, No. 1.

The doses of sulphur should be small, but they should be constant. The form in which the medicine is offered to the animals will best be decided by the manager of the estate. With stall-fed cattle there can be no difficulty at all; but with the cattle of large estates, which are seldom handled and sometimes not seen for long periods, it will be necessary to prepare the sulphur with salt as a "lick," to which cattle will readily help themselves if it is scattered about.

The success of this sulphur treatment has so far been encouraging, both at the Cape of Good Hope and in the United States. Doubtless with continued study other similar preventive remedies will from time to time be discovered, and thus rid the stockowners of the tropics of one of their most dreaded enemies.

C. A. BARBER.

NOTES.

PROF. HUXLEY'S health is at present a source of great anxiety to his friends. Symptoms of renal insufficiency appeared last week, and this, with the other complications which have attended his protracted illness, has made his condition a very critical one, but we are glad to learn that it is improving.

WE deeply regret to notice the announcement of the death of Dr. W. C. Williamson, Emeritus Professor of Botany in Owens College, Manchester. Dr. Williamson was elected into the Royal Society in 1854.

PROF. VERNEUIL, the eminent French surgeon, and Member of the Paris Academy of Sciences, died on June 12.

PROF. SIMON NEWCOMB has been elected Associé étranger of the Paris Academy of Sciences, in succession to the late von Helmholtz.

PROF. W. PETERSON, Principal of the University College, Dundee, has accepted the position of Principal of McGill University, Montreal, in succession to Sir William Dawson.

SIR E. MAUNDE THOMPSON, principal librarian of the British Museum, has been elected a Corresponding Member of the Philosophico-historical Section of the Berlin Academy of Sciences.

THE University of Pennsylvania has received gifts, within a few days, aggregating nearly a million dollars. This includes half a million dollars from Provost Harrison, already noted in these columns. Scarcely a week passes without our being able to record similar gifts from private benefactors to the universities and colleges of the United States. *Science* reports that Dr. D. K. Pearson has offered £10,000 to Mount Holyoke College if an additional £30,000 can be raised. It is said that Dr. Pearson has already given £400,000 to various colleges.

THE death is announced of Dr. A. Eliseief, known for his explorations and anthropological work.

THE St. Petersburg correspondent of the *Lancet* reports that the Emperor of Russia has appointed a committee to organise the collection of subscriptions for the monument which the Institute of France propose to erect to Lavoisier.

THE trustees of Columbia College decided, a few days ago, to grant the Barnard Medal to Lord Rayleigh and Prof. Ramsay jointly for their discovery of argon. Only Lord Rayleigh's name was mentioned in the previous announcement of the award.

DR. BACKLUND has been elected a Correspondant of the Paris Academy, in the Section of Astronomy, in the place of the late

M. Wolf: and Prof. Kowalewsky has been elected to fill the late M. Cotteau's place as Correspondant in the Section of Anatomy and Zoology.

The French Association for the Advancement of Science will meet at Bordeaux, from August 4 to August 9, under the presidency of M. E. Trélat. Applications for membership should be addressed to the Secretary of the Association, 28 rue Serpente, Paris.

The third international meeting of Psychologists will be held at Munich from August 4 to 7. The first meeting was held at Paris in 1889, and the second in London in 1892. Prof. Stumpf, of Berlin, will act as President, and Dr. von Schrenck-Notzing, of Munich, as General Secretary.

The second Italian Geographical Congress will be held in Rome next September, under the patronage of the King of Italy and the Duke of Genoa. The President of the Congress will be Marquis G. Doria, President of the Societá Geographica Italiana. The Secretary is Prof. D. Vinciguerra, and his address is Via del Plebiscito, 102, Roma.

Dr. T. G. Brodie has succeeded Prof. C. S. Sherrington, F.R.S., as Lecturer on Physiology at St. Thomas's Hospital.

Prof. E. Hering, of Prague, has been proposed as successor of the late Carl Ludwig in the chair of Physiology at Leipzig.

Prof. E. Mach, of Prague, well known by his book on Mechanics, and by his experimental researches on Physics, has been appointed Professor of Philosophy at the Vienna University. Vienna will, therefore, be the first place where Philosophy will be taught on a modern and scientific basis.

The Cracow Academy of Sciences offers prizes of 1000 and 500 florins for the best discussion of theories referring to the physical condition of the earth, and for the advancement of an important point connected with the subject. Memoirs must be sent in before the end of 1898.

The International Conference on the Protection of Wild Birds met at Paris on Tuesday, under the presidency of M. Gadaud, Minister of Agriculture. England was represented by Sir Herbert Maxwell, Mr. Howard Saunders, and Mr. F. Harford, of the British Embassy at Paris. Belgium, Holland, Germany, Russia, Austria-Hungary, Luxemburg, Switzerland, Italy, Greece, and Spain have also sent delegates. The conference meets as the result of a resolution passed at the International Agricultural Congress held at the Hague in 1891.

At the recent annual meeting of the Royal Society of Canada, the following officers (says *Science*) were elected for the ensuing year:—President, Dr. R. S. C. Selwyn, C.M.G., F.R.S.; Vice-President, the Archbishop of Halifax, Dr. O'Brien: Secretary, Dr. J. G. Bourinot, C.M.G.; Treasurer, Prof. J. Fletcher. Prof. Bovey, Dean of the Faculty of Applied Science, McGill University, was chosen President of the Section of Mathematical, Physical, and Chemical Sciences, Prof. Dupuis, Vice-President, and Captain E. Deville, Surveyor-General of the Dominion, Secretary. In the Section of Geological and Biological Sciences the following choice was made:—President, Prof. Wesley Mills; Vice-President, Prof. Penhallow; Secretary, Dr. Burgess.

At the annual general meeting of the Numismatic Society of London, held on Thursday last, Sir John Evans, President, in the chair, the silver medal of the Society was awarded to Prof. Theodor Mommsen, for his distinguished service to the science of Numismatics. Dr. Barclay Head, keeper of coins in the British Museum, in returning thanks on behalf of Prof. Mommsen, drew attention to the fact that quite recently Mommsen had handed over to the Royal Academy of Sciences of Berlin the sum of 25,000 marks, presented to him as a testimonial from his disciples in all countries on the occasion of the jubilee of his Doctorate, with directions that it should be devoted to the compilation and publication, under the auspices of the Academy, of a complete *corpus* of all known extant Greek coins.

Few neighbourhoods offer more features and objects of interest than the district around Galway. An excursion to this district, arranged by the Irish Field Club Union, will therefore probably be a very successful one. The country west of Galway presents the geologist with a great variety of rocks and rock structures. Some of the most interesting studies in Ethnography afforded in the British Isles may there be found, and the antiquarian and archæologist are offered exceptional attractions. The party will meet at Galway on Thursday, July 11, and will stay in the neighbourhood until the following Wednesday. The places to be visited are: The Twelve Bens, Connemara, Ballyvaughan and the Burren district, the Aran Islands, Oughterard and Lough Corrie. A programme, containing notes on the topography, geology, botany, zoology, ethnography, and archæology of these places has been prepared. During the reunion, a conference will be held for the consideration and discussion of matters relating to the advancement and extension of Field Club work in Ireland. The Secretary of the Union is Mr. R. Lloyd Praegar, National Library, Dublin.

It has long been known in a general way that the time required for hatching out the eggs of cold-blooded animals is dependent on the temperature at which they are kept; and that in the case of birds "the period of incubation is much related to the size of the bird." Mr. A. Sutherland (Roy. Soc. of Victoria, December 1894) has recently made some observations on the relations between hatching-time and temperature, and formulates a law based upon his results. He has further investigated incubation among birds and gestation. Birds and mammals keep at a practically constant temperature—between 37° C. and 43° C.; and it may be assumed that sitting birds keep their eggs at a tolerably definite temperature. Why then should the period of incubation or gestation vary so much? Mr Sutherland asserts that the time of incubation or gestation, as the case may be, has a certain definite relation to the weight of an animal. He states the two laws he has arrived at in the following words:—(1) "For animals of the same size the time of embryo development is inversely proportional to the square of the temperature, that temperature being reckoned from a definite point." (2) "At the same temperature, the period of development is directly proportional to the sixth root of the weight of the mature animal."

A few months ago, M. de Montessus published an interesting paper on the frequency of earthquakes, of which a summary is given in a previous note (vol. li. p. 540). This he has followed up by another paper of still greater value on the relation between seismic frequency and the relief of the ground (*Comptes rendus*, vol. cxx. pp. 1183-1186). The following are the general conclusions at which he has arrived from a study of 348 regions, in which 9700 earthquakes and 5000 volcanic eruptions are known to have occurred. In a group of adjoining districts, the most unstable are those which present the greatest differences of relief, *i.e.* those whose average slope is greatest. The unstable regions follow the great lines of folding of the earth's crust. Mountainous countries are generally more unstable than flat ones, and, in any one mountain-chain, the

short and steep slope is the more unstable of the two, especially in its steepest parts. Coast regions with a rapidly deepening sea are unstable, especially if bordered by an important mountain-chain: those with a slightly sloping sea-bed are stable, especially if they adjoin a flat country. Lastly, in regions which are frequently disturbed by earthquakes, and which at the same time possess very active volcanoes, the seismic frequency and volcanicity are independent. It follows, therefore, that earthquakes are a purely geological phenomenon, and probably have their origin in the same dynamical forces to which the present relief of the earth's crust is due.

RADIOLARIAN earth of Tertiary age has long been familiar from Barbados: in a recent number of the *Bull. Museum Comp. Zool.* (Harvard), Mr. R. T. Hill records it from the island of Cuba. It occurs at one place only, near Baracoa, where it is over 500 feet in thickness and is well stratified, the strata being vertical. The rock is chalky in appearance, with occasional thin separation-layers of a grey-blue clay, and some flint-like siliceous nodules: sponge-spicules and echinoid fragments were found in it, but no diatoms. It appears to lie below certain yellow beds identified as Miocene. The paper contains much other information on the geology of Cuba, and the origin of the circular harbours of the north coast is dealt with. The author finds no evidence of any movement of depression in the island since the beginning of Tertiary times.

DR. F. KLENGEL, of Leipzig, has sent us a copy of his paper, read some time since before the Bohemian Society of Sciences, on the non-periodical variations of temperature in the district of the Pic du Midi and Puy de Dôme, compared with those at St. Bernard, for which station a longer series of observations is available. The problem undertaken by the author was mainly to show how far the irregular variations of temperature in these three widely separated and high regions of Central Europe agreed together. The most important conclusions drawn from various tables are, that a remarkable agreement is shown in the non-periodical changes at the mountain stations, whereas in the plains the variations differ materially from each other. The influence of the sea is visible in the lower region of of the Pic du Midi, but at the higher level it entirely disappears.

THE papers in the June *Journal* of the Royal Microscopical Society include one on British patents taken out in connection with the microscope, between 1666 and 1800.

THE Department of Mines of Victoria has issued a report on the Victorian coalfields, the development of which is proceeding rapidly. Evidence is given to show that the coal is of drifted origin: among other points, the mixture of conifers and ferns in the flora can only be explained by transport before deposition.

MR. JOHN TEBBUTT has sent us a report of the work done at his observatory, Windsor, New South Wales, during 1894. Meteorological observations have now been made at the observatory for thirty-two years. Among the astronomical work of last year were observations of lunar occultations of stars, of southern comets, and of double stars.

THE sixty-first annual report of the Natural History, Literary, and Polytechnic Society of York School gives evidence of enthusiastic work in many branches of science. Few school societies of a similar kind can boast of reports running into the sixties. With this report we received the *Natural History Journal* and *School Reporter* for June 15, conducted by the societies in Friends' schools. The journal contains articles on Southern Tyrol and on the planet Mars, as well as notes, and records of observations of scientific interest.

Bulletin No. 48 of the U.S. National Museum is devoted to "A Revision of the Deltoid Moths," by Prof. John B. Smith, the paper being a contribution towards a monograph of the insects of the Lepidopterous family Noctuidæ of Boreal North America. Fourteen plates, showing the different species of these Noctuids, and the structural characters of the Heliini, Herminiini, and Hypenini, accompany the descriptive text. The genera Pseudorygia and Rivula are not included in the series, Prof. Smith being of the opinion that they do not possess real Deltoid characteristics.

THE Report of the Geological Survey of Canada for 1894 describes the results of geological expeditions in the Labrador Peninsula and west of Hudson Bay. In consequence of lack of money it was found necessary to reduce the number of parties working in the field, while there is an accumulation of material awaiting publication. A deep boring for petroleum has been begun at Athabasca Landing, but at a depth of 1011 feet the oil had not been reached: all indications, however, point to the existence of great quantities of petroleum in the Devonian strata which immediately underlie the Cretaceous.

THE Central Physical Observatory of St. Petersburg has made an important addition to its comprehensive *Monthly Weather Report* by showing on a chart the deviations of temperature and rainfall of the month from the normal conditions. To arrive at this, M. Wild states that the values have been calculated for no less than 322 stations, all of which are represented in the report. The excess or defect of temperature at each place is shown on the chart by drawing curves through those places where the deviation is equal in amount, while the deviation of rainfall is represented by red and blue tints. The work is very neatly executed, and shows clearly, at a glance, the climatic conditions of the month.

THE 1895 *Photography Annual*, edited by Mr. Henry Sturmey, is an invaluable compendium of photographic information, and a useful record of the progress made during last year in the various branches of the science and practice of photography. In it Mr. C. H. Bothamley traces the advances of photographic chemistry; Mr. Chapman Jones describes the work done in the field of photographic optics; Mr. T. Bolas records the progress made in photo-mechanical printing; Captain Abney writes on spectrum photography; and Mr. Albert Taylor contributes a very full account of what was done in astronomical photography during 1894. These records, together with descriptions of new photographic apparatus and materials, technical articles, and particulars of photographic societies throughout the United Kingdom, render the *Annual* indispensable to all who take an intelligent interest in photography. The publishers are Messrs. Iliffe and Son.

THE current number of the *Comptes rendus* contains an account, by M. Berthelot, of a new combination of argon. Following up his researches on argon, this author has discovered that free nitrogen, prepared pure from nitrites, can be caused to enter into combination with the elements of carbon disulphide when subjected to the spark or silent discharge after saturation with disulphide vapour. The resulting compound contains some mercury sulphocyanide, and does not regenerate nitrogen under the action of heat or of concentrated sulphuric acid. When argon is employed in place of nitrogen, a similar reaction appears to take place. Under the continued action of the silent discharge, a sample of 6·55 c.c. of argon, as pure as it could possibly be obtained, saturated with carbon disulphide vapour

at 20° C., and confined in the reaction tube by mercury, gave a continuous absorption which appeared to go on indefinitely. The product contained mercury, but gave no reaction for sulphocyanide. When heated, a quantity of gas was recovered equal to about one half the volume absorbed, and this recovered gas was proved to be argon by condensation with benzene, and production of the remarkable fluorescence previously described. Though this work has been done on such small quantities of material that an exhaustive examination of the product was not possible, M. Berthelot believes that he has satisfactorily demonstrated the significant property of argon, that it can enter into combination and be regenerated from its compound or compounds with its initial properties intact.

As a result of observations carried on by the *Investigator* in the autumns of 1892-3-4, Commander C. F. Oldham, R.N., contributes two papers on the Laccadive Islands to the *Journal of the Asiatic Society of Bengal* (vol. lxiv. pt. ii. No. 1, April 1895). The group consists of four submerged coral-reefs, six reefs with small islets ("sand-cays"), and eight inhabited atolls: three of the reefs and five of the atolls were examined. The islands and sand-cays occur, in all cases but one, on the eastern side of the atolls; they cannot, therefore, have been built up by the action of the ordinary monsoon winds which blow mainly from the west, but must be due to the occasional hurricanes which reach the eastern and north-eastern sides of the atolls. The effect of the tides and currents is seen in the more vigorous growth of the atolls to the south and west. The islands and islets are extending at their extremities, and in some cases are being added to on the south-western sides where they face the lagoon. No evidence of either elevation or subsidence was observed.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mrs. Stevens; two Javan Parrakeets (*Palæornis javanica*) from Java, presented by Lieut.-General Sir H. B. Lumsden; a Green-winged Trumpeter (*Psophia viridis*) from Brazil, presented by Mr. H. A. Astlett; a Diamond Snake (*Morelia spilotes*) from Australia, presented by Mr. M. Mitchener; a Natal Python (*Python natalensis*) from South Africa, presented by Mr. William Norman; a Korin Gazelle (*Gazella rufifrons*, ♀) from Senegambia, a Blue and Yellow Macaw (*Ara ararauna*) from South America, a Naked-necked Iguana (*Iguana delicatissima*) from Tropical America, thirty-four Black Salamanders (*Salamandra atra*), South European, deposited; a Tachiro Goshawk (*Astur tachiro*) from South Africa, nine Red-beaked Weaver-Birds (*Quelea sanguinirostris*) from West Africa, purchased; a White-crested Jay Thrush (*Garrulax leucolophus*), a Striated Jay Thrush (*Grammatoptila striata*) from India, received in exchange; a Burrhel Wild Sheep (*Ovis burrhel*, ♀), a Patagonian Cavy (*Dolichotis patachonica*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE YERKES OBSERVATORY.—From a note in the *Astrophysical Journal* for June, we learn that the construction of the buildings of the Yerkes Observatory is advancing rapidly, and it is hoped the 40-inch refractor will be ready for use in September or October. The Observatory is situated on the shores of Lake Geneva, Wisconsin, at an elevation of 180 feet above the lake, and is about seventy-five miles from Chicago. The dome for the great telescope, which is being built by Warner and Swasey, is 90 feet in diameter, with a shutter opening 12 feet; the rising floor is 75 feet in diameter, and will have a vertical movement of 22 feet. The motive power for turning the dome and elevating the floor of the Observatory will be supplied by electro-motors.

In addition to the large telescope, provision is made for the use of the 12-inch telescope now at the Kenwood Observatory, and another telescope of 16 inches aperture. The meridian room is designed to accommodate a large meridian circle, but, in the first instance, a transit instrument will be employed.

The Observatory buildings appear to be designed on a very liberal scale, and comprise offices, library, lecture theatre, spectroscopic, physical, chemical, photographic, and other laboratories. We understand that Prof. Barnard and Prof. Burnham have accepted positions in the Observatory.

THE GRANULATION OF THE SUN'S SURFACE.—The granular or mottled appearance of the surface of the sun is familiar to all observers, and the great resemblance to terrestrial cirrus clouds has long been recognised. A possible cause of this appearance has been recently suggested by Dr. Scheiner (*Astr. Nach.* 3279), the idea being that Helmholtz's investigations on the formation of waves in our own atmosphere apply also in the case of the sun. According to Helmholtz, air waves are produced when two strata of air of different temperature and density glide over each other; if the lower layer is nearly saturated with aqueous vapour, the wave crests will be centres of condensation, in consequence of diminished pressure, and will appear as clouds, while the depressions will form transparent interspaces. On this theory a "mackerel sky" is produced when two series of waves cross each other. Dr. Scheiner points out that somewhat similar conditions prevail in the sun; there are layers of different temperature, and currents in various directions in these layers, and in the photosphere the condensable gases are in an over-saturated state. He therefore considers that the bright grains of the photosphere are wave-crests of two crossing systems of waves, rendered visible by an increase of condensation. In the case of the sun, the observed lengths of the waves—that is, the distance between the separate grains—is from 1000 to 3000 kilometres, and it is believed that waves of this magnitude might be produced without the assumption of extraordinary velocities.

Assuming this to be a true explanation, the photosphere must be a very thin layer; and since the granules are of about the same size in all parts of the surface, the velocity of the currents must be nearly equal in all heliocentric latitudes.

THE SATELLITES OF JUPITER.—Not contented with his brilliant discovery of a fifth satellite to Jupiter, Prof. Barnard has been employing the great resources of the Lick telescope in further investigations of the satellites which were discovered by Galileo (*Monthly Notices, R.A.S.*, vol. lv. p. 332). One part of his work has consisted of micrometric measurements of the diameters of the satellites, and the results, reduced to a mean distance of the planet from the sun equal to 5·20, are as follows:

	Angular diameter. ″	Diameter in miles.
Satellite I. ...	1·048 ...	2452
,, II. ...	0·874 ...	2045
,, III. ...	1·521 ...	3558
,, IV. ...	1·430 ...	3345

It is pointed out that these values are in good accordance with the mean values derived from nine sets of measures made by as many different observers since 1829. Of the earlier estimations, those made by Schroeter in 1798 agree most closely with modern results.

Special attention appears to have been given by Prof. Barnard to Satellite I., on which he discovered, with the 12-inch equatorial, on September 8, 1890, the existence of a bright equatorial belt and dark polar caps. These appearances have been verified at every favourable opportunity, and "they are, beyond question, permanent features of the satellite, and will always be visible when a favourable transit occurs." These markings on the satellite fully account for all the phenomena which have been reported of the distortion or ellipticity of its disc, as well as for the apparent doubling of the satellite during some transits. When the satellite is transiting over a dark part of the planet, the white belt appears very prominently, while the dark poles are correspondingly difficult to see, so that, without very close attention, the satellite looks like a thin white strip. If, on the other hand, it be transiting across a bright part of the planet, the white belt is lost in the bright background, and the polar regions appear as two separate dark spots, making the satellite appear double. The dark polar caps are darkest at the poles, and become rapidly less intense towards the equator. Prof. Barnard considers that the phenomena observed on this satellite indicate that its physical condition is similar to that of Jupiter.

THE SUN'S PLACE IN NATURE.[1]

VII.

AT the end of the last lecture, some evidence was brought forward which leads to the conclusion that in those stars in the spectrum of which bright lines are seen, we are dealing with bodies closely associated with nebulæ. It was at once suggested

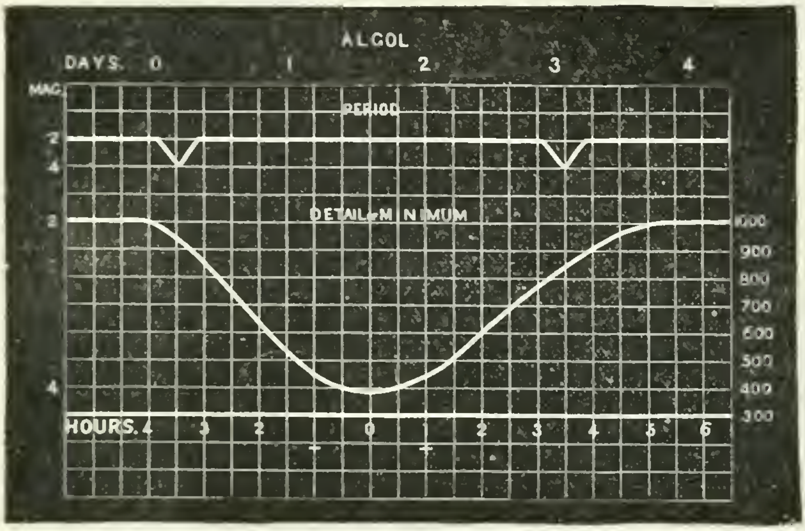


FIG. 2.—Light-curve of Algol.

that possibly by those new methods of inquiry to which I have already referred, we might be enabled to demonstrate the existence of the nebulæ, although we can never hope to see them by the unaided human eye. The idea occurred to me that long exposed photographs might give us stars surrounded by nebulæ. So I wrote to Dr. Roberts, who always kindly places himself at the disposal of any student, and asked him if he would be so good as to photograph that region of the heavens in which most of the bright-line stars have been observed. He at once acceded to my request, and took photographs, as desired, with his instrument, giving an exposure of three and a quarter hours. The result a little disappointed me, because he reported that there was no indication whatever of any nebulosity surrounding these stars. Possibly it was on this account that Dr. Huggins felt himself justified in objecting to the view which associated these stars with nebulous surroundings. But that is not the whole story. Some time afterwards, at the request of Mr. Espin, Dr. Max Wolf, who has an instrument which is even more competent to pick up faint details than the wonderful telescope employed by Dr. Roberts, took photographs of this same region; and I need not tell you that, being anxious to carry the inquiry as far as he could, he made the exposure what we should consider almost impossibly long—so long, in fact, that one single night was not sufficient. His first photograph of this region was exposed for thirteen hours on three nights; the next one was exposed for eleven hours. Now I will throw on the screen the result which was obtained by Dr. Wolf with the instrument which at the present moment is the most competent to give a verdict upon such inquiries as this. Here, in the first instance, we have a photograph of the region surrounding the brightest star in the constellation Cygnus, and you will observe that we have here and there indications of nebulous matter as well as of stars. That is rendered evident by the fact that in certain other regions we get a perfectly flat background, whilst in this the background itself is luminous. Now we come to the region in which these bright-line stars have been recorded for several years, and you see it is almost impossible to point out in this photograph a large area in which there is not a most obvious indication of this luminous nebulosity. Patches here and there seem to indicate that the great differentiation between this part of the sky and others, lies not in the wealth of stars, but in the wealth of the luminosity in which they are situated.

It was obvious therefore, from this experiment, that I was perfectly justified in stating that these bright line stars were associated with nebulæ, since we find the statement made on theoretical grounds now backed up by these exquisite data, which indicate that most certainly there is a complete association of nebulous matter with these stars.

I do not want to part with that diagram until I have pointed out to you the enormous advantage students of science now have in possessing such magnificent photographs as these. Not only is the wealth of science rendered obvious, but the wealth of nature. Here, you see, is what modern science makes of a little patch of the sky on which the naked eye sees nothing at all.

The conclusion is therefore this: there seems to be no doubt that bright-line stars are directly connected with nebulous matter. I am glad to add that this is also the conclusion of the American astronomers who have inquired into the subject.

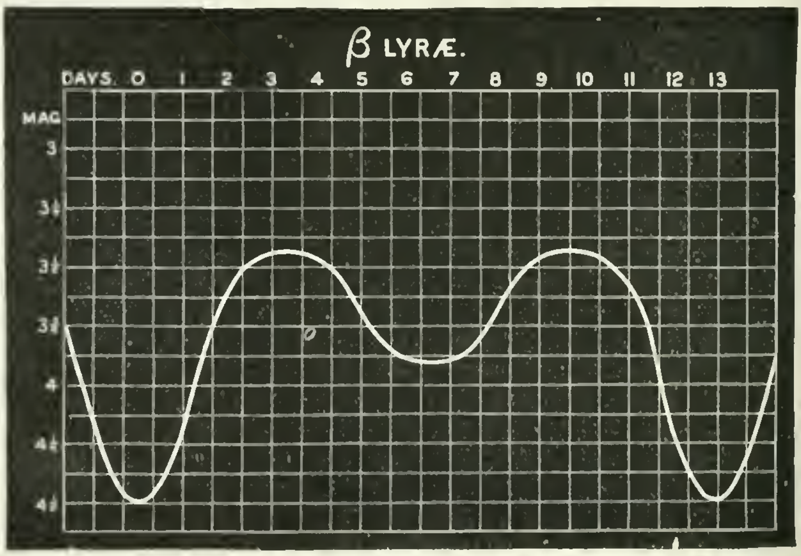


FIG. [illegible].—Light-curve of β Lyræ.

The next point in the meteoritic hypothesis—that some of the heavenly bodies are increasing, others diminishing their temperature—is one which I have brought out in that strong form, but I do not propose to say very much about it to-night. You may remember what has been said with reference to the hypothesis of Kant and Laplace, and especially Laplace's view that in the nebulæ we have to deal, as also in the stars

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December 1894. (Continued from page [illegible].)

associated with them, with gases at a very high temperature. Now, in the hypothesis which I have ventured to put before the world of science, I differ in this particular both from Laplace and also from Vogel, who has most industriously attempted to establish a classification of the celestial bodies. I pointed out that in accordance with thermo-dynamical principles, the temperature must increase with condensation, and of course it will depend, therefore, upon the condensation of the gas, whether we have to deal with high or low temperatures in the bright-line stars and the nebulæ. I wish to take this occasion to state that Prof. Darwin has recently shown, as the result of a most profound inquiry, that swarms of meteorites in space will behave exactly like a gas; therefore, what can be said of the thermo-dynamics of a gas may be said also of the thermo-dynamics of a meteoritic swarm.

Now we come to a very interesting part of the inquiry, because it lands us among phenomena which so far have been considered to be exceptional. I refer to the phenomena of the so-called variable stars. You will see in a moment that if there is any truth in what has been brought before you, the light of stars as they pass from the nebulous to the more luminous stage must change during the progress of that evolution. But remember, that change will not be visible to one generation of men, probably not to a thousand generations of men. It is a change which will require millions, and possibly billions, of years for its accomplishment; and therefore we must not associate the word "variable" with any change which depends wholly upon the evolution of these various stellar conditions. But in addition to that, we can see almost in hours, certainly in days, frequently in months, sometimes in years, changes in the light of certain stars: and it is these short period changes which mark out and define for us the phenomena of variable stars.

Take a star like the sun. It is pretty obvious to you that any change in the sun, such as we see it now, would require a very considerable time for its accomplishment, so as to be obviously visible to us all: but if you take two bodies like the sun, you might imagine a condition of things in which one body would come exactly in the line between the earth and the other body, and would so eclipse the further one. There you have at once the possibility of an eclipse due to the passage of one body in front of another, and therefore of a variability which depends upon eclipses. So much for two bodies like the sun; but we know that in various parts of celestial space some of the stars have run through their life of light, and exist as dark bodies. Obviously we should get the same eclipse phenomena when dealing with one star like the sun and another dark body, provided always that the dark body came and eclipsed the light one. That is a very well known and accepted cause of variability, and one of the most obvious cases of this kind we have in the star Algol. There we have two bodies, a bright and a dark one, and a diagram will give us what is called the light-curve, the curve indicating the variability brought out by such a condition as that I refer to. When we come to examine the light-curve of a body like this, we find that the luminosity of the star remains constant for some considerable time in relation to the period of variability, and then it suddenly decreases. It almost at once—in an hour or two—goes up again, continues then for another period, and suddenly diminishes again (Fig. 29).

Spectroscopically we can inquire into the question as to whether there is or is not any physical change connected with this. Obviously, if it is merely an eclipse, there should be no physical change, and therefore no change in the spectrum. Here, by the kindness of Prof. Pickering, I can show you two photographs of the spectrum of this star, when it is most luminous, and when it is least luminous, and the spectra of these two conditions are, you see, quite similar. The broad lines are alike; in other dark lines also there is no change. Therefore, spectroscopically, we are justified in saying that the theory that variability is caused by eclipses is a perfectly justifiable one.

But supposing we consider no longer two bodies like the sun, or even one sun and another body more condensed and colder than the sun, but two not completely condensed meteoritic swarms; various probabilities never before considered will lie open to our inquiry.

We may take the remarkable case of variability presented to us by one of the brighter stars in the constellation of the Lyre, β Lyræ. The spectrum of that star has been very carefully studied, and if you will look at the details now on this diagram, you will see a series of the most marvellous spectral changes showing at once that we are not in the presence of phenomena at all similar to those presented in the last star examined. Fig. 30 shows the light curve of β Lyræ, which when at its lowest brightness is a $4\frac{1}{2}$ magnitude star, and at its greatest brightness is a $3\frac{1}{2}$ magnitude star, the changes going through one magnitude. In this scale you see that the changes are run through in a period of thirteen days. From the period of the greatest obscuration of

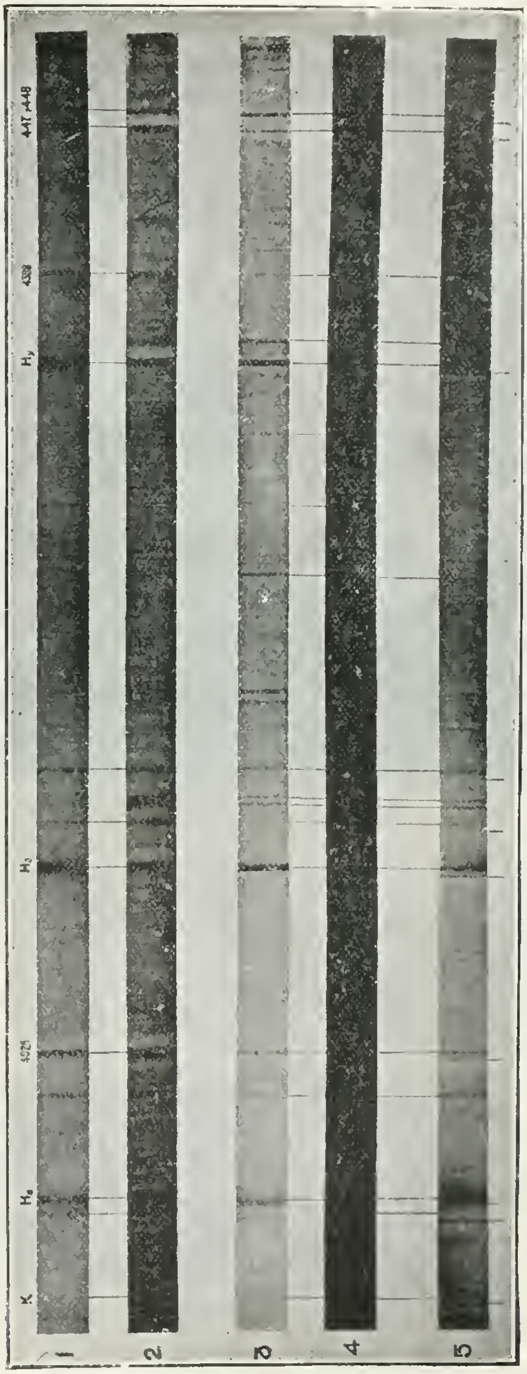


FIG. 31.—Spectra of β Lyræ (2, 3, 5) compared with Bellatrix (1) and Rigel (4).

light, in nearly three days we get to the highest luminosity, then at the sixth day we get to what is called a secondary minimum, *i.e.* the light has gone down a bit, but not so much as it had done at the beginning of this light cycle: then it goes up again, so that on the tenth day we get a maximum of light such as we had on the third day: after that it goes down, so that on the

thirteenth day, or thereabouts, we get to another minimum, and then the cycle begins again. Associated with these changes we have considerable changes in the spectrum. We have been fortunate enough to get a spectrum of this marvellous star for every day included in this period of change, although of course the photographs have not been taken in a period of thirteen days or in ten periods of thirteen days; but by knowing this period, we have been able to place the different photographs

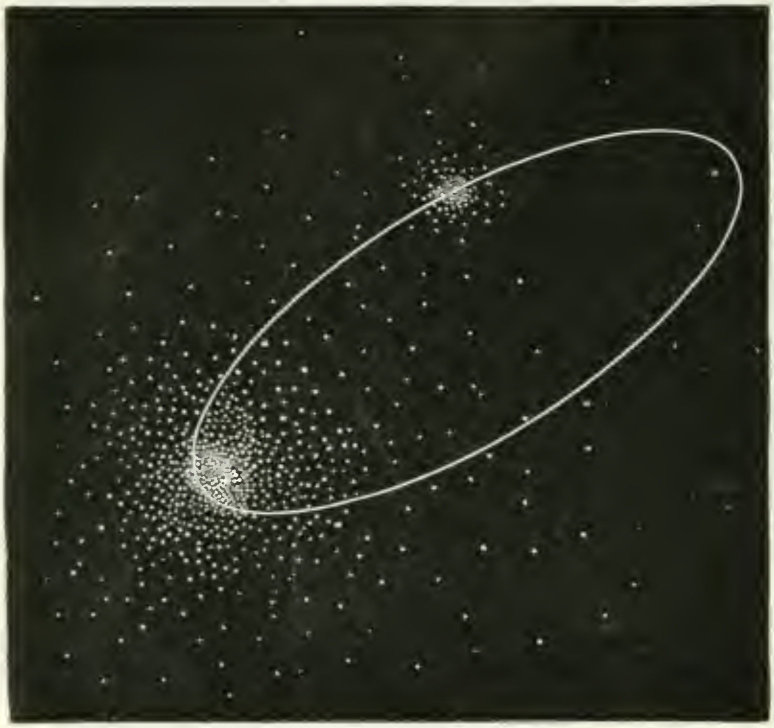


FIG. 32.—Cause of variability in uncondensed swarms.

together so as to see exactly what happens. We get bright lines and dark lines, and bright lines changing their places; but the main point we have been able to make out so far, is that we are dealing with two stars very much like a number of stars that we see in the constellation of Orion. In Fig. 31 we have photographs of the spectra of two of the stars in the constellation of Orion, and associated with them, three photographs of the spectrum of β Lyræ; from the change in the position and coincidence of these lines we are able to make out that the

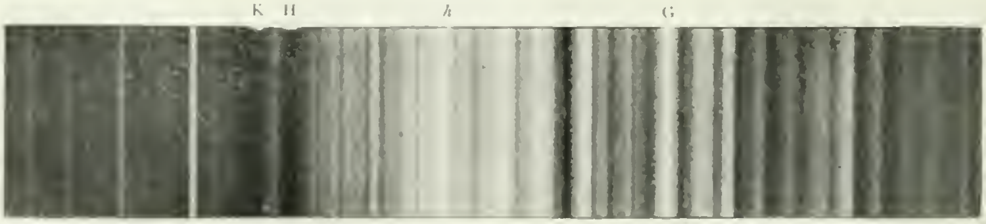


FIG. —Spectrum of o Ceti (Pickering).

variability of β Lyræ is produced by the revolution round each other of two stars like certain stars in the constellation of Orion, and that part of the light is probably cut off by some kind of eclipse; also that a certain amount of light which writes out for us these bright lines is produced at a certain part of the light curve. The photographs show that about the time of principal minimum, the dark line spectrum of β Lyræ (2) is very similar to that of Bellatrix (1), while about the time of secondary minimum the spectrum of β Lyræ (3) becomes more like that of Rigel (4), the differences at these times being mainly in the intensities of the lines. The photograph of the spectrum about the time of second maximum (5) shows that there are two spectra displaced with respect to each other. The spectrum displaced to the less refrangible side is shown to resemble that of Rigel, while that displaced to the more refrangible side closely resembles Bellatrix. I do not profess for one moment to imagine that all the conditions of variability in that star have been thoroughly explained, but we know enough to say that it is something quite different from the condition which obtains in such a star as Algol. Also, from the fact that we are dealing with stars like those in Orion, we know that we have to do with more or less condensed bodies, bodies not so condensed as the sun is, but still condensed enough to be called stars without fear of making any great mistake.

But in this class of condensed bodies we have only really touched one part of the subject, because if that condition holds for bodies which are condensed, it will not have held good for them and for others when they were less condensed than they are now. How, then, can we explain the variability of uncondensed swarms? Fig. 32 shows this.

Here we are dealing with two swarms so sparse that they may be almost considered as nebulæ; and we will suppose that round the denser and larger one a smaller one is moving in the orbit represented on the diagram. You will see that for a considerable part of the orbit the smaller swarm can perform its movement along the orbit without any chance of running up against any of the constituents of the greater swarm; but when that little swarm has got to go round what is called the periastron, *i.e.* the region nearest the centre of gravity, which is occupied by the densest portion of the primary swarm, it is impossible that it can get through without a considerable number of collisions between its own constituents and the constituents of the majority (I am not talking politics). What will happen? You will get light and heat produced, forming a variable star, which will give the greatest amount of light when those two swarms are closest together, and the least amount of light when they are furthest apart.

You can imagine also, that, instead of dealing with a highly elliptic orbit such as imagined in Fig. 32, we may have one in which the main mass is very much nearer the centre of the orbit of the smallest swarm, that orbit being much more circular than in the former case. There you will get a chance of a greater number of collisions in one part of the orbit than in another; but there will not be anything like so great a difference between the number of collisions at the two ends of the major axis of the orbit as there would have been in the first case supposed. In that way, therefore, we can explain the variability of these uncondensed swarms, and not only the variability, but a very considerable difference in the time of the cycle occupied by the changes and in the intensity of the greatest

light produced. So much is that to be anticipated, that I predicted in 1888 that when we got any indications of stars the spectra of which showed that they were really sparse swarms, such as that depicted on the diagram, at the maximum of their luminosity we should get bright lines, and in all probability bright lines of hydrogen, visible in their spectra. It so happened that shortly after this prediction was made—and when a man of science predicts he does it chiefly not for the sake of influencing others, but to point out where the path of truth really lies—I, in common with many other students in this country, received from Prof. Pickering a photograph of the spectrum of that most wonderful of all variable stars, commonly called Mira, or the marvellous star (Fig. 33). We knew before we received the photograph what its spectrum would in all probability be, but the interesting point was to see whether or not there were any bright lines in it. You see there is an obvious bright line at that part of the spectrum which represents the wave-length of one of the hydrogen lines; there is another where the wave-length of another hydrogen line is represented, and there is another very obvious bright line in another part of the spectrum. So that this photograph entirely justifies the prediction that had been made with regard to this class of stars. And so well is that now recognised that, quite independent of the meteoritic hypothesis, one of the most characteristic features of this class of stars is acknowledged to be the appearance at the top of the light curve—at the moment of the greatest giving out of light—the bright lines of hydrogen and possibly of other substances in the spectrum. Forty old variables of this class show bright lines, and twenty new variables have been detected by the appearance of bright lines, *i.e.* bright lines being seen in them suggested that they were variable, and a further inquiry into the old records showed that undoubtedly their light had varied.

J. Norman Lockyer.

(*To be continued.*)

THE INSTITUTION OF NAVAL ARCHITECTS.

THE summer meeting of the Institution of Naval Architects has been held this year in Paris, and has proved one of the most successful gatherings of the kind it has ever been our good fortune to attend. It had become known amongst members for some time past that a very strong Reception Committee had been formed, consisting of many French gentlemen, eminent both in the scientific and naval world. A large part of the week devoted to the meeting was given up to purely pleasure excursions and entertainments. Of these it is not within our province to speak, but it would be ungracious on the part of any English journal, dealing with the meeting in any way, not to say a word in recognition of the generous hospitality so lavishly displayed by all those connected with the organisation of the programme in France.

There were three sittings for the reading and discussion of papers; Lord Brassey, the President of the Institution, taking the chair on each occasion. Members assembled for the first time in the new amphitheatre of the Sorbonne, which had been kindly placed at the disposal of the Executive by the Rector of the University of Paris, M. Octave Gréard. Vice-Admiral Charles Duperré, President of the Reception Committee, welcomed the members, and Lord Brassey responded in a brief address.

The following is a list of the papers set down for reading and discussion on the programme.

"The Amplitude of Rolling on a Non-Synchronous Wave," by Émile Bertin, Directeur des Constructions Navales, and Directeur de l'École d'Application Maritime.

"On Wood and Copper Sheathing for Steel Ships," by Sir William White, Director of Naval Construction, and Assistant Controller of the Navy.

"The M.G. Metre," by Archibald Denny.

"On the utility of making the calculation of the total external volume of ships, and of drawing out the complete scale of solidity, from the triple point of view of tonnage laws, stability and load-line," by V. Daymard, Engineer in Chief of the Compagnie Générale Transatlantique.

"On Light Scantling Steamers," by B. Martell, Chief Surveyor Lloyd's Registry of Shipping.

"On Coupling Boilers of Different Systems," by Pierre Sigaudy, Engineer in Chief of the Forges et Chantiers de la Méditerranée.

"On the Cost of Warships," by Francis Elgar.

"On some necessary conditions for resisting intense firing in water tube boilers," by Augustin Normand.

"On the Niclausse Boiler," by Mark Robinson.

M. Bertin's paper, which was the first to be read, treated a highly technical subject from a strictly mathematical point of view. The author pointed out that perfect synchronism between the period of rolling and of the wave is practically a purely theoretical case. He referred to the latest calculations made which bear upon a large number of particular cases, and also to the principle of the graphic method, which has been previously described, and which is a simple extension of the method employed to determine the amplitude of rolling on a synchronous swell. The subject is one of extreme interest, but we fear we must refer those of our readers who are not acquainted with it to the published paper in the volume of the "Transactions" of the Institution. It would be impossible to give an abstract of M. Bertin's mathematics, or, indeed, to make the matter clear without the diagrams which accompanied the paper. One result, however, which may be quoted, is that M. Bertin confirms the facts brought out by Sir William White as to the great increase of efficiency of bilge-keels in large as compared with small ships. This, as our readers are aware, came somewhat as a surprise to those engaged in these matters. M. Bertin states: "We find, therefore, in bilge-keels a more powerful method of checking heavy rolling than has been foreseen. In a different condition of things, free liquid provides a more rapid means of extinguishing small rolls than could have been foreseen from any calculations founded on the known properties of liquids." M. Bertin states that the question upon which he treats is one that cannot be solved by calculation; accurate observations made at sea are the necessary complement of all the theoretical researches and experimental study made in port.

Sir William White opened the discussion on this paper. It will be remembered that at the spring meeting of the Institution the Director of Naval Construction was unable to be present, owing to a very severe illness. In spite of this, a paper which he had written on the subject now under consideration was read in his absence. His reappearance at the meetings was the occasion of a very general outburst of enthusiasm on the part of the members present, for no one is more popular, and indeed few have done more for the Institution, than Sir William White. Sir William pointed out that for mathematical purposes it was necessary to make assumptions which could be corrected by and applied to practical work. He paid a handsome compliment to the author by coupling his name with that of the late Mr. Froude.

The next paper read was Sir William White's own contribution on sheathed ships. This, as the author pointed out, was a direct contrast to the paper last read, being of a simply practical nature. As is well known, the purpose for which steel vessels of war are sheathed with wood, is in order that they may be coppered, and their bottoms may thus be preserved from fouling. It is needless to say that the wooden planking is applied as a means of preventing galvanic action between the copper and steel. In order to effect this, it is necessary that the planking should be water-tight, for sea water, in contact both with the copper and the steel skin, would set up galvanic action. It may be stated, however, in passing, that if the sea water is not in circulation, the galvanic action will not be intense or continuous, which is a fact that might be anticipated. In order to make the planking water-tight, it was originally thought necessary that a double skin should be used, and very elaborate precautions were taken in regard to fastenings. Sir William White, then Mr. White, came to the conclusion that the double planking was unnecessary, and that with proper care a single skin could be made to answer the purpose required. In this he was opposed by a large number of eminent authorities, but having the courage of his convictions, he introduced the new system into Her Majesty's Navy. The result has justified his anticipations, for after several years' experience, the hulls of ships thus sheathed have not been found to suffer.

Mr. Archibald Denny's paper described a small instrument he has invented by which the metacentric height of a vessel can be ascertained. It is intended for the use of captains of ships, so that they may ascertain the stability of their vessels under various conditions of load and trim. The instrument is simply a spirit-level pivoted at one end and adjusted at the other, by means of a micrometer screw. This combined with a diagram gives the value M.G. The method of using the instru-

ment is given in detail in the paper, and is made clear by means of diagrams.

M. Daymard's paper was of a commercial rather than a scientific interest. We all recognise that our tonnage laws are anomalous. Unfortunately they have become so interwoven with our commercial system, that it would require nothing less than a revolution to reduce them to a common-sense standard. M. Daymard commands our admiration by his courageous attempt, but as was shown during the discussion, the new laws he proposes, however unexceptionable from a scientific standpoint, would introduce undesirable features. As indicated by the title, he proposes to take the whole external volume of a ship in estimating her tonnage and load-line as well as stability. This seems reasonable, but as an illustration of the undesirability of such a law, it may be pointed out that the tendency of the ship designer working for commercial ends, as all designers of mercantile vessels must do, would be to stint engine accommodation to the manifest danger and discomfort of the engineering staff. The subject, is, however, one which we need not pursue.

Mr. Martell's contribution was one full of information and instruction to the designer of light draught vessels. Its value consisted chiefly in the thirteen plates of illustration containing details of construction of a large number of vessels designed for shallow water navigation. The descriptions which accompanied the illustrations were also of great practical information.

M. Sigaudy's paper, on coupling boilers of different systems, was a brief but instructive contribution. The introduction of the water-tube boiler, which may be said now to be complete in the case of small and exceedingly fast war vessels, appears likely to make headway even in craft not of this special description. The water-tube boiler is, however, something new, and the average engineer, engaged in practical work, always shies at novelties. That is but natural, and it is the result of common sense that caution should be observed when risks have to be run. By the system advocated by M. Sigaudy, the risk is reduced to the smallest dimensions. In a tug-boat built by his Company, an ordinary return tube marine boiler is combined with two water-tube boilers. The engineers of the vessel have therefore a steam generator at their disposal, which they thoroughly understand, and which is sufficient to supply steam to drive the boat at moderate speed. Should the water-tube boilers fail, therefore, they would not be left helpless. One advantage of the water-tube boilers is that steam can be raised very quickly, and this is a very desirable feature in a tug which has at times to be used in cases of emergency. The time occupied upon two trials in raising steam was respectively 22 and 23 minutes. The consumption of fuel was 1·78 lbs. per horse-power per hour, which, it need hardly be said, is a very satisfactory result. No trouble has been found, since the tug has been used, to arise from the combination of the two systems of boilers. In the discussion which followed the reading of this paper, Mr. Yarrow stated that a similar system has been adopted by the Dutch Government in some cruisers they are having built. These vessels are naturally of much larger size than the tug-boat described by M. Sigaudy, and their trials will be looked forward to with considerable interest by the naval world.

Dr. Elgar's paper, on the cost of war ships, constituted a new departure in the annals of the Institution. It has generally been considered, if not expressly stated, that financial questions are tabooed by the Institution. In the case of Government vessels, doubtless more latitude should be allowed, but in any event it is a difficult thing to exclude money considerations from discussions on subjects which have a commercial basis. After all, ships are built to earn money, and even the designer of war-ships has to keep the question of cost incessantly before him. It would be useless, for instance, suggesting a new form of marine engine, however perfect from a scientific point of view or economical in its working, if its first cost were to be prohibitive. In the discussion which followed the reading of the paper, views similar to these were expressed by prominent members of the Institution, and it is probable that more latitude will be given for the future in this respect. For our own part, it will be impossible to abstract, in anything like reasonable space, the vast quantities of figures given by the author of the paper. His analysis of the subject was very complete, and it may be stated, briefly, led to the happy conclusion that dockyard-built war vessels are costing less than they did of old, relatively to the work put into them. It may be stated, although Dr. Elgar failed to point the fact out in his paper, that this happy state of affairs is largely due to the good work he himself did when Director of Dockyards.

The last two papers of the meeting were on the subject of the hour, water-tube boilers. M. Normand, the well-known builder of torpedo boats at Havre, and one of the most scientific and best informed marine engineers of the day, gave a very valuable analysis of the points which should be observed in designing a water-tube boiler. Naturally, circulation occupied his chief attention, and it may be said briefly that if sufficient activity of circulation of water and steam in the boiler can be maintained, that boiler is likely to be an efficient steam generator. How to obtain such circulation is a complex and disputed question, and here we find our own great authority on the subject, Mr. Thornycroft, at issue with the author of the paper. Mr. Thornycroft, as is well known, is a strong advocate of above water discharge into the steam drum. M. Normand, on the other hand, upholds "drowned" tubes. The subject is a large one, far too large for discussion in a report of this nature. To us it appears that M. Normand is not warranted in all the assumptions upon which he bases his conclusion, and further it may be said that Mr. Thornycroft has experimental data on his side in maintaining that the circulation of water is more active with above water discharge than with drowned tubes. Whether with the latter the circulation is sufficiently active for all practical purposes is of course another matter, the bearing of which it remains for practical experience to prove. For, like the problem M. Bertin attacked in his paper, it is not solvable by theoretical analysis.

Mr. Mark Robinson, in his paper, described a very promising form of water-tube boiler which has been introduced in France by M. Niclausse, the inventor. Without illustrations it would be utterly impossible to make the design clear; but it may be said that the principle followed is that of the Field tube, in which circulation is promoted by means of a pipe inside and coaxial with the heating tube. Curiously enough, however, the tubes in the Niclausse boiler are horizontal, or approximately horizontal, so that the circulation is maintained in the "header" which is divided by a diaphragm, the difference between the specific gravities of the water, or water and steam, contained on each side of the diaphragm causing the movement of the water. This boiler appears to be one of great promise amongst water-tube boilers in situations where the highest evaporative efficiency is not required. It is, however, in these positions that the ordinary return tube boiler is strongest. Whether it will be supplanted by a water-tube boiler remains to be seen; but should such be the case, the Niclausse boiler has the appearance of being a formidable competitor.

No account of the Paris meeting would be complete without reference being made to the beautiful series of photographs shown by M. Bertin in illustration of the movements of ships in a sea-wave. These photographs were taken by the method devised by M. Marey, to which reference has already been made in these columns. A dozen or more different views are given of a ship during its passage through a wave, and the whole movement can thus be fixed and analysed. The value of such data to the naval architect is, of course, immense. In connection with these photographs, which were shown on the screen, there were also exhibited some very beautiful projections of photographs in colours. These were shown by M. Charles Comte, one of M. Marey's assistants. The subject is one which has been attracting attention of late, and has been referred to elsewhere in these columns.

METEOROLOGICAL PROBLEMS FOR PHYSICAL LABORATORIES.

IN response to several requests from both teachers and students for suggestions as to problems that can be taken up in physical laboratories, Prof. Cleveland Abbe gives the following list of subjects, in the *American Meteorological Journal* for May. The initialled subjects are due to Prof. C. F. Marvin.

SUBJECTS FOR EXPERIMENTAL INVESTIGATION.

(1) The internal sensitiveness of thermometers, or the length of time required to bring the top of the thermometer column to the proper reading when the external surface of bulb and stem is kept at a constant temperature below, or above, some initial temperature.

(2) The influence of the wind on the pressure within a room, or other closed space, containing a barometer.

(3) The influence of the condition of any surface (as to

chemical nature, cleanness, and dust) upon the deposition of dew and the determination of the dew-point.

(4) The behaviour of the wet-bulb thermometer, when covered with water, in an atmosphere of water vapour and of ice vapour.

(5) The influence of radiant heat on wet bulbs covered with ice or water.

(6) The increase of the reading of the wet-bulb thermometer due to any compression that may result from the formation of the ice film on the muslin covering; its dependence on the muslin rather than on the ice.

(7) The determination of the tension of water vapour and ice vapour at and below freezing.

(8) The rate of diffusion of ice vapour as distinguished from aqueous vapour, and also the rates of evaporation from ice and water at the same temperature.

(9) The condensation of vapour in a region free from solid nuclei, and after the temperature has been reduced to, or below, the point of saturation so that the vapour is in a state of unstable equilibrium.

(10) The change that can be produced in the pressure and temperature of a confined volume of *dust free* "dry saturated" steam or other vapour by the introduction of dust particles having various chemical and physical properties. This is the secret of the action of the "cloud engine" of Montgomery J. Storms.

(11) Invention of improved and practical methods of obtaining the moisture contents of the air—especially at low temperatures. C. F. M.

(12) Invention of recording thermometers, barometers, and hygrometers adapted by their accuracy, their extreme lightness, and the quickness with which they respond to atmospheric changes, to be carried up by balloons and by kites in investigations into the condition of the higher atmosphere.—C. F. M.

(13) The development and perfecting of the art of constructing and flying kites with a view of rendering this practically applicable in investigations of the condition of the atmosphere at moderate elevations. C. F. M.

(14) Invention of improved and practical devices for the registration of sunshine and cloudiness, both day and night.—C. F. M.

(15) Invention of devices recording exactly the beginnings and endings, amounts and rates, of precipitation, &c.—C. F. M.

(16) Explanation of the formation of ice-needles in gravelly soil, and determination of the amount of heat and moisture retained at the earth's surface by this formation.

(17) Explanation of the origin of the hollow tubes in the ice-needles and the similar hollow tubes in snow crystals and the analogous holes in hailstones.

(18) The connection between atmospheric conditions and the formation of snow crystals of different shapes and sizes.

(19) The radiating and conducting powers of layers of snow freshly fallen or old and granulated.

(20) The radiation and absorption of heat by dustless, dry air, and also by ordinary atmospheric air containing dust and vapour or ice particles.

(21) Investigation of the formula for computing the velocity and the pressure of the wind from various forms of anemometers, especially the whirling, the pressure, and the suction anemometers.

(22) Invention of the most convenient and cheapest form of nephoscope for determining either direction or velocity, or both these elements of the motion of the clouds.

(23) Investigation of the correction to be made to the record of the ordinary cylindrical rain and snow gauge for the effect of the wind in drifting the rain, and especially the snow.

(24) Study of the temperature of the soil at different depths from the surface-layer down to three feet and under different conditions, as to moisture-content, sunshine, and wind.

(25) Invention of better methods of determining at any moment the temperature and moisture at any depth in the soil.

(26) Determination of the quantity of water evaporated from natural surfaces, especially ocean water, ice or snow, fresh water, and forests or cultivated fields, and its relation to humidity, temperature, and wind.

(27) Improvements in the actinometer and a series of determinations of the amounts of heat received at any point, both from the sun directly and from the clouds and the atmosphere by reflection or radiation.

(28) Observations of the polarisation and the intensity of blue sky light and comparison with optical theories.

(29) Instrumental methods for recording some of the various chemical effects directly produced by solar radiation, and which are of special importance in the growth of plants, the decomposition of the soil, and the purification of water.

(30) A series of determinations or, still better, a continuous record of the simultaneous differences of electric potential between the earth's surface, and several points in the free atmosphere, one hundred feet apart, vertically, meridionally, and prime-vertically.

(31) A similar series for several points beneath the earth's surface as to their electro-magnetic condition, and a correlation of the distribution of electric conditions with the electric currents in the air and the earth.

(32) A study of the scintillation of the stars and its relation to atmospheric conditions.

(33) A study of the apparent acoustic opacity of the atmosphere at certain places and times.

(34) An explanation of the sounds attending large aerolites, and an explanation as to what may be learned therefrom regarding the upper atmosphere and in regard to the improvement of fog signals.

(35) A study of the formation of halos, parhelia, and corona, by the action of snow crystals and water-drops on sunlight.

(36) Investigation of the first step in the process of convection, as it occurs in the free atmosphere by which small currents of warm air, rising as slender rolls and whirls, mix with the cooler air, and are broken up within a few feet of the earth's surface; a determination of the limit at which such convection becomes inappreciable.

(37) A study of the larger convection currents, their relation to the horizontal motion, the extent to which they retard and accelerate the motions or increase and decrease the pressures in the upper and lower strata.

THE SENSES OF INSECTS.[1]

OF the five ordinary senses recognised in ourselves and most higher animals, insects have, beyond all doubt, the sense of sight, and there can be as little question that they possess the senses of touch, taste, smell, and hearing. Yet, save perhaps that of touch, none of these senses, as possessed by insects, can be strictly compared with our own, while there is the best of evidence that insects possess other senses which we do not, and that they have sense organs with which we have none to compare. He who tries to comprehend the mechanism of our own senses—the manner in which the subtler sensations are conveyed to the brain—will realise how little we know thereof after all that has been written. It is not to be wondered at, therefore, that authors should differ as to the nature of many of the sense organs of insects, or that there should be little or no absolute knowledge of the manner in which the senses act upon them. The solution of psychical problems may never, indeed, be obtained, so infinitely minute are the ultimate atoms of matter; and those who have given most attention to the subject must echo the sentiment of Lubbock, that the principal impression which the more recent works on the intelligence and senses of animals leave on the mind is that we know very little, indeed, on the subject. We can but empirically observe and experiment and draw conclusions from well attested results.

Sight. Taking first the sense of sight, much has been written as to the picture which the compound eye of insects produces upon the brain or upon the nerve centres. Most insects which undergo complete metamorphoses possess in their adolescent states simple eyes or ocelli, and sometimes groups of them of varying size and in varying situations. It is difficult, if not impossible, to demonstrate experimentally their efficiency as organs of sight; the probabilities are that they give but the faintest impressions, but otherwise act as do our own. The fact that they are possessed only by larvæ which are exposed more or less fully to the light, while those larvæ which are endophytous, or otherwise hidden from light, generally lack them, is in itself proof that they perform the ordinary functions of sight, however low in degree. In the imago state the great majority of insects have their simple eyes in addition to the compound eyes. In many cases, however, the former are more or less covered with vestiture, which is another evidence that their function is of a low

[1] From an address on "Social Insects," delivered by Prof. C. V. Riley, as President of the Biological Society of Washington. (Reprinted (slightly condensed) from *Insect Life*, vol. vii. No. 1.)

order, and lends weight to the view that they are useful chiefly for near vision and in dark places. The compound eyes are prominent and adjustable in proportion as they are of service to the species, as

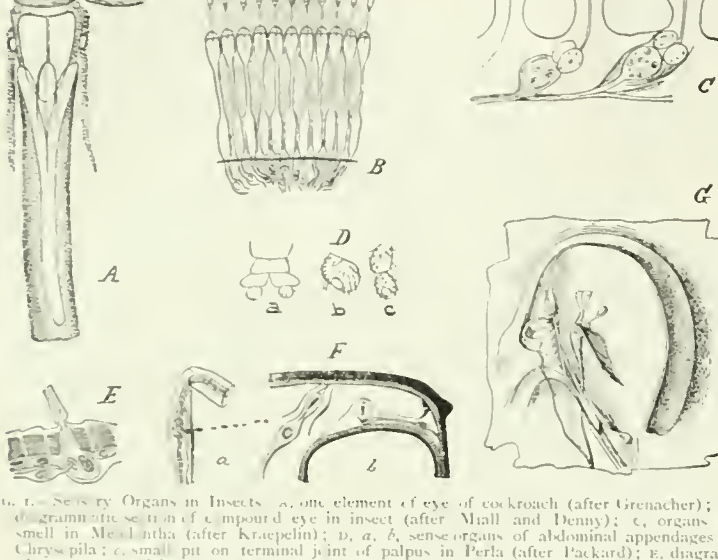


Fig. 1.—Sensory Organs in Insects: A, one element of eye of cockroach (after Grenacher); B, diagrammatic section of compound eye in insect (after Miall and Denny); C, organs of smell in Melolontha (after Kraepelin); D, *a*, *b*, sense-organs of abdominal appendages of Chrysopila; *c*, small pit on terminal joint of palpus in Perla (after Packard); E, diagram of sensory ear of insect (after Miall and Denny); F, auditory apparatus of Meconema; *a*, fore tibia of this locust; *b*, diagrammatic section through same (after Graber); G, auditory apparatus of Caloptenus, seen from inner side, showing tympanum, auditory nerve, terminal ganglion, stigma, and opening and closing muscle of same, as well as muscle of tympanum membrane (after Graber). All very greatly enlarged.

witness those of the common house-fly and of the Libellulidæ or dragon-flies. It is obvious from the structure of these compound eyes that impressions through them must be very different from those received through our own, and, in point of fact, the experimental researches of Hickson, Plateau, Tocke and Lemmermann, Pankrath, Exner, and Viallanes have practically established the fact that while insects are shortsighted and perceive stationary objects imperfectly, yet their compound eyes are better fitted than the vertebrate eye for apprehending objects set in relief or in motion, and are likewise keenly sensitive to colour.

So far as experiments have gone, they show that insects have a keen colour sense, though here again their sensations of colour are different from those produced upon us. Thus, as Lubbock has shown, ants are very sensitive to the ultra-violet rays of the spectrum, which we cannot perceive, though he was led to conclude that to the ant the general aspect of nature is presented in an aspect very different from that in which it appears to us. In reference to bees, the experiments of the same author prove clearly that they have this sense of colour highly developed, as indeed might be expected when we consider the part they have played in the development of flowers. While these experiments seem to show that blue is the bee's favourite colour, this does not accord with Albert Müller's experience in nature, nor with the general experience of apiarians, who, if asked, would very generally agree that bees show a preference for white flowers.

Touch.—The sense of touch is supposed to reside chiefly in the antennæ or feelers, though it requires but the simplest observation to show that with soft-bodied insects the sense resides in any portion of the body, very much as it does in other animals.

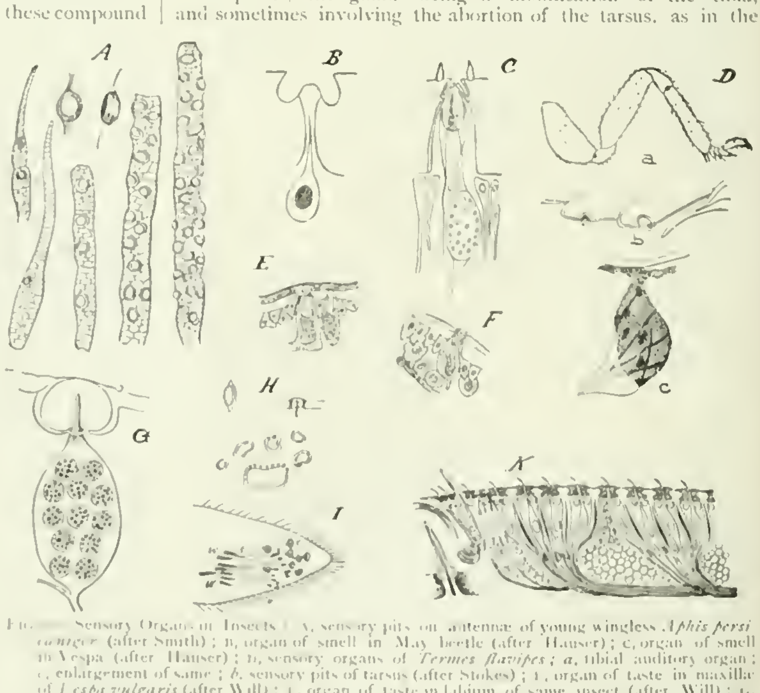


Fig. 2.—Sensory Organs in Insects: A, sensory pits on antennæ of young wingless *Aphis persicæniger* (after Smith); B, organ of smell in May beetle (after Hauser); C, organ of smell in Vespa (after Hauser); D, sensory organs of *Termes flavipes*; *a*, tibial auditory organ; *c*, enlargement of same; *b*, sensory pits of tarsus (after Stokes); E, organ of taste in maxillæ of *Vespa vulgaris* (after Will); F, organ of taste in labium of same insect (after Will); G, organ of smell in Caloptenus (after Hauser); H, sensory pilose depressions on tibia of Termes (after Stokes); I, terminal portion of antennæ of *Myrmica ruginodis*, *c*, cork-shaped organs; *s*, outer sac; *t*, tube; *w*, posterior chamber (after Lubbock); K, longitudinal section through portion of flagellum of antennæ of worker bee, showing sensory hairs and supposed olfactory organs (after Cheshire). All very greatly enlarged.

In short, this is the one sense which, in its manifestations, may be conceded to resemble our own. Yet it is evidently more specialised in the maxillary and labial palpi and the tongue than in the antennæ in most insects.

Taste.—Very little can be positively proved as to the sense of taste in insects. Its existence may be confidently predicated from the acute discrimination which most monophagous species exercise in the choice of their food, and its location may be assumed to be the mouth or some of the special trophial organs which have no counterpart among vertebrates. Indeed, certain pits in the epipharynx of many mandibulate insects and in the ligula and the maxillæ of bees and wasps are conceded by the authorities to be gustatory.

Smell.—That insects possess the power of smell is a matter of common observation, and has been experimentally proved. The many experiments of Lubbock upon ants left no doubt in his mind that the sense of smell is highly developed in them. Indeed, it is the acuteness of the sense of smell which attracts many insects so unerringly to given objects, and which has led many persons to believe them sharp-sighted. Moreover, the innumerable glands and special organs for secreting odours furnish the strongest indirect proof of the same fact. Some of these, of which the osmaterium in Papilionid larvæ and the eversible glands in Parorgyia are conspicuous examples, are intended for protection against inimical insects or other animals; while others, possessed by one only of the sexes, are obviously intended to please or attract. A notable development of this kind is seen in the large gland on the hind legs of the males of some species of Hepialus, the gland being a modification of the tibia, and sometimes involving the abortion of the tarsus, as in the European *H. hectus* (L.) and our own *H. behrensi* (Stretch.) The possession of odoriferous glands, in other words, implies the pos-

session of olfactory organs. Yet there is among insects no one specialised olfactory organ as among vertebrates; for while there is conclusive proof that this sense rests in the antennæ with many insects, especially among Lepidoptera, there is good evidence that in some Hymenoptera it is localised in an ampulla at the base of the tongue, while Graber gives reasons for believing that in certain Orthoptera (Blattidæ) it is located in the anal cerci and the palpi.

Hearing.—In regard to the sense of hearing, the most casual experimentation will show (and general experience confirms it) that most insects, while keenly alive to the slightest movements or vibrations, are for the most part deaf to the sounds which affect us. That they have a sense of sound is equally certain, but its range is very different from ours. A sensitive flame, arranged for Lubbock by the late Prof. Tyndall, gave no response from ants, and a sensitive microphone, arranged for him by Prof. Bell, gave record of no other sound than the patter of feet in walking. But the most sensitive tests we can experimentally apply may be, and doubtless are, too gross to adjust themselves to the finer sensibilities of such minute, active, and nervous creatures. There can be no question that insects not only produce sounds, but receive the impression of sounds entirely beyond our own range of perception, or, as Lubbock puts it, that "we can no more form an idea of than we should have been able to conceive red or green if the human race had been blind. The human ear is sensitive to vibrations reaching at the outside to 38,000 in a second. The sensation of red is produced when 470 millions of millions of vibrations enter the eye in a similar time; but between these two numbers vibrations produce on us only the sensation of heat. We have no especial organ of sense adapted to them." It is quite certain that ants do make sounds, and the sound-producing organs on some of the abdominal joints have been carefully described. The fact that so many insects have the power of producing sounds that are even audible to us, is the best evidence that they possess auditory organs. These are, however, never vocal, but are situated upon various parts of the body, or upon different members thereof.

Special Sense and Sense Organs.—While from what has preceded it is somewhat difficult to compare the more obvious senses possessed by insects with our own, except perhaps in the sense of touch, it is, I repeat, just as obvious to the careful student of insect life that they possess special senses which it is difficult for us to comprehend. The sense of direction, for instance, is very marked in the social Hymenoptera which we have been considering, and in this respect insects remind us of many of the lower vertebrates which have this sense much more strongly developed than we have. Indeed, they manifest more especially what has been referred to in man as a sixth sense, viz. a certain intuition which is essentially psychical, and which undoubtedly serves and acts to the advantage of the species as fully, perhaps, as any of the other senses. Lubbock demonstrated that an ant will recognise one of its own colony from among the individuals of another colony of the same species; and when we consider that the members of a colony number at times, not thousands, but hundreds of thousands, this remarkable power will be fully appreciated.

The neuter Termites are blind, and can have no sense of light in their internal or subterranean burrowings; yet they will undermine buildings, and pulverise various parts of elaborate furniture without once gnawing through to the surface; and those species which use clay, will fill up their burrowings to strengthen the supports of structures which might otherwise fall and injure the insects or betray their work. The bat in a lighted room, though blinded as to sight, will fly in all directions with such swiftness and infallible certainty of avoiding concussion or contact, that its feeling at a distance is practically incomprehensible to us.

Telepathy.—But however difficult it may be to define this intuitive sense which, while apparently combining some of the other senses, has many attributes peculiar to itself, and however difficult it may be for us to analyse the remarkable sense of direction, there can be no doubt that many insects possess the power of communicating at a distance, of which we can form some conception by what is known as telepathy in man. This power would seem to depend neither upon scent nor upon hearing in the ordinary understanding of these senses, but rather on certain subtle vibrations as difficult for us to comprehend as is the exact nature of electricity. The fact that men can telegraphically transmit sound almost instantaneously around the globe, and that his very speech may be telephonically transmitted, as quickly as uttered, for thousands of miles, may suggest something of this subtle power, even though it furnish no explanation thereof.

The power of sembling amongst certain moths, for instance, especially those of the family Bombycidæ, is well known to entomologists, and many remarkable instances are recorded. I am tempted to put on record for the first time an individual experience which very well illustrates this power, as on a number of occasions when I have narrated it most persons not familiar with the general facts have deemed it remarkable. In 1863 I obtained from the then Commissioner of Agriculture, Colonel Capron, eggs of *Samia cynthia*, the Ailanthus silkworm of Japan, which had been recently introduced by him. I was living in Chicago at the time, and in my garden there grew two Ailanthus trees, which were the cause of my sending for the aforesaid eggs. I had every reason to believe that there were no other eggs of this species received in any part of the country within hundreds of miles around. It seemed a good opportunity to test the power of this sembling, and after rearing a number of larvæ I carefully watched for the appearance of the first moths from the cocoons. I kept the first moths separate, and confining a virgin female in an improvised wicker cage out of doors on one of the Ailanthus trees. On the same evening I took a male to another part of the city, and let him loose, having previously tied a silk thread around the base of the abdomen to insure identification. The distance between the captive female and the released male was at least a mile and a half, and yet the next morning these two individuals were together.

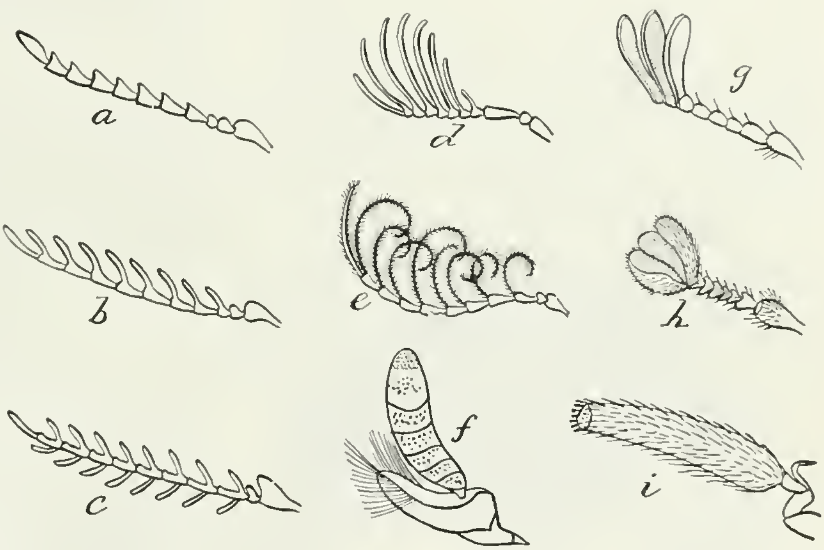


FIG. 3.—Some Antennæ of Coleoptera: *a*, Ludius; *b*, Corymbites; *c*, Prinocyphon; *d*, Acneus; *e*, Dendroides; *f*, Dineutes; *g*, Lachnosterna; *h*, Bolbocerus; *i*, Adranes (after Le Conte and Horn). All greatly enlarged.

Now, in the moths of this family the male antennæ are elaborately pectinate, the pectinations broad and each branch minutely hairy (see Fig. 5, *a*.) These feelers vibrate incessantly, while in the female, in which the feelers are less complex, there is a similar movement connected with an intense vibration of the whole body and of the wings. There is, therefore, every reason to believe that the sense is in some way a vibratory sense, as, indeed, at base is true of all senses, and no one can study the wonderfully diversified structure of the antennæ in insects,

especially in males, as very well exemplified in some of the commoner gnats (see Fig. 5, *d*, *e*), without feeling that they have been developed in obedience to, and as a result of, some such subtle and intuitive power as this of telepathy. Every minute ramification of the wonderfully delicate feelers of the male mosquito, in all probability, pulsates in response to the piping sounds which the female is known to produce, and doubtless through considerable distance.

There is every justification for believing that all the subtle cosmic forces involved in the generation and development of the

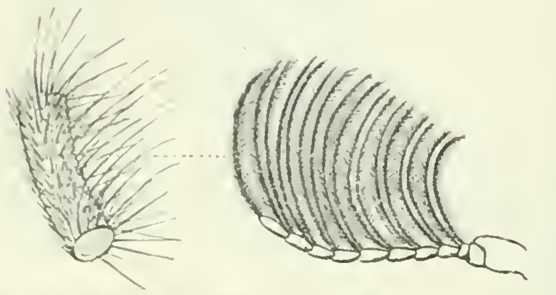


Fig. 4.—Antenna of male Phengodes with portion of ray.—Greatly enlarged (original).

highest are equally involved in the production and building up of the lowest of organisms, and that the complexing and compounding and specialisation of parts have gone on in every possible and conceivable direction, according to the species. The highly developed and delicate antennæ in the male Chironomus, for instance, may be likened to an external brain, its ramifying fibres corresponding to the highly complicated pro-

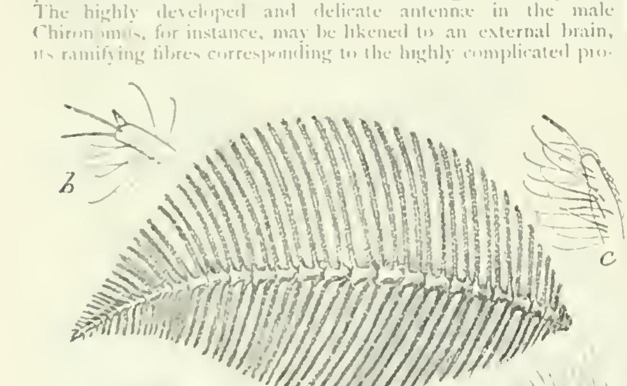


Fig. 5.—[illegible] Antennæ of [illegible] *a*, Telephorus [illegible] male [illegible]; *b*, [illegible] still more enlarged; *d*, Chironomus × 6; *e*, [illegible] still more enlarged (original).

cesses that ramify from the nerve cells in the internal brains of higher animals, and responding in a somewhat similar way to external impressions. While having no sort of sympathy with the foolish notions that the spiritualists proclaim, to edify or terrify the gullible and unscientific, I am just as much out of sympathy with that class of materialists who refuse to recognise that there may be and are subtle psychical phenomena beyond the reach of present experimental methods. The one class too readily assumes supernatural power to explain abnormal phenomena; the other denies the abnormal, because it, likewise, is past our limited understanding.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The Harkness Scholarship in Geology has been awarded to Arthur William Rogers, of Christ's College.

Mr. J. S. Gardiner, of Caius College, has been chosen to occupy the University's table at the Naples Zoological Station for six months from October 1.

The Newall Observer reports that the fine spectroscope designed for use with the Newall Telescope is now ready, and that the preliminary trials of it have been satisfactory. The mounting has been made by the Cambridge Scientific Instrument Company, and the optical parts by Mr. Brashear, of Alleghany.

Mr. F. Darwin, Mr. W. G. P. Ellis, Prof. Liveing, Mr. T. B. Wood, Prof. M. Foster, Mr. A. Eichholz, Mr. A. E. Shipley, Mr. C. Warburton, Prof. Hughes, Mr. P. Lake, Mr. O. P. Fisher, Mr. J. Owen, Mr. R. Menzies, and Mr. C. B. Fisher, have been appointed Examiners in the Science and Art of Agriculture for the University Diploma. The examination will be held in July.

Sir David L. Salomons, Bart., has founded, in connection with Caius College, a Scholarship in Engineering. The first award will be made in October. The value of the Scholarship is £40 a year for three years. The Salomons Scholar must become a candidate for the Mechanical Sciences Tripos. Applications for further information should be made to the Tutors of Caius College.

The Conference on Technical Education held at the Society of Arts last Thursday, resulted in the adoption of the following resolution:—"That in the opinion of this meeting it is desirable that provision should be made for examination and inspection in the subjects of instruction undertaken by technical instruction committees but not at present included in the schemes of the Science and Art Department, the City and Guilds of London Institute, and the Society of Arts, and that with the object of giving effect to the same this conference recommends that a representative committee be appointed to draw up a report and prepare recommendations on the whole subject."

SCIENTIFIC SERIALS.

American Journal of Science, June.—The preparation of perchloric acid and its application to the determination of potassium, by D. Albert Kreider. The difficulty attending the removal of the potassium in the ordinary preparation of this acid from potassium chlorate may be overcome by using the sodium salt instead. The insolubility of chloride of sodium in strong hydrochloric acid, with the aid of the acid-proof Gooch crucible, affords a means for the liberation of the perchloric acid and the removal of the greater part of the sodium in one operation. Sodium chlorate is heated until it gives off oxygen. When all the possible oxygen has been given off, and only the chloride and the perchlorate remain, the residue is treated with strong hydrochloric acid and filtered. The perchloric acid is thus liberated, and the sodium precipitated as chloride. The liquid is decanted, and undergoes the same operation again. The solution, containing hydrochloric and perchloric acids and a small amount of sodium chloride, is evaporated till the former acid is driven off and the heavy white fumes of the perchloric acid appear. It is then ready for potassium determinations, with which the small residue of sodium does not interfere. The filtering is done by means of a Gooch crucible, and the operation requires less time and attention than the old process, and is much less dangerous.—Mode of growth and development of the graptolitic genus *Diplograptus*, by R. Ruedemann. By the possession of a pneumatocyst and the arrangement of the reproductive organs at the bases of the stipes, the colonial stocks of *Diplograptus* have a general similarity to those of certain *Siphonophoræ*, while the chitinous structure of the hydrothecæ and gonangia can only be referred to the Sertularians. It thus becomes evident that the genus *Diplograptus*, like so many palæozoic fossils, has the combined properties of different groups, thus giving valuable hints in regard to the common ancestors of those groups.—On the elevation along the Rocky Mountain range in British America since the close of the Cretaceous period, by Dr. G. M. Dawson. In the mountains, the cretaceous rocks have been involved in all the flexure, faulting, and overthrust suffered by the Palæozoic; and both in the mountains and foothills these rocks are found at all angles up to vertical, and even overturned.

It is thus difficult to know the amount of elevation of these rocks, but about latitude 50° the base of the cretaceous must in several places have considerably exceeded 10,000 feet in altitude.

Symons's Monthly Meteorological Magazine, June. The principal article deals with rainfall in China, with remarks by the editor, based on observations made from 1886-92, and published in various places by Dr. Doberck, of Hong Kong. The mean annual rainfall is small in the north, and increases greatly towards the south. In the Gulf of Pe-chi-li the fall is 20 inches, but reaches double that amount in the Delta of the Yang-Tse-Kiang, 58 inches at Hankow, and 68 inches at Ningpo. In Formosa it ranges from 60 to 90 inches, but at Keelung, in the north-east, it reaches 148 inches. The seasonal rainfall is shown in tables divided into six districts. Notwithstanding the proximity of most of the stations to the sea, the distribution is, generally speaking, of that type which prevails over the greater part of Asia.

SOCIETIES AND ACADEMIES.

London.

Royal Society, January 24.—"Micro-Metallography of Iron." Part I. By Thomas Andrews, F.R.S.

In the course of a research with high microscopical powers (including 300, 500, 800, 1200, and upwards to 2000 diameters) on the micro-crystalline structure of large masses of wrought iron, the author observed the following novel metallurgical facts:—

When large masses, several tons in weight, of practically pure wrought iron were allowed to slowly cool from a white heat, a secondary or subcrystallisation of the metallic iron occurred. The normal primary crystals of the iron, or those which have hitherto been regarded as constituting the ultimate structure of the metal, were found to enclose a subcrystalline formation consisting of very minute, and much smaller, crystals of pure iron also belonging to the regular order of crystallisation. These crystals sometimes manifested the hexagonal form, the predominant angle being about 120°, and often they assumed the form of simple cubes. The secondary crystals were contained within the area of the larger primary crystals.

Typical illustrations of this duplex crystallisation found in two large iron forgings are given in Figs. 1 and 2, and the relative dimensions of a number of individual crystals are given in the paper.

The results of twenty measurements of the primary crystals and twenty measurements of the secondary crystals taken on each forging are given on these tables.

The markings of the intercrystalline spaces or junctions of the secondary crystals were very clearly defined, but they were exceedingly minute. The general form, contour, and relative size of the primary and secondary crystals, as seen in section, will be noticed on reference to the accurate tracings, Figs. 1 and 2. The linear dimensions of the primary crystals would average about 0·01 inch, the linear dimensions of the secondary crystals averaging about 0·001 inch.

Judging roughly from the indications of the average micro-measurements, there would appear to be approximately 1,000,000,000 of the secondary crystals in a cubic inch of the metallic iron.

In the case of both the primary and secondary crystals the predominant well-defined angles of the facets of the crystals hovered more or less about the angle of 120°. The majority of the angle readings, made with the goniometer attached to the microscope, indicating generally a hexagonal structure on form of crystallisation. There were, however, also perfect cubical crystals observed.

The observations were made with a Ross first-class microscope. The micro-measurements afford an indication of the comparative size of the primary and secondary crystals. These measurements were carefully taken by a Jackson micrometer, and in some cases by a Ramsden screw micrometer, both accurately calibrated with a standard stage micrometer. The wrought iron forgings on which the observations were made were constituted of practically pure hammered wrought iron, the dimensions of the mass being about 10 feet long and about 12 inches square. The great length of time required for such large masses of iron to cool from a white heat appeared to facilitate the production of the crystals of the secondary formation.

The *rationale* of this duplex crystallisation has apparently been as follows:—The mass of metallic iron on cooling having reached the crystallising point at about 740° C., the periphery or skeletons of the larger or primary crystals were then formed. As the period of cooling was, however, very slow, the semi-fluid or viscous metal in the interior of these primary crystals was, on finally consolidating, apparently further broken up or subdivided

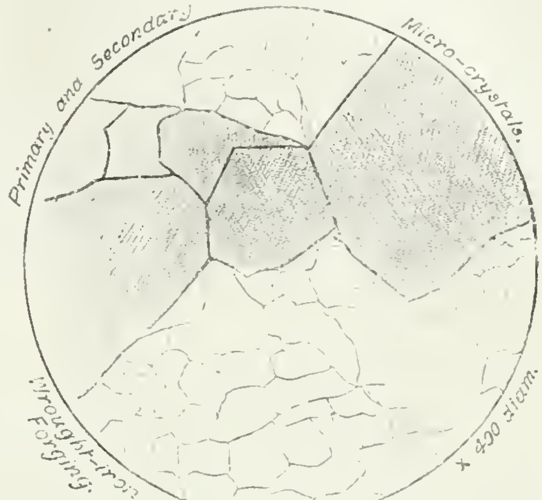


Fig. 1.

into a considerable number of smaller crystals, enclosed within the boundary or periphery of the primary crystals.

In the course of further experiments on the cooling of large masses of wrought iron, the author has also found, by the use of high power objectives, that the secondary crystals sometimes enclosed a still more minute form of crystals of pure iron, of the cubical form, which may hence be regarded as constituting a tertiary system of crystallisation in pure metallic iron. These

Fig. 2.

experiments therefore indicate that large masses of heated wrought iron, on cooling from above the temperature of the crystallisation of metallic iron, viz. 740° C., are capable of crystallising in three distinct modifications which may tentatively be called the primary, secondary, and tertiary system of crystallisation in iron, these various crystalline modifications being all, however, connected with the regular system of crystallisation.

The crystals of this secondary formation are not often distinctly discernible in smaller masses of metallic iron, such as rolled rods, plates, or sheets, as these in the course of manufacture rapidly cool, and are frequently manipulated during the finishing processes at temperatures below the crystallising point of wrought iron (740° C.).

The microscopical examinations were made on carefully prepared and polished samples, etched in nitric acid (1 part HNO_3, sp. gr. 1·20, and 49 parts water), and by the use of high microscopical powers ($\frac{1}{8}$-inch to $\frac{1}{16}$-inch, and other ojectives). The drawings were accurately made with the camera lucida. In each observation the etching was prolonged, under constant observation with lenses, a suitable time to develop the accurate structure of the metal.

June 13.—"On the New Gas obtained from Uraninite." Fourth Note. By J. Norman Lockyer, C.B., F.R.S.

Continued experiments on the gases obtained by heating the minerals broggerite and euxenite *in vacuo* have revealed the presence in the spectrum of an important line in the infra-red. By comparisons with the solar spectrum in the first order grating spectrum, the wave-length of the line has been approximately

(2) Contrariwise, when we are dealing with a known compound gas: at the lowest tension we may get the complete spectrum of the compound without any trace of its constituents, and we may then, by increasing the tension, gradually bring in the lines of the constituents until, when complete dissociation is finally reached, the spectrum of the compound itself disappears.

Working on these lines, the spectrum of the spark at atmospheric pressure, passing through the gas, or gases, distilled from bröggerite, has been studied with reference to the special lines C (hydrogen), D_3, 667, and 447.

The first result is that all the lines do not vary equally, as they should do if we were dealing with a simple gas.

The second result is that at the lowest tension 667 is relatively more brilliant than the other lines; on increasing the tension, C and D_3 considerably increase their brilliancy, 667 relatively and absolutely becoming more feeble; while 447, seen easily as a narrow line at low tension, is almost broadened out into invisibility as the tension is increased in some of the tubes, or is greatly brightened as well as broadened in others (Fig. 1).

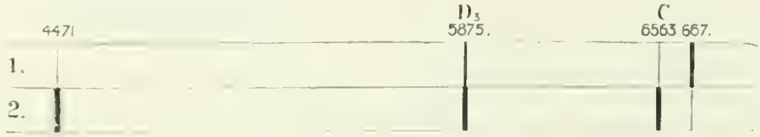


Fig. 1.—Diagram showing changes in intensities of lines brought about by varying the tension of the spark. (1) Without air-break. (2) With air-break.

determined as 7065. There can be little doubt, from the observations which have been made, that this new line is coincident with a chromospheric line which occurs in Young's list, having a frequency of 100, and of which the wave-length on Rowland's scale is stated to be 7065·5.

It follows therefore that, besides the hydrogen lines, all three chromospheric lines in Young's list which have a frequency of 100 have now been recorded in the spectra of the new gas or gases obtained from minerals by the distillation method.

These are as follows:—

7065·5
5875·98
4471·8

The wave-lengths of the lines are in Rowland's scale, as given in Scheiner's "Astronomical Spectroscopy."[1] In a partial

The above observations were made with a battery of five Grove cells; the reduction of cells from 5 to 2 made no difference in the phenomena except in reducing their brilliancy.

Reasoning from the above observations, it seems evident that the effect of the higher tension is to break up a compound, or compounds, of which C, D_3, and 447 represent constituent elements; while, at the same time, it would appear that 667 represents a line of some compound which is simultaneously dissociated.

The unequal behaviour of the lines has been further noted in another experiment, in which the products of distillation of bröggerite were observed in a vacuum tube and photographed at various stages. After the first heating, D_3 and 4471 were seen bright, before any lines other than those of carbon and hydrogen made their appearance. With continued heating, 667, 5016, and 492 also appeared, although there was no notable increase of

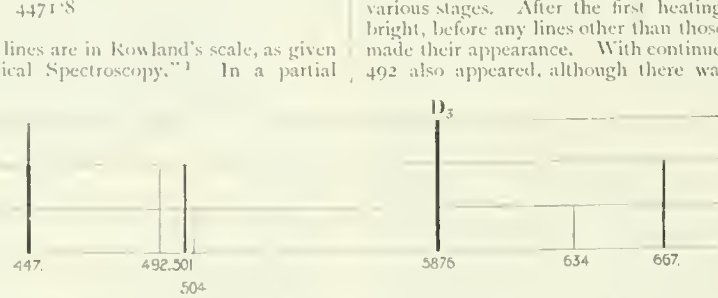


Fig. 2.—Diagram showing order in which lines appear in spectrum of vacuum tube when bröggerite is heated.

revision of his chromospheric list, Prof. Young gives the *corona* line 5316·79 as also having a frequency of 100 in the chromosphere, but, up to the present, this line has not been among those obtained in the laboratory.

"On the New Gas obtained from Uraninite." Fifth Note. By J. Norman Lockyer, C.B., F.R.S.

In a former communication I pointed out the spectroscopic evidence, furnished by the isolation of lines in certain minerals, which indicates that the complete spectrum obtained when broggerite is submitted to the distillation method is produced by a mixture of gases.

In order to test this view, I have recently made some observations, based on the following considerations:

(1) In a simple gas like hydrogen, when the tension of the electric current given by an induction coil is increased, by inserting first a jar, and then an air-break into the circuit, the effect is to increase the brilliancy and breadth of all the lines, the brilliancy and breadth being greatest when the longest air-break is used.

[1] Fr[illegible] translation, p. 1[illegible]4.

brightness in the yellow line; still further heating introduced additional lines 5048 and 6347.

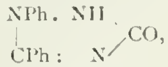
These changes are represented graphically in the following diagram (Fig. 2).

It was recorded further that the yellow line was at times dimmed, while the other lines were brightened.

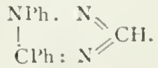
"On the Origin of the Triradiate Spicules of *Leucosolenia*." By E. A. Minchin.

Chemical Society, June 6.—Mr. A. Vernon Harcourt, President, in the chair. The following papers were read: The molecular refractions of dissolved salts and acids, by J. H. Gladstone and W. Hibbert. The authors show that in many cases when a pure substance dissolves in water, an alteration of its specific refractive energy occurs.—A comparison of some properties of acetic acid and its chloro- and bromo-derivatives, by S. U. Pickering. A number of thermal and other physical properties of acetic acid, and its monochloro- and monobromoderivatives have been quantitatively examined and compared; four distinct crystalline modifications of monochloracetic acid

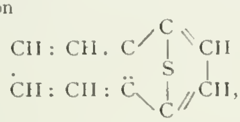
have been prepared.—$\beta\beta$-Dinaphthyl and its quinones, by F. D. Chattaway. Two quinones are obtained by oxidising $\beta\beta$-dinaphthyl under different conditions; from their chemical behaviour these seem to be β-naphthyl naphthoquinone, $C_{10}H_5.O.C_{10}H_7.O$ (1 : 2 : 4) and $\beta\beta$-di-α-naphthoquinone, $C_{10}H_5O_2.C_{10}H_5O_2$ (1 : 2 : 4 : 1 : 2 : 4).—Action of benzaldehyde on phenylsemicarbazide, by G. Young. The interaction of benzaldehyde and phenylsemicarbazide yields a diphenyloxytriazole



which on reduction gives diphenyltriazole



—Note on the latent heat of fusion, by N. F. Deerr. Acid compounds of some natural yellow colouring matters, part 1, by A. G. Perkin and L. Pate. The yellow colouring matters, quercitin, rhamnazin, rhamnetin, luteolin, fisetin and morin form orange or scarlet crystalline compounds with some of the mineral acids; catechin and maceurin do not yield such compounds.—Action of sulphur on α-nitronaphthalene, by A. Herzfelder. On heating a mixture of sulphur and α-nitronaphthalene an amorphous substance is obtained, which probably has the constitution



and to which the name $\alpha\alpha^1$-thionaphthalene is given.

Mathematical Society, June 13.—Major MacMahon, R.A., F.R.S., President, in the chair.—Mr. G. H. Bryan, F.R.S., communicated a note on an extension of Boltzmann's minimum theorem, by Mr. S. H. Burbury, F.R.S.—Dr. J. Larmor, F.R.S., gave a brief sketch of a paper by Mr. J. Brill, entitled "On the form of the energy integral in the variable motion of a viscous incompressible fluid for the case in which the motion is two dimensional, and the case in which the motion is symmetrical about an axis."—A paper by Dr. Routh, F.R.S., on an expansion of the potential function $1/R^{\kappa-1}$ in Legendre's functions, was taken as read.—Mr. Macaulay read a paper entitled "Groups of points on curves treated by the method of residuation." The President stated that Prof. A. M. Nash, of the Presidency College, Calcutta, had died on the voyage home, for a two years' furlough, after twenty years' residence in India.

Zoological Society, June 18.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—Mr. J. Graham Kerr read a paper on some points in the anatomy of *Nautilus pompilius*. The author advocated the abandonment of the view that the arms in Cephalopods are pedal, and the resumption of what appeared the inherently more probable view, that they are processes of the head-region. In conclusion, the author drew attention to certain indications which appeared to point to the Amphineura, and especially to the Chitons, as being of all living Mollusca those which most nearly approximate to the ancestral form of the time when the Cephalopods diverged from the main Molluscan stem.—A communication was read from Mr. F. E. Beddard, F.R.S., and Mr. A. C. Haddon, containing an account of a collection of Nudibranchiate Mollusca recently made by the latter in Torres Straits.—Mr. Boulenger read a paper on a large collection of fishes made by Dr. C. Ternitz in the Rio Paraguay.—A communication was read from the Babu Ram Bramha Sányál, giving an account of the moulting of some Birds of Paradise in the Zoological Gardens, Calcutta.—A communication was read from Mr. O. Thomas and Colonel J. W. Yerbury, giving a description of a collection of mammals made at Aden by Colonel Yerbury in the winter of this year. It was shown that thirty-six species of mammals were now known to occur in the Aden district.—A communication was read from Mr. Edwyn C. Reed, containing a list of the Hemiptera-Heteroptera of Chili.—Mr. H. H. Druce read a paper on Bornean butterflies of the family Lycænidæ, in which he had catalogued all the species already recorded from that island, and gave descriptions of a considerable number of new species, principally from Mount Kina-Balu. Mr. Druce stated that the number of butterflies of this family previously recorded from Borneo was about 75, and that his paper contained references to about 220.—A communication was read from Dr. A. G. Butler, containing an account of a small collection of butterflies sent by Mr. R. Crawshay from the country west of Lake Nyasa. Five species were described as new to science.—Mr. J. Anderson, F.R.S., read a paper describing a collection of reptiles and batrachians made by Colonel Yerbury at Aden and its neighbourhood during the past winter.—Mr. Boulenger, F.R.S., gave an account of the reptiles and batrachians collected by Dr. A. Donaldson Smith during his recent expedition in Western Somaliland and the Galla country.

Royal Meteorological Society, June 19.—Mr. R. Inwards, President, in the chair.—Mr. R. H. Curtis read a paper on the hourly variation of sunshine at seven stations in the British Isles, which was based upon the records for the ten years 1881-90. Falmouth is decidedly the most sunny station of the seven, having a daily average amount of sunshine of $4\frac{1}{2}$ hours. This amount is half an hour more than that recorded at Valencia, and three-quarters of an hour more than at Kew. Of the other four stations, Aberdeen, the most northern but at the same time a coast station, with 3·64 hours, has more than either Stonyhurst or Armagh, both inland stations; whilst Glasgow, with only 3 hours, or about a quarter of its possible amount, has the smallest record of the seven, a result to some extent due to the nearness of the observatory to the large manufacturing works with which the city of Glasgow abounds. At Valencia, Kew, Stonyhurst, and Armagh, the maximum duration is reached in May, the daily mean amount varying in the order named from $6\frac{3}{4}$ to 6 hours. At Falmouth and at the Scotch stations the increase goes on to June, when the mean duration at Falmouth reaches $7\frac{1}{2}$ hours, at Aberdeen $6\frac{1}{4}$ hours, and at Glasgow 5·6 hours. January and December are the most sunless months of the year. The most prominent feature brought out at all the stations is the rapid increase in the mean hourly amount of sunshine recorded during the first few hours following sunrise, and the even more rapid falling off again just before sunset.—Mr. H. Harries read a paper on the frequency, size, and distribution of hail at sea. The author has examined a large number of ships' logs in the Meteorological Office, and finds that hail has been observed in all latitudes as far as ships go north and south of the equator, and that seamen meet with it over wide belts on the polar side of the 35th parallel.

Royal Irish Academy, June 10.—Dr. J. K. Ingram, President, in the chair.—A paper on a basaltic hill of Tertiary age in county Galway, by A. MacHenry and Prof. W. J. Sollas, F.R.S., was read (communicated by permission of the Director-General of the Geological Survey). The extensive occurrence of basaltic dykes running with a general north-west to south-east direction through the whole northern third of Ireland has been described by Sir Archibald Geikie, who, in a bold but true generalisation, has referred them to the Tertiary period. The authors bring forward evidence of a still more southern and western extension of igneous activity in Ireland during this period, basaltic rocks similar to those of Antrim being shown to occur at Bunowen, seven miles south-west of Clifden, and thus about five or six miles north of the latitude of Dublin. They form a hill rising to a height of 200 feet above the surrounding plain, which is composed of gneissose rocks, through which the basalt has been extruded. The hill trends from north to south, and is 450 yards in length. It consists of olivine bearing dolerite, and vasicular basalt containing unaltered glass, and a substance which has been described [1] as a mineral under the name of "hullite." This substance is shown not only to occur in the vesicles of the basalt as volcanic glass does in the "amygdaloids" of the Tynemouth dyke described by Teall, but also to contribute to the ground mass, where it presents all the characters of an interstitial glass. Its most remarkable character is its extremely low specific gravity (1·76), which is small even for a hydrous volcanic glass, such as this so-called mineral must be admitted to be.

PARIS.

Academy of Sciences, June 17.—M. Cornu in the chair.—The President announced to the Academy the decease of M. Verneuil, member of the Medicine and Surgery Section.—A note on the law of absorption of bands of the oxygen spectrum, by M. J. Janssen.—On the necessarily harmonic form of displacements in ocean rollers, even when the

[1] "On Hullite," by E. T. Hardman and E. Hull (*Proc.* R. I. A., Second Series, vol. iii. p. 161.)

non-linear terms of the equations of movement are not neglected, by M. J. Boussinesq. On the combination of free nitrogen with the elements of carbon disulphide, by M. Berthelot. (See Notes, p. 202.) A new combination of argon, its synthesis and analysis, by M. Berthelot. (See Notes, p. 202.) Preparation and properties of pure fused molybdenum, by M. Henri Moissan. Pure fused molybdenum has been obtained by means of the electric furnace. Its properties and reactions are very fully given in the paper. Among these it is stated to have a density = 9·01, to be as malleable as iron, and capable of being filed cold or forged hot. When heated in contact with carbon, it forms a steel by cementation much harder than the pure metal. It is suggested that molybdenum may be used in the Bessemer process in place of manganese, because it furnishes a volatile oxide disengaged in the gaseous state, and any excess of the metal remaining in the iron would be as malleable as the iron itself, and similarly capable of being hardened.—Action of phenyl isocyanate on campholic, carboxylcampholic, and phthalic acids, by M. A. Haller.—Discovery of a third permanent radiation of the solar atmosphere in the gas from cleveite, by M. H. Deslandres. The line of wave-length 706·55 has been obtained in the spectrum of cleveite gas, using a very luminous tube. This corresponds to a third permanent chromospheric line, leaving now only the green line 531·66—the coronal line not obtained from terrestrial sources. The new line corresponds with a line observed in the argon spectrum by the author, employing argon prepared by means of lithium. It bears out the suggestion of Prof. Ramsay, that argon and cleveite gas contain a common constituent. Comparative observations with declinometers of different magnetic moments, by M. Ch. Lagrange.—On the molecular transformations of chromic hydrate, by M. A. Recoura.—On some basic halogen compounds of the alkaline-earthy metals, by M. Tassilly.—Action of heat on the double alkaline nitrites of metals of the platinum group: Iridium compounds, by MM. A. Joly and E. Leidie. Among the products of the action of heat on potassium iridium nitrite, the author signalises the compounds: $6IrO_2.K_2O$, and $12IrO_2.K_2O$.—On the ammonium sodium acid tungstates, by M. L. A. Hallopeau. The compounds $16WO_3.3Na_2O.3(NH_4)_2O.22H_2O$ and $12WO_3.4Na_2O.(NH_4)_2O.25H_2O$ are described.—Rotatory powers of some amyl derivatives in the liquid and gaseous states, by MM. Ph. A. Guye and A. P. do Amaral.—On synthesised colloids and coagulation, by M. J. W. Pickering. Synthetic colloids behave, when injected into the vascular system, in a very similar manner to the nucleo-albumins.—On a new bed of "cipolin" in the rocks of the Central Plateau, by M. L. de Launay.—Glacial and fluvio-glacial deposits of the basin of the Durance, by MM. W. Kilian and A. Penck.—On the coexistence, in the basin of the Durance, of two systems of conjugate folds of different age, by M. Emile Haug.—On the Jurassic and Cretaceous systems in the Balearic Islands, by M. H. Nolan.—On the Miocene of the Novalaise Valley, by MM. J. Revil and H. Douxami. Researches on the sugar and glycogen in lymph, by M. A. Dastre. Lymph contains an appreciable quantity (0·007 per thousand) of glycogen, obtainable by the usual methods. Glycogen is destroyed in lymph, in less than twenty-four hours, by a diastasic ferment (lymphodiastase). Rohmann has shown the existence of a ferment of this kind in lymph. The glycogen appears to be entirely carried by the solid elements, and absent from the liquid plasma. The doctrine that sugar is the circulating form of carbohydrate is thus confirmed.—Modification of the heat radiated by the skin, under the influence of continuous currents, by M. Lecercle. Demonstration, by a new pupillometer, of the direct action of light on the iris, by M. Charles Henry. Experimental production of generalised ganglionary lymphadenoma in a dog, by M. Pierre Delbet. The author has proved the infectious nature of this disease, and has isolated the pathogenic bacillus causing it.—On serotherapeutics in cancer, by M. Paul Gabier. Details of serum injections in two cases of cancer and the consequent effects.—Kirghiz Island and its hydrological peculiarities, by M. Venukoff.—The recent earthquakes and their periodicity, by M. Ch. V. Zenger.

BERLIN.

Meteorological Society, May 7.—Prof. Hellmann, President, in the chair.—Dr. H. Meyer spoke on most probable and mean temperatures of the air. He showed by several examples (Berlin, Nertschinsk, Alexandria) that the values of the summit of the curve of frequency and of the arithmetic mean exhibit a relationship to each other which is dependent on cloudiness, and shows diurnal and annual periodicities which are of considerable importance for the characterising of climate. The same speaker next dealt with the applicability of Lambert's formula to the calculation of the average direction of the wind. He showed that later observers had neglected Lambert's presupposition that either the velocity or pressure of the wind must be introduced into his formula, and had employed the "frequency" instead, a fact which must lead to worthless results. But even when the formula is employed in accordance with Lambert's instructions the resultant direction arrived at has no climatic significance. A lengthy discussion ensued, which the President summed up as indicating that Lambert's formula was not generally regarded as sufficing for the calculation of the average direction of the wind. Only in the case where the movements of the air lie close together for a given period, and do not differ by more than 2°, does it appear at all profitable to calculate the resultant by means of this formula.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Garden Flowers and Plants: J. Wright (Macmillan).—Longmans' School Algebra: W. S. Beard and A. Telfer (Longmans).—Bulletin of the U.S. National Museum, No. 48. A Revision of the Deltoid Moths: Dr. J. B. Smith (Washington).—Heligoland as an Ornithological Observatory: H. Gatke, translated by R. Rosenstock (Edinburgh, Douglas).—An Introduction to Chemical Crystallography: Dr. A. Fock, translated and edited by W. J. Pope (Oxford, Clarendon Press).—Leitfaden for Histiologische Untersuchungen: Dr. B. Rawitz, Zweite Auflage (Jena, Fischer).—Das Pflanzenphysiologische Praktikum: Dr. W. Detmer, Zweite Auflage (Jena, Fischer).—Untersuchungen über die Starkekorner: Dr. A. Meyer (Jena, Fischer).—A Text-Book of the Science and Art of Bread-Making: W. Jago (Simpkin).—The Structure and Life of Birds: F. W. Headley (Macmillan).—Photography Annual for 1895 (Iliffe).—Exterior and Interior Photography: F. W. Mills (Dawbarn).—La Geologie Comparée: Prof. S. Meunier (Paris, Alcan).—Mind and Motion and Monism: Dr. G. J. Romanes (Longmans).

PAMPHLETS.—Protoplasme et Noyau: Prof. J. Pérez (Bordeaux).—Ueber die Auslese in der Erdgeschichte: Dr. J. Walther (Jena, Fischer).—Walks in Belgium (50 Fleet Street).

SERIALS.—Bulletin de l'Académie Royale des Sciences, &c., de Belgique, Tome 29, Nos. 4 and 5 (Bruxelles).—American Journal of Mathematics, Vol. xvii. No. 3 (Baltimore).—Botanische Jahrbucher für Systematik, Pflanzengeschichte und Pflanzengeographie, Einundzwanzigster Band, 1 and 2 Heft (Leipzig, Engelmann).—Morphologisches Jahrbuch, 22 Band, 4 Heft (Leipzig, Engelmann).—Economic Journal, June (Macmillan).—Royal Natural History, Vol. 4, Part 20 (Warne).—Travaux de la Société des Naturalistes à l'Université Imperiale de Kharkow, tome xxviii. 1893-94.—Quarterly Journal of Microscopical Science, June (Churchill).—Astrophysical Journal, June (Chicago).—Bulletin of the Geographical Club of Philadelphia, Vol. 1, No. 5 (Philadelphia).—Zeitschrift für Wissenschaftliche Zoologie, lix. Band, 3 Heft (Leipzig, Engelmann).—Longman's Magazine, July (Longmans).

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THURSDAY, JULY 4, 1895.

THE MOLUCCAS.

Reisen in den Molukken, in Ambon, den Uliassern, Seran (Ceram) und Buru. Eine Schilderung von Land und Leuten. Von K. Martin. Large 8vo. Pp. xviii. and 404, and volume of plates. (Leyden: E. J. Brill, 1894.)

THE Moluccas, the spice islands of the farthest east, were the most powerful magnets which drew the fleets of Portugal eastward around the Cape of Good Hope in the fifteenth century, and in the sixteenth induced Magellan to start on that voyage through his straits which culminated in the first circumnavigation of the globe. They were the most coveted lands on earth at the commencement of the history of modern discovery, the most eagerly sought for, first acquired, and the most firmly held of the colonies of Europe. Yet while the group has changed hands again and again without passing out of European ownership, the islands are still most imperfectly known. The system of exclusion which animated Portuguese, Spaniards, and Dutchmen alike, discouraged systematic exploration; and the works of such travellers as have recently made explorations in the islands, are for the most part locked up from the general reader in the Dutch language. In English, indeed, there is the matchless work of Wallace; but this, like several later English books on the Malay archipelago, is mainly concerned with the study of biological conditions.

Dr. Martin, the Professor of Geology in the University of Leyden, already well known for his admirable work on the Dutch West Indies, obtained a grant from his Government in 1891, and with a year's leave of absence proceeded to the study of some of the more interesting and less known islands of the Malay archipelago. Leaving Batavia on November 3, 1891, he coasted along Java, touched at Bali, visited various points on Celebes and Jilolo, making such geological and general observations as were possible in the limited time at his disposal. On November 27 he reached Amboyna, and until July 27, 1892, he devoted his whole time to detailed exploration, determining positions and heights, photographing scenery, people and houses, and collecting everything that came in his way in the islands of Amboyna, the Uliasser, Buru, and Ceram. In this way many places were visited which had never been adequately described before, several districts which had never been traversed by Europeans, and some which even the natives had avoided as sacred or unclean. The book is mainly geographical, going so far into the structure and vegetation of the land as is necessary in order to understand the life-conditions of the inhabitants, on whom also great attention is bestowed. Detailed reports on the geology, botany, the birds, insects, and other collections are being prepared by Prof. Martin and other specialists; but here he confines himself to the narrative of his expedition, with numerous explanations suggested by the preliminary results.

We so often find that books of travel are flabby masses of ill-upholstered padding, put together at second-hand by some big-game hunter or globe-trotter after his return, that we feel it a duty to call attention to the admirable form and substance of this one. It is of the order of Darwin's, Wallace's, and Bates' work, and though based on shorter experience than theirs, is none the less scientifically put together.

Prof. Martin says in his preface, that he gives a pure record of actual observations taken directly from his note-books and collections. After writing it, he proceeded to read up all the available literature on the subject, and took occasion in a series of footnotes to explain discrepancies or criticise his predecessors. In many respects this is an excellent method to pursue. The mind is free from prejudice or anticipation, and the observations bring the charm, and leave the stimulus of discoveries. On the other hand, unless what is known is previously worked up, there is apt to be much loss of time which could be more profitably spent, and points of the highest interest, being unsuspected, may pass unnoticed. We are inclined to believe, however, that, in spite of his modest disclaimer, Prof. Martin had a very good notion of what had been done before he entered the field. Otherwise he could scarcely have been so self-denying as to turn away from the people of Amboyna and the Uliasser, who have been fully studied by van Hoëvell, Joest, Riedel, and others, and give attention mainly to the features of the land. And in Ceram he knew very well where the coast-lines were faultiest on the maps, and the mountains and rivers scattered according to the freest fancy of the cartographer; for there he proceeded to fix positions and draw maps, while giving attention at the same time to general collecting and to the study of the people and their customs.

In view of the distrust which has gradually beset the aneroid when used for measuring heights, it is interesting to learn that the result of Dr. L. S. Siertsema's discussion of the numerous aneroid readings made on this journey is to show that it is, after all, an instrument of considerable precision for elevations well under 10,000 feet.

The book is to be welcomed as the thoroughly sound work of an experienced man of science, and as containing a notable contribution to our scanty knowledge of a most fascinating region, and of primitive peoples whose ancient customs are fast giving way before the pervading European influence. It suggests forcibly the importance of the study of regional geography in those places where the natural equilibrium of life and physical environment has not been disturbed; such places as are now scarcely to be found. It is exasperating to think that the careless traders and earnest missionaries who haunt the islands of the sea are every day rejecting sybilline books, the value of which seems likely to remain unsuspected, save to a handful of anthropologists, until the last of them is destroyed beyond recovery. The facts that primitive man must be studied at once if he is to be studied at all, and that purely natural floras and faunas are doomed to early disappearance from this planet, call for more workers like Prof. Martin, and demand them soon. The demand is for educated scientific explorers such as there is at present no means for training in this country. The day when geographical work of the first magnitude can be done by mere pluck and perseverance is almost past, and

the explorer of to-day must add to his enterprise scientific training, and to scientific training diligent study.

The contrast between the means of training for explorers in this country and on the continent, suggests many curious thoughts as to the proportion in which different countries will undertake the detailed study of the earth's surface in all its aspects, from which alone a true view of nature can be obtained. The theoretical training in geography only to be obtained in continental universities, and the practical training in the use of instruments and methods offered only by the Royal Geographical Society, are too far apart, and until they come together the general level of original work in unexplored countries will fall far short of the standard set by Prof. Martin. HUGH ROBERT MILL.

MILL ENGINEERING.

Steam Power and Mill Work. By George William Sutcliffe, M.Inst.C.E. (London: Whittaker and Co., 1895.)

THE Specialist Series of technical books is well known and appreciated; many subjects are admirably treated by well-known authors. The present volume, of some 800 pages, is no exception to this rule, and it fully maintains the high character of the series. In the preface we are told that this work aims at giving an account of modern practice for the consideration of those interested in the manufacture, control, and operation of boilers, engines, and mill work, also of the leading principles and calculations affecting such work, most of the subject-matter being based upon the personal experience of the author. Useful information has also been obtained from the minutes of proceedings of the leading technical societies and from various journals, adding considerably to the value of the book. Taken as a whole, this work will be of much use to steam users, particularly those employing steam power extensively with much machinery in operation.

It is impossible to notice in the space at our disposal the large amount of ground covered in this book. The author has arranged his matter in a sensible manner, and explains himself in a practical way. Many steam users are under the erroneous impression that the economical firing of a boiler is easily accomplished, that any labourer is capable of handling the shovel; to such men we recommend a careful perusal of this work, wherein they will discover that economical firing means more than they anticipated.

The author has much to say in chapter vi. on convection, circulation, evaporation, and priming in boilers. These points are thoroughly well discussed, being all-important to the life of the boiler and the comfort of the user. The estimation of the moisture in steam as delivered from the boiler is often necessary, and the difficulty of obtaining a true sample of the steam has to be met. Much useful information is given on this subject, and we would draw the attention of the author to an instrument designed by Prof. Watkinson, of Glasgow, which appears to give true results for the direct estimation of small quantities of water in steam.

The different methods of forced draught are discussed in the following chapter. The author, when describing the closed stoke-hole system, observes: "It is scarcely possible to imagine a case in which it would be wise to adopt a closed stoke-hole in stationary work." This is a very usual opinion held by engineers, who believe that most of the break-downs in the Navy, through leaky tubes, may be traced to this system of forced draught being adopted. The question of gas firing appears to be coming to the front for steam boilers in towns, for the prevention of smoke, and an increased economy in fuel. There is no doubt that a good deal can be said in favour of the system. The author has much useful information on the subject, particularly on the production of gas for the purpose. Liquid fuel, again, is another innovation in the way of raising steam. This system has reached its present state of perfection in the hands of Mr. Holden, the locomotive engineer of the Great Eastern Railway, who has successfully applied it to locomotives and stationary boilers.

The important subject of a pure water supply for steam boilers is pointed out in the following chapter. The effects of different impurities are described, besides the dangerous results involved by admitting grease into a boiler with the feed-water through contamination with the exhaust steam. Many furnaces have collapsed from this cause. We now come to the more mechanical part of the book, commencing with the construction and general fittings of Lancashire and Cornish boilers. Taken as a whole, the subject of boilers generally is fairly well dealt with. In the paragraph on internal flues, we find no description of Fox's corrugated flues, nor those of the Farnley Company; both are very commonly in use, and should have been mentioned. Under the head of "riveting" it might be well to point out that, although steel rivets are now the general rule when closed by machine, the few hand rivets necessary should in all cases be of Yorkshire iron. Caulking is now generally done by steam or pneumatic tools, the best of which is certainly Macewan Ross's patent, of Glasgow. Probably the most important fitting for a boiler is the glass-water gauge, and this should close automatically if the glass breaks. There are many of these in the market, more or less trustworthy; those supplied by Messrs. Dewrance and Co. being among the best. The author recommends the pendant syphon arrangement for fixing the pressure gauge to the boiler. This allows too much heat to reach the gauge through the heavy metallic fitting, and cannot be recommended for this reason. The locomotive type of stationary boiler is being largely used for steam raising: it is economical, easily set in position, and produces large quantities of steam when pressed.

Under the heading of "Types of Steam Engines," we find much information of a varied nature. The Willans central valve engine is, however, not described. This is a pity, because it is now being largely used for ordinary work, and gives great satisfaction. It is most economical, and will run for months without attention. The author goes into much detail when discussing valve arrangements for steam engines, commencing with the well-known "technical school" diagrams of slide valves with and without lap, &c., and ending with the piston valve; then follows double beat valves, Corlin valves, and many others. All these descriptions are clear and to the point.

Chapters xxiv. to xxxiv. may be said to contain descriptions of the construction and design of the principal parts of steam engines. Some formulæ are given, as well as a few maximum pressures allowable on the different parts. On page 428, the author says that the pressure of 80 lbs. per square inch of bearing surface is allowed in locomotive practice between the slide blocks and bars, *when both surfaces are of hardened steel*. It is not the usual practice to make the slide block surfaces of hardened steel, and in engines built years ago, the pressure per square inch very much exceeded this limit. In most recent practice with cast-iron bars and slide blocks, this limit may be safely used. The taking of indicator diagrams is always one of interest. Chapter xxv. deals very thoroughly with this subject. Trials in connection with the power and efficiency of engines and boilers naturally follow the indicator, and very complete instructions are given for carrying these out, including precautions in advance of the trial. The concluding chapters of this work deal principally with mill work in its many branches. Friction and lubrication are explained, and many valuaole hints are given. This book should prove of assistance to the steam user. The information given is of such a nature which will appeal to his partial knowledge of the subject, and render him more capable of understanding machinery generally.

N. J. L.

LECTURES ON DARWINISM.

Lectures on the Darwinian Theory. Delivered by the late Arthur Milnes Marshall, M.A., M.D., D.Sc., F.R.S., Edited by C. F. Marshall, M.B., B.Sc., F.R.C.S. (London: David Nutt, 1894.)

ALL the characteristics of the late Prof. Milnes Marshall are strikingly apparent in these lectures. In dealing with the many aspects of a subject which is often imperfectly understood, these lectures are clear and forcible, and the metaphors apt and convincing.

The first lecture deals with the history of the theory of evolution, and contains a concise and interesting epitome of the growth of this great conception, together with a brief account of the chief writers on the subject. The relationship between the process of evolution and the causes upon which it depends are perhaps liable to misinterpretation, the want of any feasible suggestion as to the latter being spoken of as a "fatal flaw" in, or a "fatal objection" to the former. Undoubtedly the want of some efficient cause at first prevented a wide belief in evolution, but logically the two questions are entirely distinct, and the evidence for evolution itself would stand undisputed, even if every one of the causes which now find acceptance were to be abandoned for ever. We know that Darwin himself was a convinced evolutionist long before his discovery of the principle of natural selection.

The second lecture treats of artificial and natural selection, and is accompanied by useful figures showing some of the changes which man has been able to accomplish in the creation of his domestic breeds. The whole lecture is clear and telling, the last paragraph being alone liable to possible misconception. In stating that "every species is for itself and for itself alone," it would have been advisable to bring forward instances in which a species benefits itself by benefiting others. It is most probable that such cases were described in the actual delivery of the lecture.

Then follow the arguments in favour of evolution, palæontology being first considered. We here meet, as in many of the other lectures, with exceedingly apt quotations from Darwin, Wallace, and others. It is an unfortunate omission that references are not given. In the delivery of the lectures to a general audience they may have been out of place, but there could have been no difficulty in their insertion in the present volume. Here, too, we find many useful figures of some of the extinct forms which are of the highest interest to the student of evolution. The reasons for the imperfection of the geological record are very excellently, and yet briefly, surveyed; and the same may be said of the sketch of the argument from geographical distribution, in which, however, by an obvious slip, the forest region of Brazil is spoken of as "south of the river La Plata" (p. 75).

The argument from embryology was probably the most congenial to the lecturer. This chapter is well illustrated, and contains more detail than the others. The term "acquired or larval characters" (p. 103) is open to exception, and the statement that rudimentary organs must be "inherited, for in no other way can their presence be explained" (*loc. cit.*), is too brief to be clear. It is probable that this sentence served as a note to be expanded by the lecturer; but it also required expansion by the editor. The chapter will be found extremely interesting and instructive by those who wish to read a popular account of the bearing of embryological facts upon the Darwinian theory.

The chapter on the colours of animals and plants, although containing much information in a little space, is not worked out in so complete and balanced a form as the other chapters, and in large part consists, apparently, of notes for the lecturer's use. It is erroneously stated that the colours of certain lepidopterous larvæ are due to their food, and some of the supposed examples of the direct action of environment are by no means proved to be caused in this way.

Then follows an interesting lecture on the "objections to the Darwinian theory." The figures of Pteropus on p. 165, although sufficient in themselves, are clumsily arranged. Here, too, many aspects of the subject are only treated in brief lecturer's notes, although these frequently contain trenchant remarks.

The "origin of vertebrated animals" is next considered, and the series concludes with an excellent epitome of "the life and work of Darwin."

It will be seen that the sequence of subjects is a very natural one, and well calculated to lead a general audience to follow and understand the most prominent and important aspects of the Darwinian theory.

E. B. P.

OUR BOOK SHELF.

My Climbs in the Alps and Caucasus. By A. F. Mummery. Illustrated. (London: T. Fisher Unwin, 1895.)

MR. MUMMERY is a bold man. Not only has he dared greatly among peaks and glaciers, but also he does not scruple to declare that he enjoys mountain

climbing for its own sake. He leaves science for others, cares nothing for topography except as ministering to his pastime, and holds a plane-table in abhorrence. Thus between his book and Sir W. M. Conway's "Climbing in the Karakoram Himalayas," there is a great difference. Still this is common to both: a delight in the wild beauty and silent grandeur of the crags, pinnacles and snows of the higher peaks. There is, no doubt, a beauty in the Alps which all the world can see, as Ruskin has truly remarked; but there is another aspect, solemn, almost stern, yet with a strange, thrilling fascination, which he only can appreciate who has grasped their rocky ledges, or planted his ice-axe in their unsullied snows. Vain it is to rebuke Mr. Mummery for treating the mountains like greased poles. He retorts, unabashed, that the pole is slippery, not greasy, and that he enjoys trying to climb it. But he seeks not to vulgarise the mountains; he has no love for the crowd of tourists which now annually deluges the Alps, nothing but contempt for the cockney "mountaineer" who is hauled up a peak by his guides, like a bale of goods, or who makes an ascent simply because it is "the thing to do." Perhaps Mr. Mummery may sometimes carry daring beyond the verge of rashness. It is to be hoped that few readers of this book will be tempted to follow his example of making difficult ascents without guides; for such work requires not only gymnastic skill, but also knowledge and judgment, which very few amateurs can ever acquire. Still it is difficult to avoid sympathising with his love of a struggle—it is the spirit which has made England great, a spirit which is too often lacking in this age of molluscous sentimentality and invertebrate opportunism.

Mr. Mummery's book, as we have said, contains no science and hardly any geography, but those who love the story of a plucky scramble, clearly told in good pithy English, will be loth to lay it down. It is well illustrated, with a number of small sketches introduced into the text, and eleven full-page pictures from either drawings or photographs. One or two of these will repay study as fine examples of the forms of weathered crags. None is better than the photogravure of the lower peak of the Aiguille Grépon. Among the expeditions described are two ascents of the Matterhorn by unwonted routes, a passage of the Col du Lion and Col des Courtes, ascents of the Teufelsgrat (written by Mrs. Mummery), of the Aiguilles des Charmoz, Grépon, du Plan, Verte (also by two unwonted routes), and of the Dent du Requin. The chapters on the Caucasus describe some fine excursions, the chief of which is the first ascent of the Dychtau (17,054 feet) in 1888, a magnificent peak, called in the *Alpine Journal* of that date Koshtantau, for apparently this and a slightly lower summit to the east (climbed by another party in the following year) indulge in a distracting habit of exchanging names. In a concluding chapter Mr. Mummery discusses various moot points in Alpine craft, advocating a preference for "two on a rope" in difficult places, a preference which is not likely to pass unquestioned by some of his brother climbers. T. G. Bonney.

Dairy Bacteriology. By Dr. Ed. von Freudenreich. Translated by J. R. Ainsworth Davis. (London: Methuen and Co., 1895.)

An English translation of Dr. Freudenreich's little book appears very appropriately at the present juncture, when serious efforts are at length being made to raise the standard of our dairy produce by providing special courses of study for those engaged in its production. Although some of the peripatetic instruction on dairy-work instituted in various districts by local County Councils has not been attended with the success anticipated, yet there can be no doubt that systematic training in this direction is very urgently required. As the translator truly remarks, "Not only Denmark, but America, France, Germany, and Switzerland are far ahead of us in these matters, and compete against home dairy products with only too much success, while Australia is rapidly becoming another serious rival." The information contained in "Dairy Bacteriology" as to the *scientific* origin of some of the troubles with which, in actual practice, the manufacturer of dairy produce is only too well acquainted, will doubtless be a revelation to many, whilst the instructions given for their successful elimination from the dairy, should at any rate impress the student with the hopelessness of attempting such delicate operations as are involved in dairy work without an adequate knowledge of the various parts played by bacteria in dairies.

The little volume is but an introduction to the subject, otherwise we should have been justified in expecting a better account of the milk-microbes which have been discovered; it is, however, written in an attractive manner, and the author has, moreover, succeeded in making it interesting and readable to the public generally, who as consumers are even more concerned than the manufacturers in the hygienic aspects of our dairy produce.

We note that an edition of this useful little manual has already appeared in French, Italian, and Hungarian, and it only remains for us to congratulate Prof. Davis upon the excellent manner in which he has translated it into English.

Longmans' School Algebra. By W. S. Beard and A. Telfer. Pp. 528. (London: Longmans, Green, and Co., 1895.)

So far as abundance of examples goes, this book is in advance of other text-books of algebra. There are as many as 5200 examples in the book, 500 of which are collected as miscellaneous examples at the end. Teachers who like to have plenty of material upon which to exercise their pupils' minds, will find that this volume satisfies their requirements. It seems hardly necessary, however, to include in a school algebra such a very large number of examples; in our opinion, the volume would have been improved by omitting many of them, and amplifying the very scanty descriptive text.

Fallacies of Race Theories as Applied to National Characteristics. By the late W. D. Babington, M.A. Pp. 277. (London: Longmans, Green, and Co., 1895.)

Mr. H. H. G. MacDonnell prefaces these collected essays with a brief statement of the views expressed in them. The late author contended that the mental and moral characteristics of nations are mainly the result of environment, and are not derived from ancestors by heredity. The transmission of physical characteristics is not taken into consideration, and the treatment throughout is more historical than scientific.

A Chapter on Birds. By R. Bowdler Sharpe, LL.D., F.L.S. Pp. 124. (London: Society for Promoting Christian Knowledge, 1895.)

Eighteen of our rare avian visitors, and their eggs, are brilliantly depicted by chromo-lithography in this attractive volume for lovers of birds. Dr. Sharpe's notes on the life-histories and natural relations of the different species, furnish instructive reading for young students of ornithology. Such a volume ought not, however, to be published without an index.

Nature in Acadie. By H. K. Swann. Pp. 74. (London: John Ball and Sons, 1895.)

From the observations of birds, insects, and other forms of life, made by the author while on a voyage to Nova Scotia, and diffusely recorded in this book, it is possible to find notes of interest to naturalists. A systematic list of the species of North American birds mentioned in the text, is given in an appendix.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Size of the Pages of Scientific Publications.

IT was with much surprise that we received the circular of the Royal Society stating that it had been decided to abandon the present size of its *Proceedings* in favour of royal octavo, accompanied by a voting card on the question of a similar change in the size of the *Transactions*. At the Oxford meeting of the British Association, a Committee was appointed, by Section A, to endeavour to secure greater uniformity in the sizes of the pages of the *Transactions* and *Proceedings* of all societies which publish mathematical and physical papers. In view of the report which that Committee will present shortly at Ipswich, it is much to be hoped that the Council of the Royal Society will take no immediate steps toward carrying their recommendations into effect.

A considerable degree of uniformity already exists. The present octavo size of the *Proceedings of the Royal Society* is very nearly the same size as the *Philosophical Magazine*, the *Report of the British Association*, the *Proceedings of the London Mathematical Society*, and of the *Cambridge Philosophical Society*, and many other publications. The *Annalen der Physik und Chemie* is so very little smaller, that reprints from it can be bound up with others from the afore-mentioned sources, without paring down their margins excessively. For papers involving long mathematics or large diagrams, the quarto size of the present *Philosophical Transactions* approximates to uniformity with the *American Journal of Mathematics*, the *Comptes rendus* of the Académie des Sciences of Paris, the *Cambridge Transactions*, the *Edinburgh Transactions*, and numerous other quarto *Transactions*, such as those of the Institution of Naval Architects.

It is very important that specialists in any branch of science should be able to collect, and bind together, reprints of papers on their own particular subjects, and such volumes are of permanent value as works of reference. So long as there are only two sizes to deal with—the above-mentioned quarto and octavo—there is little difficulty about this, but occasionally one comes across a paper of intermediate size, which cannot be bound up with either, and the collection is thus necessarily incomplete. It is hoped that the report, so shortly to be presented, will be a guide to authors of papers in indicating which publications to select, and which to avoid, if they desire to conform to the average standard sizes. Although the work of the Committee is at present confined to mathematical and physical papers, it might perhaps be of advantage that the matter should be discussed in, and representatives on the Committee appointed from the other Sections of the British Association as well. The question of changing the size of the *Proceedings* was recently discussed by the London Mathematical Society, but it was decided to retain the existing form, at any rate for the present, mainly on account of its uniformity with other publications. It will be most unfortunate if the Royal Society takes any retrograde step which may prevent the sizes of its *Proceedings* and *Transactions* from being adopted as the standards.

G. H. BRYAN,
SYLVANUS P. THOMPSON.

On the Minimum Theorem in the Theory of Gases.

YOU would oblige me by inserting the following lines in NATURE. The last remark made by Mr. Burbury points out, indeed, the weakest point of the demonstration of the H-theorem. If condition (A) is fulfilled at $t=0$, it is not a mechanical necessity that it should be fulfilled at all subsequent times. But let the mean path of a molecule be very long in comparison with the average distance of two neighbouring molecules: then the absolute position in space of the place where one impact of a given molecule occurs, will be far removed from the place where the next impact of the same molecule occurs. For this reason, the distribution of the molecules surrounding the place of the second impact will be independent of the conditions in the neighbourhood of the place where the first impact occurred, and therefore independent of the motion of the molecule itself. Then the probability that a second molecule moving with given velocity should fall within the space traversed by the first molecule, can be found by multiplying the volume of this space by the function f. This is condition (A).

Only under the condition, that all the molecules were arranged intentionally in a particular manner, would it be possible that the frequency (number in unit volume) of molecules with a given velocity, should depend on whether these molecules were about to encounter other molecules or not. Condition (A) is simply this. that the laws of probability are applicable for finding the number of collisions.

Therefore, I think that the assumption of external disturbances is not necessary, provided that the given system is a very large one, and that the mean path is great in comparison with the mean distance of two neighbouring molecules.

LUDWIG BOLTZMANN.

9 Tuerkenstrasse, Vienna, June 20.

Argon and the Kinetic Theory.

THE spectrum exhibited by argon undoubtedly shows that, under the conditions of the experiment, the molecules composing the gas are set into an intense state of vibration, while the ratio of the specific heats (5/3, about) shows, according to the equation $\beta = \frac{2}{3}\frac{1}{\gamma - 1}$, that $\beta = 1$, and therefore the gas is, as pointed out by Lord Rayleigh, monatomic, and cannot therefore be capable of vibrating. But there is, I think, a very simple explanation of this apparent contradiction, and that is, that the above equation is not true, and that it should be, as will be proved hereafter, $\beta = 3k(\gamma - 1)$, where k is very nearly 1 for argon and other so-called permanent gases. This latter equation gives 2 for the value of β in argon, a value easily understood.

The virial equation for smooth elastic spheres *of finite magnitude* is $\frac{3}{2}PV = \Sigma\frac{1}{2}mv^2 - \frac{1}{2}\Sigma Rr$; and since the resilience is unity and r finite, the term $-\frac{1}{2}\Sigma Rr$ cannot vanish. Now the term $\frac{3}{2}PV$ represents work or its equivalent of energy; hence the right-hand member of the equation must represent the same, and since the term $\Sigma\frac{1}{2}mv^2$ is obviously kinetic energy, or its equivalent of work, the term $-\frac{1}{2}\Sigma Rr$ must also represent work or energy. Now we can find the value of $\frac{3}{2}PV$ in terms of $\Sigma\frac{1}{2}mv^2$, as follows. Imagine a cube box so constructed that one side of each pair can be used as a spring to discharge any mass in contact with a velocity v. And suppose three smooth elastic spheres each of mass $\frac{M}{3}$ to be discharged by the three spring sides with the above velocity into the interior of the box. Then the work done on each mass will be $\frac{1}{2} \cdot \frac{M}{3}v^2$. Put this equal to PV and take V equal to the volume of the box. The total work done is evidently $3PV = \frac{1}{2}Mv^2$. If, instead of three elastic spheres, we imagine a very great number of very minute ones of the same total mass to be discharged by the spring sides with the same velocity, the energy will be the same as before, and the above equation will still be applicable; and the state of affairs now represented would be that of an ideal gas. But owing to collisions after first starting the velocities of the particles will vary, and therefore we must write the equation

$$3PV = \frac{1}{2}M\overline{v^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $\overline{v^2}$ is the mean square velocity of the particles. By hypothesis V has the same value in the above equation as in the virial equation; and P can be proved, *if necessary*, to have the same value in the two equations as follows.

If f = the *mean* acceleration or retardation, as the case may be, of the cr. of gr. of an elastic sphere impinging directly against a plane; then $ft = v$. Also $f = \frac{v^2}{2s}$, $\therefore t = \frac{2s}{v}$. Here t is half the time of impact, and v the velocity normal to the plane before and after impact. Now if it can be shown that the time taken by the spring side of our imaginary box to give the same velocity is the same as the above, then it is obvious that the mean pressures in the two cases must be identical.

Assume s^3 to be the volume of the cube box, then s^2 is the area of each side. Now let the spring side be drawn back so as to act through a distance s on the mass $\frac{M}{3}$ with a constant pressure P per unit of surface; then $Ps^2 \times s = PV$ represents the work done. The velocity given to the mass is v, and the acceleration constant. Hence the mean velocity of the spring

side in passing through the distance s is $v/2$, and the time is $s \div v/2 = 2s/v$, the same as in the first case. Which proves the proposition.

Since from (1) we have $3\ PV = \frac{1}{2}\ M\overline{v^2}$ or $\frac{3}{2}\ PV = \frac{1}{4}\ M\overline{v^2}$, we may substitute this value in the virial equation, and remembering that $\Sigma\frac{1}{2}mv^2 = \frac{1}{2}M\overline{v^2}$, we get $-\frac{1}{2}\Sigma Rr = -\frac{1}{4}M\overline{v^2}$. Hence also

$$P = \tfrac{1}{3}\,\rho\overline{v^2}\ .\ .\ .\ .\ .\ .\ .\ .\ .\ (2)$$

where $\rho = \frac{M}{V}$ the density. The above equation is easily obtainable without the use of the virial equation *when the time of impact* is taken into consideration. A phenomenon which cannot be assumed to be instantaneous without upsetting the dynamical definition of the measurement of a force; which expressed algebraically is $Ft = Mv$. From which it is evident that when t the time is 0, v the velocity, is also 0.

When the virial equation is made applicable to the case of a gas composed of molecules capable of vibrating, it seems obvious that the term $\Sigma\frac{1}{2}mv^2$ should be written $\Sigma\frac{1}{2}\beta m\overline{v^2}$; because, as shown by Clausius, the internal energy of the molecules bears a constant ratio to the energy of agitation. We must look to the *mechanical structure of the molecule* for the reason of this. Here the fact is simply accepted, not explained; but it is obvious that the same forces which impart translatory energy to a molecule will impart vibratory energy also. The same reasoning applies to the term $-\frac{1}{2}\Sigma Rr$, which now becomes $-\Sigma\beta(Rr)$. The volume of the gas is unaltered by the vibrations, and the pressure is dependent on the two other terms. Hence the equation may be written

$$\tfrac{3}{2}PV = \Sigma\tfrac{1}{2}\beta m v^2 - \tfrac{1}{2}\Sigma\beta(Rr)\ .\ .\ .\ .\ .\ .\ (3)$$

And from this we get

$$P = \tfrac{1}{3}\bar{\beta}\rho v^2\ \ .\ .\ .\ .\ .\ .\ .\ (4)$$

The above equation may be written

$$P_2 = \tfrac{1}{3}\rho v\bar{v}_i\ ;\ .\ .\ .\ .\ .\ .\ .\ .\ .\ (5)$$

Where $\bar{v}_i = \beta\bar{v}$. Again equation (2) may be written

$$P_i = \tfrac{1}{3}\rho_i v_i^2\ :\ .\ .\ .\ .\ .\ .\ .\ .\ .\ (6)$$

the suffix i denoting that the pressure, density, and mean square velocity are those of an ideal gas composed of smooth elastic spheres.

If P_i, ρ_i, and $\bar{v}_i$ in (6) are taken respectively equal to P, ρ, and v_i in (5); then it is evident that v_i in (5) is the velocity of mean square of an ideal gas which, having the same density, would give the same pressure as a natural gas. Hence v_i can be found from (6). Now the total energy in unit mass of a gas is given by the equation

$$K_v T = \tfrac{1}{2}\beta v^2\ :\ .\ .\ .\ .\ .\ .\ .\ .\ (7)$$

where K_v is the specific heat at constant volume, and T is the absolute temperature. From which equation $v\sqrt{\beta}$ can be found. We have also from above

$$v\beta = v_i\ \therefore\ \sqrt{\beta} = \frac{v_i}{v\sqrt{\beta}},\ .\ .\ .\ .\ .\ .\ (8)$$

from which equation the value of $\sqrt{\beta}$ and consequently β can be found.

The equation $\beta = 3k(\gamma - 1)$ can now be proved as follows. Multiplying both sides of (4) by V, the volume of unit mass, and combining with (7), we get

$$K_v T = 3PV\ .\ .\ .\ .\ .\ .\ .\ .\ (9)$$

Now from (5) and (6), taking $\rho = \rho_i$ we get $P = P_i/\beta$, and substituting in (9) $K_v = 3P_iV/\beta T$. But $P_iV/T = K_{ip} - K_{iv}$; or the difference between the specific heats at constant pressure and constant volume; the suffix i indicating, as before, that the symbols refer to an ideal gas. Hence

$$\beta = \frac{3(K_{ip} - K_{iv})}{K_v} = \frac{3k(K_p - K_v)}{K_v} = 3k(\gamma - 1)\ \ .\ (10)$$

Here k is some factor which for so-called permanent gases is very nearly unity. For such gases we may write (10)

$$\beta = 3(\gamma - 1)\ ;\ \text{or}\ \gamma = \tfrac{1}{3}(\beta + 3)\ \ .\ .\ .\ .\ (11)$$

In the following table the values of β, except in the case of argon, are calculated from equation (8); and $\bar{v}_i$, the velocity of ideal gases having the same pressure and density as their corresponding natural gases, at standard temperature and pressure, from (6). The velocities are given in feet per second, and the value of gravity is taken at 32·2. Column (4) gives the values of γ for the different gases calculated from equation (11); and column (5) gives the experimental values of γ. The close agreement between these values is a significant fact.

	(1) β	(2) $\bar{v}_i$	(3) $\bar{v}$	(4) $\gamma=\frac{1}{3}(\beta+3)$	(5) γ Experiment	(6) k
Hydrogen	1·234	8551	6925	1·4115	1·412	1·00035
Oxygen	1·197	2140	1787	1·399	1·402	1·0021
Nitrogen	1·227	2282	1860	1·409	1·411	1·0014
Dry air	1·222	2250	1841	1·407	1·409	1·0014
Argon	2 (about)	1940	970	...	1·7	...

8, Norfolk Square, W., June 13. C. E. Basevi.

Romano-British Land Surface.—Flint Flakes Replaced.

In the early spring of the present year, whilst passing a newly-opened excavation near Caddington Church, three miles south-east of Dunstable, I noticed a very thin horizontal line of sharp flint flakes, embedded a foot deep from the surface-line of an old pasture. I could see at once that the line represented an old living surface, so I took a few of the flints away. In removing the stones from the soil, one or two little fragments of Romano-British pottery came away with them. The flakes were lustrous,

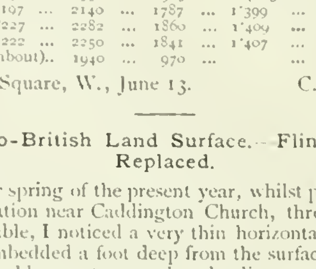


Fig. 1.—Fragment of perforated Romano-British pottery (half actual size).

chiefly black and brown-grey, and as sharp as when first struck. On looking over the flints in the evening, I was able to replace five on to each other. This fact, and the occurrence of the pottery fragments, proved the old surface to have remained intact from Romano-British times.

A little later in the spring, about six square yards of the superincumbent soil were carefully removed for me, when other flakes were found *in situ* to the exact number of four hundred; with these were eighteen fragments of Romano-British pottery, one piece—somewhat like the bottom of a pot—perforated, as here

Fig. 2. Four conjoined flint-flakes (half actual size).

illustrated. Amongst the flints were two cores, two hammer-stones, three scrapers, part of one edge of a chipped celt, and several neatly chipped but apparently unfinished little implements. A middle-brass Roman coin, too corroded for identification, was found on the same surface in a second excavation close by; with this was a small piece of wood carved to represent a horse's fore-leg, and a well-finished and perfect unpolished flint celt.

In sorting the flints I was able to replace thirty-eight on to each other in groups of from two to five. Two of these groups

are here illustrated—one a group of four, the other of two; the latter shows a straight-edged scraper above, conjoined to a simple flake below.

Hertfordshire conglomerate occurs as a surface stone at the same place, and I have at different times picked up very thin pieces without bulbs which appeared to me to have been artificially flaked. I have, however, kept no disputable objects. Hertfordshire "plum-pudding stone" was certainly flaked by the Kelts of this district, as is proved by the large, faceted and well-bulbed knife-like flake of conglomerate, found by myself at Caddington, here illustrated. This stone is not mentioned as one

FIG. 3. Straight edged scraper, conjoined to a flake (half actual size).

known to have been utilised for tools in the list given by Sir John Evans, in his "Stone Implements of Great Britain."

It is difficult to fix a date for the Romano-British living surface here mentioned, as the coin is too corroded for determination; but a correspondent, the Rev. Henry Cobbe, of Maulden, has a Roman coin, found in an adjoining field at Caddington, inscribed "C. CÆSAR AUG. GERMANICUS." If this inscription indicates the Emperor Caligula, as Mr. Cobbe believes, we have a date, A.D. 37–41, and the coin was probably brought over by one of the

FIG. 4. Large knife-like flake of Hertfordshire conglomerate (half actual size).

soldiers of Aulus Plautius under Claudius, in the second coming of the Romans in A.D. 43.

A short distance from the old land surface here described is an extensive Roman refuse pit with abundant broken pottery; so that it is safe to assume that a Roman villa once stood close by, and we seem to have evidence of the curious fact of a Kelt sitting down in close proximity to a Roman house and its refuse pit, quietly chipping his stone implements. It is equally curious that the implements and detached flakes have remained undisturbed so near the surface for nearly two thousand years.

Dunstable. WORTHINGTON G. SMITH.

The Bifilar Pendulum at the Royal Observatory, Edinburgh.

SOME interesting readings of the bifilar pendulum, designed by Mr. Horace Darwin for measuring movements in the earth's surface, were made here at noon on the 9th inst. This instrument, which indicates oscillations in a north and south direction, was erected in March of last year, and daily observation of it has since been carried on, the scale being read off each minute, from five minutes before to five minutes after Paris noon. On the 9th inst. nothing unusual was noticed during the first seven readings, these being all practically the same; but on putting my eye to the telescope for the eighth, I at once noticed that during the interval of less than a minute since the preceding reading, the mirror had rotated considerably about its vertical axis, the normal having moved towards the north, the difference between the seventh and eighth readings being no less than 7·6 mm. of the scale. An immediate examination of the lamp-stand showed it to be perfectly firm. After the regular daily readings were completed, others were made at intervals of generally two minutes, for half an hour after Paris noon. These showed two quite conspicuous oscillations of the mirror during its return to its original position, which it reached about thirteen minutes after noon. It continued to move beyond this point towards the south, till at 0h. 31m. Paris mean time it was 4·1 mm. south of the point at which the scale was first read off. Later readings in the course of the day showed that it was still moving slowly to the south, but no further oscillations were recorded. In the evening, when the mirror appeared to have come to rest, the sensitiveness of the instrument was tested, and with this the column headed "Tilt of mirror-frame" in the following table has been computed. The positive sign indicates a tilt to the north.

	Paris mean time.		Scale reading of ray from mirror.	Tilt of mirror-frame in preceding minute.
	h.	m.	mm.	
June 8 . .	23	55	284·2	″
		56	4·1	−0·005
		57	4·1	0·000
		58	4·0	−0·005
		59	4·0	0·000
June 9 . .	0	0	4·2	+0·010
		1	284·4	+0·010
		2	292·0	+0·385
		3	2·1	+0·005
		4	1·6	−0·025
	0	5	2·3	+0·035
	0	6	2·1	−0·010
		8	290·2	−0·096
		10	288·7	−0·076
		12	7·0	−0·086
		14	5·4	−0·081
		16	2·8	−0·132
		18	2·1	−0·035
		19	2·8	+0·035
		21	1·4	−0·071
		23	0·3	−0·056
		25	1·7	+0·071
		27	1·3	−0·020
	0	31	280·1	−0·061

THOMAS HEATH.

Royal Observatory, Calton Hill, Edinburgh, June 20.

Migration of a Water-beetle.

LAST night, at about ten o'clock, a beetle flew in through the open window, alighting on a bowl of roses in the centre of the dining table. On being dropped into a finger-bowl he promptly dived and swam merrily, and proved to be a specimen of the ordinary brown water-beetle, to be found in every pond or ditch of water. Now the nearest water to my dining-room window is the Thames, distant over a quarter of a mile as the crow flies, whence this water-beetle must have flown. Can any of your readers inform me whether such long flights have been observed before in connection with the pairing season or migration of this species? I enclose you a rough sketch of the beetle, not knowing its specific title amongst the Coleoptera.

ROSE HAIG THOMAS.

Basildon, Reading, June 23.

ARGON AND HELIUM IN METEORIC IRON.

IN the light of the new discoveries of argon and helium, it appeared that the reinvestigation of the gas evolved on heating meteoric iron might promise interesting results. This anticipation has been fulfilled. Meteoric iron, heated *in vacuo*, yields a small amount of both argon and helium, besides a comparatively large quantity of hydrogen.

The investigation of gases occluded in meteoric iron was undertaken by Graham in 1867 (*Proc. R. S.*, xv. 502). From 45·2 grams of a specimen of iron from Lenarto, in Hungary, Graham obtained, by heating it *in vacuo*, 16·53 c.c. of gas, consisting of 85·68 per cent. of hydrogen, 4·46 of carbonic oxide, and 9·86 per cent. of "nitrogen." And eight years later, Prof. Mallet investigated the gases from a specimen of meteoric iron from Augusta County, Virginia, and found 35·83 per cent. of hydrogen, 38·33 per cent. of carbonic oxide, 9·75 per cent. of carbonic anhydride, and 16·09 per cent. of "nitrogen." (*Proc. R. S.*, xx. 365.)

In the same year, Prof. A. W. Wright examined spectroscopically the gases evolved from two meteorites, one the "great Texas meteorite" in the museum of Yale College, which weighs 742 kilograms; another a specimen of meteoric iron from Tazewell County, Tennessee; and a third set of experiments was made with fragments of a meteorite from Arva, in Hungary. The gases obtained were examined spectroscopically, and were found to show the usual spectra of hydrogen, carbon compounds, oxygen, and nitrogen. He was searching for lines present in the spectra of stars, but found none; and he concludes that the spectrum of the solar corona is to be ascribed merely to atmospheric gases. A preliminary account of the examination of a fourth (a stony) meteorite is given in the same journal (*Amer. Journal of Science* [3], ix. pp. 294 and 459), and the full account in vol. x. 44. Suffice it to say that the last fractions of gas evolved contained 6·91 per cent. of "nitrogen." On p. 257 of the next volume (xi.), Prof. Wright gives analyses of the gases from various samples of meteorites, which contain from 1·54 to 5·38 per cent. of "nitrogen." And lastly, in vol. xii. p. 165, he gives further details, including descriptions of spectra, in none of which he noticed anything unusual.

Prof. Wright's interesting papers are instructive, inasmuch as they show how little reliance is to be placed on the evidence of the spectroscope as to the presence of any one gas in a gaseous mixture consisting of a large proportion of other gases. There is no doubt that in future, much attention should be paid to the relative conductivity of gases. The characteristic spectrum of argon is almost completely masked by the presence of a few parts per cent. of nitrogen or of hydrogen; and that of helium is similarly affected, although to a less degree. Though no quantitative experiments have been made on the subject, yet I should judge that the presence of from 5 to 10 per cent. of nitrogen entirely obscures the characteristic yellow line; the other strong lines still remain visible. I hope soon to be able to communicate further information on this interesting subject.

The presence of both argon and helium has been demonstrated in the meteorite from Augusta County, Virginia, a sample of which was purchased from Mr. Gregory. Two ounces of turnings of this meteorite were heated to bright redness in a hard glass tube, all air having been first removed in the cold by a Töpler's pump. From this iron, 45 c.c. of gas were obtained. It was mixed with oxygen in a gas burette, and exploded. It appeared to consist for the most part of hydrogen. After absorption of any carbon dioxide and the excess of oxygen with alkaline pyrogallate, the residue amounted to about half a cubic centimetre. It was transferred to a small tube and dried with a morsel of solid caustic potash, and with it several vacuum tubes were filled. The spectrum showed that it consisted for the most part of argon; the trace of nitrogen which appeared at first rapidly disappeared under the influence of the discharge.

The spectrum was carefully compared with that given by a tube of atmospheric argon, provided with magnesium electrodes. This sample of argon always shows the D lines of sodium, owing to the magnesium electrodes, and proves especially convenient for the detection of helium, the yellow line of which is not coincident with the lines of sodium. Both spectra were thrown into a two-prism spectroscope at the same time, and on careful comparison it was evident that all the argon lines were present. Besides these, the yellow line D_3 of helium was faintly visible, not coincident with the sodium lines; and on comparing the spectrum of the gas directly with that of helium from cleveite, it was possible to recognise the identity of the red, blue-green, blue, and violet lines of helium in the meteoric gas. No other lines were visible than those of argon and helium. It may thus be concluded, on spectroscopic evidence, that both argon and helium are contained in meteoric iron, the former in much larger amount than the latter. This conclusion was verified by mixing about 90 per cent. of argon with 10 per cent. of helium. The spectrum of helium, under these circumstances, was much more brilliant than that of argon; hence it may be concluded that less than 10 per cent. of this gaseous residue consisted of helium.

It appeared likely that metallic iron might be induced to absorb argon. It had been noticed, last October, that in attempting to prepare argon by passing atmospheric nitrogen through iron tubes filled with magnesium turnings, and heated to redness, a smaller quantity of argon than usual was collected. This rendered it not improbable that iron at a red heat is permeable to argon. If permeable, then it might be permanently absorbed. An experiment was therefore undertaken by Mr. Kellas, to whom I have to express my indebtedness, to ascertain whether finely divided iron, obtained by the reduction of ferric oxide in hydrogen, would occlude argon.

About 14 grams of the finely divided iron was placed in a combustion-tube, the capacity of which was 53·6 c.c. The tube was connected by a three-way stopcock to a Sprengel's pump and to a water-jacketed reservoir containing argon over mercury. After exhausting the tube, argon was allowed to enter, and the temperature was slowly raised to 600° and maintained for three hours. Until equilibrium of temperature had been established, no perceptible change of volume could be noted. The tube was allowed to cool, connection with the argon reservoir was closed, and the gas was pumped off. The volume, corrected for temperature and pressure, was 54·2 c.c. On heating the tube, about 59 c.c. of gas was given off; it was collected in three fractions, (*a*), (*b*), and (*c*), the heating having been continued for twelve hours.

(*a*) The volume of this gas was 30 c.c. It was collected at about 200° C. This was exploded with oxygen; and a residue was obtained, of which the greater part dissolved in caustic potash, showing that the gas had consisted of hydrogen and hydrocarbons. The final residue was 1·7 c.c.

(*b*) The second fraction, collected at 450°, amounted to 15 c.c., and after treatment as above, the residue was 0·25 c.c. This residue was united with that from (*a*), and a vacuum tube was filled. The flutings of carbon were visible, and also a trace of hydrogen, but no argon. This gas was sparked with 1 c.c. of oxygen, and on absorbing the excess of oxygen with alkaline pyrogallate, 0·45 c.c. remained. On transferring this residue to a vacuum tube, the banded spectrum of nitrogen was alone visible.

(*c*) The third fraction, collected at a red heat, also showed only the spectrum of nitrogen, when purified and transferred to a vacuum tube, and on continuing the discharge it also disappeared and the tube became

phosphorescent. Judging from previous experience, the presence of argon would have revealed itself after the nitrogen had disappeared. It may therefore be concluded that whether iron is permeable to argon at a red heat or not, it does not permanently retain the gas. It is not improbable that the condition of retention may be that the iron is heated to fusion in an atmosphere of hydrogen, hydrocarbons, argon, and helium, and that it is then suddenly cooled. This I should imagine to be the case if the iron were ejected from some stellar body at a high temperature. I am, however, unaware whether any of the lines of the argon spectrum have been identified in the spectra of stars; if not, it is probably because they are masked by the spectra of hydrogen and carbon.

W. RAMSAY.

SUBTERRANEAN FAUNAS.

THE researches of geologists and engineers have revealed the existence of vast tracts of underground waters, often associated with more or less extensive caves. The investigation of these underground waters is interesting to naturalists, as it has led to the discovery of a special subterranean fauna, different in different regions, it is true, but characterised throughout by modifications in certain definite directions. The study of these modifications is a fascinating one, and the problem of their evolution seems to be rendered comparatively easy by the simplicity and limitations of the conditions of life which obtain beneath the earth's surface; for these subterranean forms live in continual darkness, and are exposed to a fairly uniform temperature at all times. It is also, in many cases, possible to tell from what surface-species an underground form has descended, and to infer the age of the latter with a fair approach to accuracy; the nature of the changes undergone, and the rate at which these modifications have taken place, can thus be estimated in particular instances.

It will be remembered that in Packard's well-known memoir on the Cave Fauna of America, the peculiar modifications of subterranean animals were interpreted as lending strong support to the theory of the inheritance of acquired characters. Recently, however, in a careful and interesting memoir on the subterranean Crustacea of New Zealand (*Trans. Linn. Soc.*, London, vol. vi., 1894), Dr. Chilton considers the question from the Neo-Darwinian aspect; and he adduces a number of facts and arguments which greatly tend to reduce the force of Packard's contentions.

Dr. Chilton begins his memoir with a completed account of the New Zealand subterranean Crustacea, including a description of some new species. The underground crustacean fauna of New Zealand has a more varied aspect than that of Europe or North America; of the six species known, three are Amphipods and three Isopods, and these belong to as many as five different genera. Among them *Gammarus fragilis* is interesting to us as being allied to the blind *Niphargus* of Europe. *Cruregens fontanus*, an Isopod belonging to the family Anthuridæ, is curious in possessing only six pairs of legs; the seventh segment is small and without appendages, as is the case also in young Isopods; this larval character is retained in *Cruregens*, probably owing to an arrest in development on account of the scanty supply of food. Two subterranean species of the genus *Phreatoicus* are described, *P. typicus* and *P. assimilis*, n. sp.; a surface species, *P. australis*, lives on the top of Mount Kosciusko in Australia. This genus is peculiar, and the type of a new family of Isopods which approaches the Asellidæ in some respects, but differs in the possession of a laterally compressed body and a long six-jointed pleon.

In addition to the description of these underground forms, the writer gives a *résumé* of our only too scanty knowledge of the habits and conditions of life of subterranean animals. He discusses also the question of the origin of cave forms, and arrives at the conclusion that the New Zealand subterranean crustacea have clearly been derived from a surface fauna, though the affinities of one or two species seem to be rather with marine than with known fresh-water forms. It is pointed out, however, that the cave fauna is not necessarily descended from the present surface fauna of the country; *Crangonyx compactus*, for instance, has its nearest allies in Europe and North America, and the remarkable habitat of the fresh-water species of *Phreatoicus* has already been mentioned.

Cave crustaceans, according to Packard, live "in a sphere where there is little, if any, occasion for struggling for existence between these organisms." Chilton, however, suggests that there is evidence for thinking that Natural Selection has come into play in the evolution of cave animals. He points out that the scanty supply of food must inevitably lead to a keen struggle. Moreover, Packard himself states that the *Cæcidotea* and *Crangonyx* of the North American caves are eaten by the blind crayfish, which in its turn is devoured by the blind fish *Amblyopsis*; so that these animals must struggle with their destroyers. To this end have probably been developed the additional olfactory setæ, described by Packard and others, to enable the pursued animals to escape from their enemies. If there were no occasion for struggling for existence, why should these additional sense organs have been developed at all? At first sight, it certainly seems natural to attribute the degeneration of the eyes, observed in underground forms, to disuse; and it is but a further step to assume that these new characters, resulting from disuse and adaptation to new conditions of life, were inherited by successive generations. But Chilton ingeniously remarks that, if the modifications in the eyes of cave animals were the direct inherited effect of the environment, we should expect to find the lines of modification similar in all animals subjected to the same conditions. This, however, is not the case, as Packard's own investigations have shown. The influences leading to degeneration act uniformly on all individuals, but the modifications produced in the eyes are various, and occur in different ways. In some cases there is total atrophy of the optic lobes and optic nerves, with or without the persistence in part of the pigment (or retina) and the crystalline lens; in others the optic lobes and optic nerve persist, but there is total atrophy of the rods and cones, retina, and facets; while in extreme cases there is total atrophy of the optic lobes and nerves, and all the optic elements. These examples, showing a development apparently capricious and varying in direction in animals all subjected to the same or similar environment, point rather to the action of Natural Selection than to that of the direct inherited influence of the conditions of life.

In a more recent essay in the *American Naturalist* (September 1894), Packard has restated his views on the subject of the modifications of the eyes in subterranean animals, and concludes his remarks with the following words: "That while the heredity of acquired characters was, in the beginning, the general rule, as soon as the congenitally blind preponderated, the heredity of congenital characters became the normal state of things." In support of his view, Packard cites some statistics upon the inter-marriage of deaf-mutes, which have been recently furnished by Prof. Graham Bell. It would appear that, in America at any rate, the segregation of deaf-mutes within asylums has been followed by a striking increase in intermarriages among them; so that, of the deaf-mutes who marry at the present time, no less than 80 per cent. marry deaf-mutes. A marked increase in the number of the deaf-mute population has ensued, and

Prof. Bell points out the danger which consequently exists of the formation of a distinct deaf-mute variety of mankind.

All this is clearly brought out in Prof. Bell's memoir; but Mr. Packard goes so far as to state that Mr. Bell's statistics appear to "almost demonstrate the fact of the transmission of characters acquired during the life-time of the individual," and also says that "deaf-mutes already appear to breed true to their incipient strain or variety, whether congenitally deaf *or rendered so by disease during the life-time of either or both parents*." (The italics are ours.) We are thus left in no doubt as to Mr. Packard's interpretation of Mr. Bell's researches; but an attempt on our own part to find in Mr. Bell's pages the particular statistics or remarks which may be regarded as all but demonstrating the inheritance of acquired characters has, remarkably enough, been completely unsuccessful. Mr. Bell's conclusions lend no support to such a view. So far as they bear upon the present subject, they are briefly as follows: (1) That the great factor in determining the production of deaf-muteness in offspring is the existence of a hereditary taint in the direction of deaf-muteness in one or both branches of the family. (2) That this hereditary taint is not the less potent in its effects if it fails to manifest itself in the actual parents of the deaf-mute. (3) That "non-congenital deafness, if sporadic, is little likely to be inherited."

It would thus appear, both from Chilton's presentation of the facts, and from the failure of Packard's appeal to analogy, that—often as the contrary opinion has been urged—the peculiarities of cavernicolous animals do not lend any particular degree of support to the Lamarckian principles of evolution. W. G.

PROPOSED BALLOON VOYAGE TO THE POLE.

DURING the last century many expeditions to the North Pole have been undertaken, but with no result so far as reaching it is concerned. Baron Nordenskiold, the great Arctic explorer, has made four expeditions to Spitzbergen, and two to Nova Zemlia and Greenland, besides having taken part in the celebrated voyage of the *Vega*. In all explorations both he and others have found the icebergs the chief obstacle; and it may be said that Arctic explorers are now almost all unanimously convinced that the Pole can never be reached in steamer or sledge. Attempts on foot have likewise failed, for the distance of about ten miles has never been exceeded, owing to the great difficulties and dangers.

Notwithstanding these facts, Dr. Nansen, the celebrated Norwegian explorer, attempted yet another way, and instead of cutting a path through the ice, he has allowed himself to be carried polewards by a northerly current. This took place a year and eight months ago, and he has not been heard of since.

Quite recently, at the Royal Academy of Science, Stockholm, an even more perilous project was proposed by M. Andrée, a Swedish engineer. M. Andrée proposes making the expedition in a balloon. The project is not a new one, but it has never been seriously discussed by Arctic explorers. M. Andrée, however, has had much experience in polar regions, having spent the winter of 1882–83 in the far north, and also taken part in the Swedish Meteorological Expedition, which lasted a year. He has also proved himself to be a dauntless aeronaut, his most interesting voyage being one from Gothenburg to the Isle of Gothland, in which he had to cross part of the Baltic. Everything in connection with this proposed expedition has been minutely studied and discussed; and infinite pains have been taken to solve all difficulties.

The balloon would require a double outer covering, and a volume of 6500 cubic yards. The ascensional power thus obtained would be sufficiently great to carry three persons, furnished with provisions for four months, besides allowing for the car being fitted up with necessary instruments for observation, life-buoys, and Berton's collapsible boats. The car could be suspended in such a way as to allow of instant detachment in case of a descent into the sea. The entire weight of the balloon must not exceed about three tons. In the instance of Henri Giffard's captive balloon, exhibited in 1878, and which weighed about six tons, it only required newly inflating after a year's use. According to Graham's observations, a balloon measuring 8¾ yards in diameter can be made sufficiently air-tight so as to suffer, per month, merely a loss of 13½ lbs. of its ascensional force. M. Andrée, however, hopes to produce an absolutely impermeable covering.

The balloon, being protected from the wind by a wooden enclosure, would be inflated as far north as possible, by means of hydrogen compressed in cylinders. This once accomplished, it would begin to ascend. To a certain extent it might be steered by means of a sail and several guide-ropes, which, dragging on the earth, form as it were a brake. The ropes, however, would have to be of special composition, in order to produce the same effect in water. The hindrance thus caused to the flight of the balloon, together with the pressure of the wind, would allow the use of a sail. The flight then might reach an angle of 40° away from the wind direction. This steering apparatus, invented by M. Andrée, has often been used by him in his aerial voyages.

Besides the guide-ropes, heavy lines, on which would be placed numbered metal plates, would be attached to the car; these would serve as ballast. In case of a lowering of temperature, and a consequent descent of the balloon, it could be lightened by throwing off these plates, which, if found, would, to a certain extent, show the course taken by the explorers.

Spitzbergen has been chosen as the starting-point, for this island is almost always clear of ice by the middle of June. The departure would take place in July, on a clear day, with a southerly wind. At Spitzbergen the average rate of wind per second is 10¾ yards; the guide-ropes would cause a hindrance of about 2¾ yards per second, therefore the average rate of balloon would be nearly 8 yards per second, which is about 16 miles an hour. At this rate the Pole should be reached in 43 hours.

The summer is in all respects the most suitable time for an aeronautic voyage in Spitzbergen. The lowest temperature observed at Cape Thordsen in July, 1883, was $+0^{\circ}.8$ C., and the highest $+11^{\circ}.6$ C. The movements of the balloon would therefore be very regular. Besides this, there are practically no storms, and the fall of snow in June and July is both slight and rare.

M. Andrée's project has been highly approved of by the most experienced Arctic explorers. Baron Nordenskiöld has declared himself in favour of it, and M. Eikholm, chief of the Swedish Meteorological Expedition to Spitzbergen in 1882–83, states that the conditions of the Arctic regions are most favourable for this voyage. He thinks, moreover, that in the future the balloon will be the principal means of exploring that part of the world.

For many of the above details, we are indebted to an article in the *Revue Scientifique*, by M. Charles Rabot. W.

THOMAS HENRY HUXLEY.

WE regret to announce that, after an illness extending back to last March, and relieved only by two or three brief periods of improving health, Prof. Huxley passed peacefully into the silence of death on Saturday afternoon.

So long ago as 1874, a notice of the life and work of Prof. Huxley was included in our "Scientific Worthies" (vol. ix. p. 257), and Dr. Ernst Haeckel added to it an appreciative notice of his biological labours. These twenty-year-old publications render it unnecessary that any extensive reference to the subject-matter of them should be given now, and, moreover, the chief details of his life are well known.

Huxley was born at Ealing in 1825. His scientific training began in the medical school attached to Charing Cross Hospital, which he entered in 1842. Four years later he joined the medical service of the Royal Navy, and proceeded to Haslar Hospital; from there he was selected to occupy the post of Assistant-Surgeon to H.M.S. *Rattlesnake*, then about to proceed on a surveying voyage in the Southern Seas. The ship sailed from England in the winter of 1846, and returned to England in 1850, after surveying the inner route between the Barrier Reef and the East Coast of Australia and New Guinea. During this period, Huxley sent home several papers, some of which were published in the *Philosophical Transactions* of the Royal Society. His first important paper, "On the Anatomy and Affinities of the Medusæ," was published in 1849. His communications, and the evience of ability which they furnished, led to his election into the Royal Society in 1851.

In 1854, Huxley succeeded his friend Edward Forbes as Palæontologist and Lecturer on Natural History at the Royal School of Mines, a post which he held until his retirement in 1885. He was a great teacher, and the high reputation of the School, now combined with the Royal College of Science, is largely due to his great influence. At the request of the Lords of the Committee of Council on Education, he continued to act as Honorary Dean of the School, and at death he still retained that post. He also agreed to be responsible for the general direction of the biological instruction in the School, so that his place as Professor of Biology has never been filled up.

Huxley was twice chosen Fullerian Professor of Physiology to the Royal Institution, the first time in 1854. In the same year he was appointed Examiner in Physiology and Comparative Anatomy to the University of London. Other posts and honours were crowded upon him. In 1858 he delivered the Croonian Lecture of the Royal Society, when he chose for his subject the "Theory of the Vertebrate Skull." From 1863 to 1869 he held the post of Hunterian Professor at the Royal College of Surgeons. In 1862 he was President of the Biological Section at the Cambridge meeting of the British Association, and eight years later held the Presidency of the Association at the Liverpool meeting. In 1869 and 1870 he was President of the Geological and Ethnological Societies, and in 1872 was elected Lord Rector of Aberdeen University for three years. As might be expected, Prof. Huxley held strong and well-defined views on the subject of education. He was a man who at all times had a keen sense of public duty, and it was this which induced him to seek election on the first London School Board in 1870. Ill-health compelled him to retire from that post in 1872, but during his period of service as chairman of the Education Committee he did much to mould the scheme of education adopted in the Board Schools.

He was elected Secretary of the Royal Society in 1873, and ten years later was called to the highest honorary position which an English scientific man can fill, the presidency of that Society. During the absence of the late Prof. Sir Wyville Thomson with the *Challenger* Expedition, Huxley, in 1875 and 1876, took his place as Professor of Natural History in the University of Edinburgh. From 1881 to 1885 he acted as Inspector of Salmon Fisheries. But this and all his other official posts he resigned in 1885, shortly after which he removed to Eastbourne.

In 1892, more than six years after his retirement, the dignity of Privy Councillor was conferred upon him. The Copley Medal of the Royal Society was awarded to him in 1888, the Royal Medal having been received by him in 1852; and in December last he received the Darwin Medal, the two previous recipients being Dr. A. R. Wallace and Sir Joseph Hooker. His honorary degrees were:—D.C.L. (Oxford); LL.D. (Cambridge, Edinburgh, and Dublin); M.D. (Würzburg; Ph.D. (Breslau). The King of Sweden created him Knight of the Polar Star, and he was elected into most foreign Societies and Academies of Science of note. He was a Correspondant of the Paris Académie des Sciences (Section of Anatomy and Zoology), and Corresponding Member of the St. Petersburg Académie Impériale des Sciences, the Akademie der Wissenschaften, of Berlin and of Munich, the Svenska Vetenskaps-Akademie, Stockholm, the Halle Akademie der Naturforscher, the Academies of Natural Sciences of Philadelphia, Boston and Buffalo, the Göttingen Gessellschaft der Wissenschaften, the Paris Société d'Anthropologie, and the Naturforschende Gessellschaft at Frankfurt-a-M. He was Honorary Member of the Royal Irish Academy, the Accademia dei Lincei at Rome, the Brussels Académie de Médecine, the Institut Egyptien at Alexandria, the Batavia Genootschap van Kunsten en Wetenschappen, the American Academy of Arts and Sciences, National Academy of Sciences, and the Amsterdam Akademie van Wetenschappen. He was also Foreign Member of the Brussels Académie des Sciences, the Haarlem Maatschappij der Wetenschappen, the Philadelphia Academy of Natural Science, and the Società Italiana delle Scienze.

How far-seeing Huxley was, with regard to our present scientific needs, may be gathered from his address when he retired from the presidency of the Royal Society. He saw that scientific literature would have to be organised before it could be fully utilised. His words were: "We are in the case of Tarpeia, who opened the gates of the Roman citadel to the Sabines, and was crushed under the weight of the reward bestowed upon her. It has become impossible for any man to keep pace with the progress of the whole of any important branch of science. . . . It looks as if the scientific, like other revolutions, meant to devour its own children; as if the growth of science tended to overwhelm its votaries; as if the man of science of the future were condemned to diminish into a narrower and narrower specialist as time goes on. . . . It appears to me that the only defence against this tendency to the degeneration of scientific workers, lies in the organisation and extension of scientific education, in such a manner as to secure breadth of culture without superficiality; and on the other hand, depth and precision of knowledge without narrowness." Another point touched upon in the same address was the claims of science to a place in all systems of education. "We have a right," he said, "to claim that science shall be put upon the same footing as any other great subject of instruction, that it shall have an equal share in the schools, an equal share in the recognised qualification for degrees, and in University honours and rewards. It must be recognised that science, as intellectual discipline, is at least as important as literature, and that the scientific student must no longer be handicapped by a linguistic (I will not call it literary) burden, the equivalent of which is not imposed upon his classical compeer." To the expression of such views as these we owe the increased attention now given to scientific instruction in this country, though we have not yet reached the impartial stage to which science has a right.

It may, perhaps, be too early to fix Huxley's real place in Biology. Writing in these columns in 1874, the eminent German naturalist, Haeckel, ranked him among

the first zoologists in England, taking zoology in its widest and fullest signification. "When we consider," he remarked, "the long series of distinguished memoirs with which, during the last quarter of a century, Prof. Huxley has enriched zoological literature, we find that in each of the larger divisions of the animal kingdom we are indebted to him for important discoveries." From the lowest animals he gradually extended his investigation to the highest. In the Protozoa, he was the first to come to satisfactory conclusions concerning the nature of Thalassicollidæ and Sphærozoida; and by his work on "Oceanic Hydrozoa," he greatly extended the knowledge of Zoophytes. His researches upon members of the important group of Tunicata are of great value, and many important advances in the morphology of the Mollusca and Arthropoda are due to him. Further, Huxley especially studied and advanced the knowledge of the comparative anatomy and classification of the Vertebrata. His "Lectures on the Elements of Comparative Anatomy," and his numerous monographs on living and extinct species, afford abundant evidence of what biological science owes to him.

Huxley's place as one who has largely influenced modern thought on many questions, is acknowledged by all to be a very high one. The profound and truly philosophical conceptions which guided him in his inquiries, always enabled him to distinguish the essential from the unessential. First among the subjects which owe their advancement to his support is the theory of biological evolution. When, in 1860, it became his duty as Professor at the Royal School of Mines to give a course of lectures to working men in the Jermyn Street Museum of Practical Geology, he selected for his subject "The Relation of Man to the Lower Animals." The questions arising out of this topic became the subject of warm controversy at the meeting of the British Association in that and subsequent years. The lectures were published in 1863, under the title "Evidence as to Man's Place in Nature," and excited great interest both in this country and abroad. In this and in other works he advanced the principles of the Darwinian theory, and worked out many important developments.

To again quote Haeckel: "Not only has the Evolution Theory received from Prof. Huxley a complete demonstration of its immense importance, not only has it been largely advanced by his valuable comparative researches, but its spread among the general public has been largely due to his well-known popular writings. In these he has accomplished the difficult task of rendering more fully and clearly intelligible to an educated public of very various ranks, the highest problems of philosophic biology. From the lowest to the highest organisms, he has elucidated the connecting law of development. In these several ways he has rendered science a service which must ever rank as one of the highest of his many and great scientific merits."

As a writer of English, Huxley has been unsurpassed in our time and generation. He has set a standard in scientific literature, both in clearness of exposition and in the most perfect handling of words, which it behoves his successors to closely follow. He aimed at writing clearly, and avoided the use of technical language whenever possible. As he remarks in the preface to the volume of "Collected Essays" containing his biological and geological addresses: "I have not been one of those fortunate persons who are able to regard a popular lecture as a mere *hors d'œuvre*, unworthy of being ranked among the serious efforts of a philosopher; and who keep their fame as scientific hierophants unsullied by attempts—at least, of the successful sort—to be understanded of the people. On the contrary, I found that the task of putting the truths learned in the field, the laboratory and the museum, into language which, without bating a jot of scientific accuracy shall be generally intelligible, taxed such scientific and literary abilities as I possessed to the uttermost; indeed, my experience has furnished me with no better corrective of the tendency to scholastic pedantry which besets all those who are absorbed in pursuits remote from the common ways of men, and become habituated to think and speak in the technical dialect of their own little world, as if there were no other."

This Journal especially loses in him one of its best friends. We are now in the second series of fifty volumes, and his was the hand that commenced both of them. His introduction to the fifty-first volume will be fresh in the minds of our readers, and it justified the position he had occupied since 1859, as the devoted apostle of the Darwinian theory. He was, moreover, not only a most valued contributor to our columns, but his advice on many points has been freely asked, given, and followed, during a quarter of a century.

Huxley's wonderful kindness to young men is very well known. He would discuss subjects with his students, and his perfect geniality put them entirely at their ease. Always ready to extend a helping hand, he assisted many to higher ranges than they could otherwise have attained, and by words of encouragement induced others to continue their ascent.

The objects which Huxley stated he had in mind from the commencement of his scientific career are these:—

"To promote the increase of natural knowledge and to forward the application of scientific methods of investigation to all the problems of life to the best of my ability, in the conviction which has grown with my growth and strengthened with my strength that there is no alleviation for the sufferings of mankind except veracity of thought and of action, and the resolute facing of the world as it is when the garment of make-believe by which pious hands have hidden its uglier features is stripped off. It is with this intent that I have subordinated any reasonable, or unreasonable, ambition for scientific fame, which I may have permitted myself to entertain, to other ends; to the popularisation of science; to the development and organisation of scientific education; to the endless series of battles and skirmishes over evolution; and to untiring opposition to that ecclesiastical spirit, that clericalism, which in England, as everywhere else, and to whatever denomination it may belong, is the deadly enemy of science. In striving for the attainment of these objects, I have been but one among many, and I shall be well content to be remembered, or even not remembered, as such."

How nobly he acted up to his principles we all know; how greatly the pursuit of his objects have benefited intellectual and material progress, we can only estimate.

In the preface of the fifth volume of his "Collected Essays," Huxley gives a quotation from Strauss's "Der alte und der neue Glaube," which describes so exactly the guiding principles of his life, that it is difficult to believe the lines were written by another hand nearly a quarter of a century ago. "For close upon forty years," wrote Strauss, "I have been writing with one purpose; from time to time I have fought for that which seemed to me the truth, perhaps still more, against that which I have thought error; and in this way I have reached, indeed overstepped, the threshold of old age. There every earnest man has to listen to the voice within: 'Give an account of thy stewardship, for thou mayest be no longer steward.' That I have been an unjust steward, my conscience does not bear witness. At times blundering, at times negligent, Heaven knows; but, on the whole, I have done that which I felt able and called upon to do; and I have done it without looking to the right or to the left; seeking no man's favour, fearing no man's disfavour."

Huxley leaves a wife and seven children—three sons and four daughters. They mourn the loss of a loving husband and father, and their affliction is shared by all who were fortunate enough to know him as a friend. But his loss will not only be felt by these; it affects the whole

intellectual world. Men will arise who, like him, will advance and extend scientific knowledge by research and exposition, but rarely will the qualities of the investigator and interpreter be combined with a more charming personality.

The funeral has been fixed to take place at Marylebone Cemetery this afternoon, at 2.30 o'clock.

NOTES.

AMONG the honours which Lord Rosebery recommended on leaving office, and which the Queen has approved, we notice that Dr. Robert Giffen, C.B., whose work in various departments of statistical science will be known to our readers, has become K.C.B., and that Prof. J. W. Judd has been appointed C.B. Mr. James Blyth, the well-known agriculturist, has received a baronetcy, Colonel V. D. Majendie, C.B., has been promoted to K.C.B., and Captain Lugard has been appointed C.B.

THE International Meteorological Committee, at its last meeting at Upsala, in August 1894, recommended that an International Conference of the same character as that of Munich in 1891, should be held at Paris about the middle of September, probably September 15, 1896. A circular has just been distributed among meteorologists, announcing that M. Mascart has undertaken to make the arrangements necessary for the meetings of the conference. Mr. R. H. Scott will be glad to receive, at the Meteorological Office, notes on any questions suitable for insertion in the programme for the conference. It is proposed that the definitive programme shall be prepared before the end of the present year 1895, in order to give meteorologists interested in the subjects proposed for discussion, time to formulate their views thereon.

THE death is announced of Prof. D. Kirkwood, for many years Professor of Mathematics in Indiana State University, and known for his investigations of the orbits of planets and comets.

AN influential committee has been formed in Paris, to collect funds for the erection of a monument to Francis Garnier, the explorer. The Treasurer of the Committee is M. J. Rueff, 43 rue Taitbout, Paris.

PROF. FUCHS has been elected a Correspondant of the Paris Academy of Sciences, in the Section of Geometry; Dr. Nansen has been elected a Correspondant of the Section of Geography and Navigation, and Dr. Laveran a Correspondant of the Section of Medicine and Surgery.

PROF. WILD has formally announced the resignation of his office at St. Petersburg as from September 13. His future residence will be at Zurich, and he requests that papers and books hitherto addressed to him at St. Petersburg, should be sent to his new address.

THE subject of the essays for the Howard Medal of the Royal Statistical Society, to be awarded in 1896, with £20 as heretofore, is "School Hygiene, in its Mental, Moral, and Physical Aspects." Essays should be sent in on or before June 30, 1896.

PROF. C. LLOYD MORGAN has accepted an invitation to deliver four lectures in the Columbia University Biological Course next winter. His subject will be "Some Habits and Instincts of Birds." Mr. Frank M. Chapman, of the American Museum of Natural History, will also give four lectures upon birds, from the zoologist's standpoint.

THE American Museum Expedition of 1895 has already completed the exploration of the Uinta basin fossil fauna, and established the fact that, like the *Phosphorites* of France, it is completely transitional between the Eocene and Miocene. The party is now passing north to explore Brown's Park on the eastern base of the Uinta Mountains, an ancient lake basin which has been long known but hitherto unexplored for fossils.

THE Executive of the Midland Union of Naturalists at their annual meeting, held on Monday last at Oxford, awarded to Mr. Walter E. Collinge, Assistant-Lecturer in Zoology and Comparative Anatomy, Mason College, Birmingham, the "Darwin Medal" for his recent researches on the cranial nerves and sensory canal system of fishes.

MR. GEORGE S. DAVIS, who, since January 1885, has at a very heavy loss maintained the "Index Medicus," announces he will be obliged to discontinue that very useful publication, owing to insufficient support. It would hardly be to the credit of medical societies, and scientific workers generally, if this indispensable monthly index is allowed to come to an end for want of something like £400 a year.

THE fortieth annual exhibition of the Royal Photographic Society will be inaugurated on Saturday, September 28, by a private view, followed in the evening by a conversazione. The exhibition will remain open daily (Sundays excepted) from September 30 until November 14. Medals will be placed at the disposal of judges for the artistic, scientific, and technical excellence of photographs, lantern slides, and transparencies, and for apparatus. The judges for the technical section are Captain W. de W. Abney, Mr. Chapman Jones, and Mr. Andrew Pringle.

AN International Exhibition of Hygiene, organised under the direction of M. Brouardel, was opened at Paris on Thursday last. The exhibits are divided into five groups, referring respectively to (1) the hygiene of private houses; (2) city hygiene; (3) the prophylactics of zymotic diseases, demography, sanitary statistics, &c.; (4) the hygiene of childhood, including alimentary hygiene, questions of clothing, and physical exercises; (5) industrial and professional hygiene.

THE *Weekly Weather Report* of the 29th ult. shows that the rainfall for the first half of this year is much below the average in all districts except the north-east of England. The deficiency varies from 2·5 inches in the east of Scotland, to 5 inches in the south-west of England, but in the west of Scotland the deficiency amounts to 12 inches. Some heavy amounts have, however, been measured recently; at Churchstoke, Montgomery, the abnormally large fall of 4·83 inches was recorded on the 26th ult.

A FEW days ago, the Lord Mayor of Liverpool, on behalf of the Museum Committee of the Corporation (of which Sir William B. Forwood is chairman), opened in the Public Museum, in presence of a numerous assembly, a large new gallery exclusively devoted to ethnography. An interesting account of the origin and history of the collection, and of the method of its arrangement, was given by Dr. H. O. Forbes, the Director of Museums. The African, Papuan, and New Zealand sections are especially rich, while those of Mexico, Peru, and Patagonia contain some very rare exhibits of exceptional value.

AT the annual general meeting of the Society of Arts, the following gentlemen were elected Vice-Presidents:—Sir Edward Birkbeck, Mr. B. Francis Cobb, the Hon Sir Charles W. Fremantle, Sir Douglas Galton, and Prof. W. C. Roberts-Austen. To fill the places vacated by retiring members of Council, there were elected, at the same meeting, Sir Steuart Colvin Bayley, Major-General Sir Owen Tudor Burne, Mr. R. Brudenell Carter, and Dr. Francis Elgar. Sir Frederick Bramwell was elected Treasurer of the Society.

The following recent appointments are announced in *Science*. To be assistant professors in Johns Hopkins University: Dr. C. Lane Poor, astronomy; Dr. A. S. Chessin, mathematics and mechanics; Dr. Simon Flexner, pathology: Dr. Albert Mann to be professor of biology in Ohio Wesleyan University. In Syracuse University, Dr. E. C. Quereau to be professor of geology and mineralogy, and Dr. W. H. Metzler associate professor of mathematics. Mr. M. A. Mackenzie has been appointed professor of mathematics in Trinity University, Toronto. The chair of physics in the University of California, recently filled by the late Prof. Harold Whiting, has been offered to Dr. E. P. Lewis.

A novel engineering scheme in the construction of the foundation of the retaining wall of the new speedway at High Bridge, in New York City, is the freezing of a bed of quicksand which impeded the work. A row of 4-inch pipes have been sunk a few feet apart, to the depth of 40 feet. These pipes are capped at the bottom, and inside them are inserted smaller pipes, open at the bottom. Cold air is forced from a condenser through the smaller pipes into the larger, and thence returned to the condenser. The air is cooled by expansion to a temperature of about − 45 C., thus freezing the surrounding mud and wet sand, and checking the flow into the excavation.

Those who have read Prof. Crum Brown's "Robert Boyle" Lecture, reported in our columns (vol. lii. p. 184), will be interested to learn that among the "Studies from the Princeton Laboratory," contributed to the current number of the *Psychological Review*, there is a paper on "Sensations of Rotation," by Mr. H. C. Warren. The particular object of this investigation was to determine the relative influence of sight and the internal sense of rotation on the subjective estimate of movement. By means of a mirror—which could be inserted or removed at will—the apparent motion, as given to sight, could be reversed. For the detailed results the paper itself must be consulted. In general they seem, we are told, to favour the view that the semicircular canals constitute the organ for the sense of rotation.

The Meteorological Office has received from the Central Physical Observatory of St. Petersburg, copies of a circular addressed to various institutions with reference to a proposed meteorological exhibit at Nizhny-Novgorod Exhibition in 1896. The Central Physical Observatory being desirous of making this exhibit as complete as possible, and at the same time of making known to the Russian public the progress of meteorological science in various countries, desires to obtain information on any of the following points: (1) Number of stations, of different orders. (2) Titles of periodical publications, any of which will be exhibited. (3) Summary of practical applications of meteorology, with titles of any works on the subject. (4) Copies of works containing mean values or references to them, instructions for taking observations, descriptions of instruments with methods of exposure, and charts referring to maritime meteorology.

The autumn meeting of the Iron and Steel Institute will be held at Birmingham from Tuesday to Friday, August 20-23. The programme will embrace visits to the leading industrial establishments in and around Birmingham. The Mayor of Birmingham will hold a reception, at the City Council House and Art Gallery, on the evening of August 20. The Earl and Countess of Warwick will also give a reception at Warwick Castle. Among the papers that are expected to be read are:—"The Thermo-chemistry of the Bessemer Process," by Prof. W. N. Hartley, F.R.S.; "The Hardening of Steel," by H. M. Howe; "The Mineral Resources of South Staffordshire," by H. W. Hughes; "On Tests of Cast Iron," by W. J. Keep and by T. D. West; "The Estimation of Oxide of Iron in Steel," by A. E. Tucker; "The Use of Nickel in the Metallurgy of Iron," by H. A. Wiggin.

Prof. Kikuchi, of the Science College, Tōkyō, is preparing a short life of the late Prof. Cayley, to be accompanied by a photograph, for a Japanese popular scientific monthly, viz. the "Tōyō Gakugu Zasshi."

A remarkable system of electric lights on buoys has just been completed at the Gedney Channel, off Sandy Hook. This channel is only 1000 feet wide, and vessels have not heretofore been able to pass through it by night. The new system, however, provides a brilliant thoroughfare, lighted by ten incandescent lights of 100 candle-power each, and each on a buoy, about 50 feet long, and rising 12 feet out of water. The cable which conveys the electricity carries the pressure of 1000 volts under water, and is six and half miles long, being the longest cable in the world carrying a high-pressure current under water, and also the only one of its kind ever made. It consists of a copper conductor, insulated with gutta-percha, bedded in jute, and sheathed with hard drawn copper wire. The machines have an output of only 100 volts, but the current flows through a step-up converter, back of the switchboard, where it is converted into the required voltage, thus being perfectly safe to operate.

The palæontological department of the American Museum has recently secured by purchase the entire collection of fossil mammals of North America brought together by Prof. E. D. Cope since 1870. This includes 552 of Prof. Cope's mammalian types, besides the unique single skeletons of Phenacodus, Hyracotherium and Hyrachyas, and the rich series from all formations described and figured in Cope's Tertiary Vertebrata, besides all his unpublished material. This famous collection, together with the others which are rapidly coming in from the annual western expeditions to the Rocky Mountain region, will be arranged in the large new hall upon the geological floor of the Museum, which has been designed and cased for the purpose. The collections are being prepared for exhibition and research as rapidly as possible.

By the kindness of Mr. R. H. Scott, we are able to print the following information received at the Meteorological Office with reference to some recent earthquake disturbances in the Leeward Islands. The note was drawn up by Mr. F. Watts, the Government Analytical Chemist at Antigua, and was sent to the Colonial Office with two letters on the effects of the earthquake in Barbuda. "On Monday, May 20, 1895, a long and somewhat severe earthquake shock was felt in Antigua at 4.44 p.m. This shock threw down a steel rod 4 inches long and $\frac{1}{8}$ inch in diameter, in a rough earthquake indicator at Skerretts. Slight shocks followed at intervals. I was able to ascertain that there were at least seven shocks between 4.44 and 8.20 p.m. A shock at 6.58 p.m. was rather severe, causing one of the Cathedral bells to sound slightly and stopping the clock. Slight shocks have been experienced almost every day since. Similar shocks are reported from Montserrat, Nevis, St. Kitts, and Barbuda. Some injury to buildings is reported from Barbuda, but I am unaware of the extent of the damage. It is stated that distant sounds, as of explosions, were heard in Barbuda; these appear to have been heard in a northerly direction. Through the courtesy of the Telegraph Company, I am informed that these earthquakes have not been felt in any islands save those in the groups Antigua, Montserrat, Nevis, St. Kitts, Barbuda. From this fact, coupled with the report of noises heard in Barbuda, I should infer that these disturbances are purely local, and in no way related to the earthquakes in Europe about the same time."

An elaborate investigation on the bacterial contents of margarine and margarine-products has been recently made by Messrs. Jolles and Winkler. It is satisfactory to find, in view of the large quantities of margarine which are placed on the

market in one form or another, that it is considerably freer from microbes than ordinary butter when the latter is not made with Pasteurised cream. Whereas butter contains an average of from 10 to 20 millions of microbes per grm., margarine-butter yields but from 4 to 6 millions; moreover, whilst in extreme cases as many as 47 millions of microbes have been found per grm. in butter, margarine can only boast of at most something over 11 millions. Cold appears to act more prejudicially on margarine microbes than it does on butter germs; thus in one case a reduction from $6\frac{1}{2}$ millions to 230,200 per grm. was observed in margarine, whilst a similar exposure never succeeds in eliminating more than one-third of those present in butter, according to Lafar. It is reassuring to learn that in none of the numerous samples examined were pathogenic bacteria discovered; many of the ordinary microbes present were isolated and described, and amongst these two were found which the authors believe are closely associated with rancid processes which occur in old samples of margarine. To further reduce the microbial population of margarine butter, it is suggested that only sterile milk and sterile water should be used in its manufacture from oleo-margarine, which is considerably poorer in bacterial life than the finished product.

IN the years 1891 and 1892, the Norwegian Government fitted out a vessel for the purpose of making temperature observations round the Lofoden Islands, with the view of tracing the connection between the habits of the cod and the temperature of the water, and the Parliament voted a sum of money for the purchase of thermometers for registering the temperature at various depths. We have received from Lieutenant G. Gade, who was entrusted with the investigation, a pamphlet entitled "Temperaturmaalinger i Lofoten," which contains an interesting account of the results obtained. He found that at the same places the temperature sometimes increased regularly according to the depth, while at others there were two distinct strata of water, the cold being uppermost. Although the vertical variations of temperature may have been considerable, yet he always found an increase with depth. The favourite temperature of the cod is supposed to be 5° C., and while in January 1892 this was found at the surface, in March 1891 it was only found at a depth of 160 metres; the greatest depth at which fishing takes place is 200 metres, where 6°–7° C. were recorded nearly constantly from January to the middle of April. Lieutenant Gade found that when there were two strata of water, one cold (2°–3° C.) uppermost, and one warmer (5°–7° C.) below, the cod was always found in the warmer stratum; but, as the fishing takes place at depths where the temperature is from 4°–7° C. or more (and the depths where these temperatures are found are very different), the author considers that the fisherman cannot derive practical advantage from temperature observations alone.

CHORISIS, or the doubling of the parts, is by no means a rare occurrence in flowers. In this phenomenon there appear, apparently in the place of one floral leaf, especially a stamen, two such leaves either collaterally, *i.e.* beside one another, or serially, above one another. These pairs of leaves may arise either out of a single common primordium, or directly from the axis. Up to the publication of a paper on "Das Reductionsgesetz der Blüthen," by Dr. Lad. J. Čelakovský (Stzb. der königl. böhmischen Ges. der Wissenschaften), morphologists agreed in regarding chorisis as the division or branching of an originally simple leaf. Dr. Čelakovský, however, comes to the following conclusions, amongst others, after a very complete consideration of a large number of instances. Normal chorisis occurs not as a division of a single leaf, but rather as a fusion, or at least an approximation of distinct and originally uniformly separated leaves. In the ontogeny of the plant this may occur as a branching or positive chorisis, as he terms it, of a single primordium, but this fact does not afford, according to him, a clue to the steps in the phylogenetic development, by which the present state has come about; but he believes, in opposition to the hitherto received opinion, that the present condition in these flowers was attained by negative chorisis or approximation. Normal chorisis is the expression of an incomplete transition from a state in which the individual leaves composing a whorl or whorls were more numerous, into one in which they are less numerous. It is, in fact, the resultant of two tendencies—one, the older, to polymerism, and the other and newer, to oligomerism. The reduction so effected is always governed by the law of the alternation of the consecutive leaf-whorls. Dr. Čelakovský's paper is one of great interest, and the discussion as to the origin of the various types of andrœcium will no doubt be specially useful to those who are interested in the affinities of the natural orders of dicotyledons and monocotyledons.

THE publishers of *Knowledge* announce that Dr. Isaac Roberts, F.R.S., will shortly continue in that magazine his selection of photographs of stars, star-clusters, and nebulæ. The series is intended to be in continuation of Dr. Roberts's work, "A Selection of Photographs of Stars, Star-Clusters, and Nebulæ," recently published, and which has contributed very largely to the extension of the knowledge of astronomical phenomena.

THE July number of *Natural Science* is devoted to brief descriptions of the results of the *Challenger* Expedition, from the points of view of investigations in many branches of knowledge. Each of the contributors, all of whom write with authority upon their respective subjects, more or less confines himself to answering the question, "How has the *Challenger* Expedition advanced science?" The brief summaries thus obtained form a very valuable and compact index to the advances in various fields of natural knowledge due to the Expedition.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*, ♀) from India, presented by Mr. John Norbury, Junr.; a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. H. W. Ball; a Black-eared Marmoset (*Hapale pencillata*) from South-east Brazil, presented by Mr. H. P. Roberts; a Rough Fox (*Canis rudis*) from British Guiana, presented by Dr. Irvine K. Reid; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Lady Champion de Crespigny; seven Black Salamanders (*Salamandra atra*), a Slowworm (*Anguis fragilis*) from Switzerland, presented by the Rev J. W. Horsley; a Burchell's Zebra (*Equus burchelli*, ♂) from South Africa, a Common Rhea (*Rhea americana*) from South America, deposited; two Black-necked Swans (*Cygnus nigricollis*) from Antarctic America, three Blue Snow Geese (*Chen cærulescens*) from Alaska, purchased; a Thar (*Capra jemlaica*, ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

VARIABLE STARS.—Dr. Chandler has completed a revised supplement to his well-known second catalogue of variable stars; together they furnish a complete list of known variables, comprising in all 344 stars. Some little difficulty has been experienced in connection with the southern variables, on account of the want of accurate positions and certain identifications in some cases. Dr. Chandler especially shows a want of confidence in the data relating to the variables discovered photographically at the Boyden station of the Harvard College Observatory at Arequipa, ; but considering the pressing need of a definitive nomenclature, and relying on the assurances of Prof. Pickering, most of these objects have been included in the catalogue and letters assigned to them.

All the recent observations made by the South African ob-

server, Mr. Roberts, have also been included in the new catalogue. (*Astronomical Journal*, No. 347.)

The Temperature of the Sun.—A new method of determining the temperature of the sun has been employed by H. Ebert (*Astrophysical Journal*, June). With the aid of data supplied by Langley's investigations, Rubens deduced the law that the wave-length of the maximum energy is inversely proportional to the square root of the absolute temperature of the radiating body. Experiments on the radiation of blackened bodies between absolute temperatures 373° and 1088° indicated the relation

$$\lambda\sqrt{T} = 123,$$

T being the absolute temperature, and λ being expressed in microns ($\mu = \cdot 001$ mm.). Langley has shown that the maximum energy of the continuous background of the solar spectrum is very nearly at 0·6 μ, and assuming that the incandescent particles in the sun which yield the continuous spectrum are comparable to a black body as regards their total radiating capacity, the application of the above formula gives a temperature of about 40,000° C. The parts of the sun to which this temperature applies are stated to belong to the interior regions, below the photosphere.

Dr. Ebert enters into a discussion of the electromagnetic nature of the solar radiation, in order to justify the application of the formula in the case of the sun. This leads him incidentally to suppose that the continuous background of the solar spectrum is mainly due to hydrogen in a strongly compressed state.

The Rotation of Saturn.—In 1893 Mr. Stanley Williams announced some highly interesting facts with reference to the period of rotation of Saturn, as deduced from observations of spots on different parts of the surface of the planet (Nature, vol. l. p. 32). The observations were continued during the opposition of 1894, and similar striking results have been arrived at. (*Monthly Notices*, vol. lv. p. 354). It was again found that the spots indicated widely different rotation periods in the same latitude, but in different longitudes, as shown in the following table:—

	Range in longitude.	Mean period. h.	m.	s.
Dark spots (17°–37° N.)	30–130 … … .	10	14	57·29
	140–200 … …		14	44·23
	240–360 … . ..		15	47·97
Bright spots (6° S.–6° N.)	0–80 … … . .		13	1·69
	80–160 … …		12	40·03
	160–360 … … .	10	12	25·83

The average rotation periods of the whole equatorial spot zone during the four years of observation were as follows:—

	h.	m.	s.		s.
1891 . . …	10	14	21·8	Diff.	43·6
1892 ..		13	38·2	,,	45·8
1893 .. …		12	52·4	,,	16·6
1894 … ..	10	12	35·8		

The extreme difference of 1m. 46s. observed since 1891 "means a very considerable increase in the velocity of motion of the surface material, amounting to 66 miles per hour. In other words, the great equatorial atmospheric current of Saturn was flowing 66 miles an hour more quickly in 1894 than it was in 1891."

Taken as a whole, the observations indicate a more rapid rotation of the planet in the equatorial regions than in the northern zone of spots, and they appear to establish that there are great differences of velocity in different longitudes.

To Prof. Darwin, these results "suggest a rather wild consideration" (*Observatory*, June). He considers it possible that sections of the planet parallel to the equator may not be circular, and suggests that it might be worth trying to detect systematic differences between the various equatorial diameters by micrometric measurements.

THE VISIBILITY OF SHIPS' LIGHTS.

It may be remembered that in 1890, the German Marine Observatory tested some three thousand running lights in use on board ships, and found two-thirds of them defective. Further tests of the visibility of lights of known candle-power were made by the German Committee last year, and some of the results obtained are noted in a leaflet just distributed to seamen by the U.S. Weather Bureau. The law of emission for a white light is that its visibility is proportional to the square root of its candle-power, and the results of the experiments by the Committee closely follow the law, the departures being no greater than the estimated errors of position of the vessel. The mean of a large number of observations gave as the distance at which a white light of one candle-power became visible 1·40 miles for a dark clear night, and 1·00 mile for a rainy one. Experiments undertaken in America, after the International Maritime Congress in 1889, gave the following results in very clear weather: A light of 1 candle-power was plainly visible at 1 nautical mile, and one of 3 candle-power at 2 miles. A 10 candle-power light was visible with an ordinary binocular at 4 miles; one of 29 candles faintly at 5, and one of 33 candles visible without difficulty at the same distance. On a second evening, exceptionally clear, a white light of 3·2 candle-power could readily be distinguished at 3, one of 5·6 at 4, and one of 17·2 at 5 miles. The Dutch governmental experiments, conducted at Amsterdam, gave the following results: A light of 1 candle-power was visible at 1 nautical mile; 3·5 at 2, and 16 at 5 miles. Experiments with green lights gave 0·80 as the distance in miles at which a green light of a single candle-power is just visible. The candle-power required for a green light to be visible at 1, 2, 3, and 4 nautical miles was 2, 15, 51, and 106, respectively. The American experiments before referred to give for green light: 3·2 candle-power fairly visible at 1 mile, and 28·5 clearly at 2 miles, these results being, however, from a limited number of experiments. The German trials were much more numerous. The extraordinarily rapid diminution of the visibility of the green light with the distance, even in good observing weather, and the still more rapid decrease in rainy weather of a character which will but slightly diminish the intensity of a white light, show that it is of the utmost importance to select for the glass a shade of colour which will interfere with the intensity of the light as little as possible. The shade recommended is a clear blue-green. Yellow-green and grass-green should not be employed, as they become indistinguishable from white at a very short distance. For the red, a considerably wider range is allowable, but a coppery-red is said to be the best.

THE RELATIVE POWERS OF LARGE AND SMALL TELESCOPES IN SHOWING PLANETARY DETAIL.

It is to be hoped that a definite understanding will soon be arrived at regarding the differences between large and small telescopes in revealing delicate surface-markings on Mars, Jupiter, and Saturn. The subject of relative efficiency was discussed about ten years ago, and some interesting evidence was evoked as to the different forms and sizes of telescopes, but no settlement of the question was possible in the face of the diversity of opinion existing. The time seems to have come when the subject may be suitably referred to, and the facts considered apart from mere prejudice or preference for any kind or size of instrument.

The phenomenal results recently claimed for certain small telescopes are almost of a character to shake even the faith of those disposed to acknowledge their great utility on several classes of objects, for our confidence cannot go beyond reasonable limits. In individual cases a good though small instrument, an acute well-trained eye, acting in combination with the best atmospheric conditions, will yield surprising results; but some of those lately published border upon romance, and henceforth it would seem that if all the data derived with such means are to be absolutely accepted, then large telescopes are grossly incapable on certain important objects, and may as well be packed away in the lumber rooms of our observatories.

This is the more surprising when we consider the opinions expressed during the discussion which previously took place on the same subject. Prof. C. A. Young, who has charge of the 23-inch refractor at Princeton, said: "I can almost always see with the 23-inch everything I see with the 9½-inch under the same atmospheric conditions, and see it better—if the seeing is bad only a little better, if good immensely better." Other observers having the means of comparing large and small instruments, side by side, furnished similar evidence, except in the case of M. Wolf, of Paris, who said: "I have observed a great deal with two instruments (both reflectors) of 15·7 and 47·2 inches aperture. I have rarely found any advantage in using the larger one when the object was sufficiently luminous." Prof. Asaph Hall, whose

valuable work with the 25·8-inch refractor at Washington is so well known, once said: "The large telescope does not show enough detail." The testimony was not, therefore, unanimously in favour of big telescopes.

More recently the 36-inch at Mount Hamilton has been eulogised for its fine performance. Mr. Keeler, in January 1888, said that the minutest details of Saturn's surface were visible with wonderful distinctness with this instrument. The 12-inch and 6-inch refractors at the same observatory were found far inferior in capacity to the 36-inch. Prof. Barnard has also stated: "Let the conditions be the best for observing, with the air steady, and the 36-inch is far ahead of the 12-inch." The same observer has also remarked: "350 is the most useful power on Jupiter and Mars, 520 on Saturn." For planetary work he prefers using the full aperture and low powers.

We have it on the authority of most of those who have employed both large and small telescopes, and are therefore in the best position to speak as to their relative merits, that large instruments in good air will reveal more than small ones. The observer would in preference use the largest instrument for any critical purpose: and this being so, how shall we explain their apparent failure in regard to planetary details? Is it that the big telescopes show too little, or that the small instruments exhibit too much?

And here it may be noted that only in exceptional cases do we find phenomenal results accruing from the use of small apertures. It is not every one who has a telescope of 6 or 8 inches diameter who can discover the various spots and numerous belts on Saturn, or trace the double and often interlacing canals of Mars.

During the last few years numerous dark and light spots have been detected on the ball of Saturn by Mr. A. S. Williams, who used a 6-inch reflector. These have been distinguished when Saturn was nearing conjunction with the sun, and in spite of two unfavourable circumstances—namely, the small diameter of the planet, and its proximity to the horizon. The spots have been seen so distinctly, that the observer has been enabled to describe them individually as bright or faint, small or large, round or oval, &c. These observations have not, perhaps, been fully corroborated, though several observers appear to have glimpsed the markings alluded to. When we consider that many hundreds of amateurs have been employing their telescopes upon Saturn without seeing the spots, the affirmative evidence of a few isolated persons can hardly be regarded as conclusive. It is a fact that, if any new feature on a planet, or an unknown companion to a star were confidently announced, a few of the many observers who looked for it would certainly assert they could see it though not really existing.

Prof. Hough, with the 18½-inch refractor, at Chicago, made a series of observations in 1884 and 1885 for the special purpose of detecting definite markings on Saturn and redetermining the rotation period, but he quite failed to get the necessary data. His statement was: "The belts on the disc of the planet were at times quite conspicuous and very sharply defined, but we were unable to find any spot or marking by which to observe rotation." Yet the *Monthly Notices* for June 1884 contain a drawing which gives a numerous array of condensations attached to the dark narrow belt bounding the equator on its southern side. This drawing was made with an 8¼-inch reflector, and at about the same period many other observers examined the planet with an entirely negative result as far as the existence of these condensations was concerned. A drawing was published in the *Journal* of the British Astronomical Association for July 1894, showing the planet as he appeared on March 26 of that year in a 12-inch reflector. A numerous assemblage of dark belts are shown, and many other observers appear to have seen several comparatively narrow belts. Prof. Barnard, however, using the 36-inch refractor in re-measuring the dimensions of Saturn and his rings in 1894, was led to pay some attention to the physical appearance of the planet, and significantly remarks: "But one dark narrow belt was seen upon the planet. The black and white spots recently reported with small telescopes were not seen at any time." It is certainly a remarkable circumstance that the belts and spots, if really existing, cannot be seen in the large instrument. Are the observers with small apertures suffering from some extraordinary hallucination, or must we consider that the brightness of the image in large telescopes and inferior definition are sufficient to obliterate very delicate markings? Is the glare sufficiently strong to overcome the slight contrasts of tone readily perceptible on a fainter image? Prof. Holden thus expressed himself in 1891: "There is no doubt that the belts on Saturn are often marked and mottled with brighter spots. I presume that such spots would be as easily seen in a small but perfect telescope as in a larger one. Seeing such faint markings is entirely a matter of detecting faint contrasts, and these should be detected as readily in a small instrument as in ours, if not more readily, except that the large size of our image helps us." On the other hand, Prof. Young has suggested that faint images are very encouraging to the imagination, and therefore often a source of observational errors.

Prof. Holden's remarks are tantamount to an admission that large instruments are ineffective on planetary details, for what are delicate markings but "faint contrasts"? Yet it would be conceived that the 36-inch had proved itself quite capable of dealing with such contrasts, for it is stated by Prof. Barnard, from observations of Jupiter in September-October 1894: "The red spot is fairly distinct in outline, though quite pale—a feeble red. The following end of the spot is quite dark. There are white regions on its surface. The belt south of it seems to be in contact with the spot, if it does not actually overlap it slightly."

The 36-inch is mounted in one of the finest localities for celestial observations, but shows nothing on Saturn but the dark narrow belt situated in the midst of the equatorial zone, while certain telescopes of small aperture reveal the disc furrowed with belts and mottled with spots. Nearly every small telescope shows more than one belt upon Saturn, but the delineations seldom agree as to the number or latitudes of these belts. We ought to expect approximately accordant positions; but the majority of drawings are hurriedly executed and based on rough estimations, so that they are often found inconsistent. The differences referred to are not, therefore, proof of the non-existence of the objects depicted, for the same disagreements are found with reference to well-assured formations. In some cases undoubtedly observers will, perhaps unconsciously, use their imaginations, as the desire is always to put in as much detail as possible. When mere fancy assists the optical powers, the resulting drawings are often very pretty and attractive from the number and novelty of the features shown. We can fill in any number of dark belts and bright zones, beaded with spots of various forms and tints, and tone the whole to suit our ideas; but unfortunately such drawings, though pleasing to the eye, have a bad influence, since they pervert the truth, and lack that fidelity to nature which could, alone, make them really valuable.

Mr. Williams, the discoverer of the Saturnian spots, has made some hundreds of observations of them, and fully detailed his methods and his results in the *Monthly Notices* of the R.A.S., liv. p. 297, *et seq.* First detecting them in the spring of 1891, he has now followed them during five oppositions of Saturn. The bright equatorial spots apparently show a period of rotation decreasing with the time, for the mean period during 1891 was 10h. 14m. 22s., while in 1892 it decreased 44 seconds, in 1893 43 seconds, and in 1894 15 seconds. The care with which Mr. Williams proceeded in his work, and the plan he adopted to avoid bias or preconceived ideas, are explained in the paper alluded to, and every one reading his description must be favourably impressed with it. *If his results are fully confirmed*, they will deserve to be ranked among the best observational feats of modern times. To have been the first to discover these delicate objects in all their variety, to have traced out their individual motions with unwearying persistency year by year, and to have employed all the time a very small telescope, must be regarded as a remarkable attainment. It is to be hoped that the necessary corroboration will soon be forthcoming.

I have myself practically endeavoured to afford this, but failed. The spots on Saturn are certainly not visible under powers of 252 and 312 on my 10-inch reflector. The power of 252 is the eye-lens of a Huyghenian eyepiece, that of 312 is one of the "monocentric micrometer oculars" of ¼-inch equivalent focus by Steinheil of Munich. The latter has a distinct advantage over my Huyghenian eyepieces. I have sometimes used a Barlow lens in combination with it, increasing the power to about 450, but do not think any advantage has been gained. I have occasionally had impressions of white spots mottling the bright equatorial zone of Saturn, and occasionally also of faint condensations in the dark belts; but as to seeing these details outright, and obtaining their times of transit with all the certainty of a definite spot on Jupiter, I have quite failed. I am induced to believe, from a number of observations dedicated to

the purpose, that my suspicions of spots were entirely illusory, and that such markings as objective features were invisible to my eye with the means employed. On the worst nights I could easily imagine a mottled aspect of the belts; but with good definition and a steady image, the tone of the belts and bright equator appeared perfectly even and free from noticeable irregularities. In a case of this kind the observer has to be severe with himself. There is a distinct line of demarcation between what is absolutely seen and what is possibly seen or suspected. An object may be only glimpsed, and yet it is certainly seen, for its impressions reach the eye now and then in a form not to be mistaken. But with some objects the experience is different. We fancy they are there, but cannot fix them with certainty; apparently they flit about like an *ignis fatuus*, and are intractable to our utmost efforts. Obviously in such a case the observer has but one alternative, and that is to regard the objects as imaginary.

On Mars, as well as Saturn, small instruments have done wonders. It is well known that the canals and their duplication were discovered by Schiaparelli with a refractor of only 8¼ inches aperture. In 1892, during a favourable presentation of Mars, the large American telescopes showed very little either of the canals or of their duplication. During the opposition of 1894 the planet was better placed as regards altitude (but not so near to the earth as in 1892), and the results of observations have been more satisfactory. Mr. Williams with a 6½-inch reflector, and Mr. Brenner with a 7-inch refractor, have recovered many of the double canals of Schiaparelli. Mr. P. Lowell, with the 18-inch refractor at the observatory at Arizona, has also observed many remarkable and intricate details of the planet's topography. This observer remarks that in regard to the visible markings on the inner planets of the solar system up to and including Mars, size of instrument is quite secondary to quality of atmosphere. He draws the "oases" on Mars, and a large number of interlacing lines on the planet, in *Popular Astronomy* for April 1895, and the pictures are very effective. There are many of us who would like to obtain a view of Mars similar to what he has depicted. Mr. Lowell notes that with the 18-inch a power of 420 was as high as the atmosphere permitted to be used with advantage, though drawings were generally made with 370. On the 6-inch refractor 270 showed well, the dark and light markings being more contrasted than in the larger instrument. As affecting the comparative utility of large and small telescopes, Mr. Lowell remarks: "A large instrument is assumed to be necessarily superior to a small one, quite irrespective of what it is that is to be observed. Now the fact is that there are two quite different classes of celestial phenomena—those dependent on quantity of light, and those dependent on quality of definition for their visibility, and the two means to these ends go anything but hand in hand. For the one, the illumination, the size of the instrument is the prime requisite; for the other, the definition, the atmosphere is the first essential. As an object-lesson in this, it is worth noticing that the biggest instruments have not always given the best views of Mars. In matters of Martian detail it is amply evident from the results that observer, atmosphere, instrument, is the order of weight to be given as the factors of an observation."

I have referred to this subject without any desire to take up the cudgels on behalf of any class of instrument, but it is suggestive that the large ones will not bear powers commensurate with their size on planetary details. Thus with the 36-inch at Mount Hamilton a power of 350 has been found the most effective on Mars: a similar power can be used with advantage on glasses of only 8 or 10 inches diameter. It is difficult to understand, therefore, where the superiority of large instruments comes in, as the object is sufficiently bright in small telescopes, and the latter being more easily manipulated and less affected by atmospheric tremors, they obviously possess some distinct advantages. But this interesting and important question is scarcely to be settled by a mere discussion of this sort. It is only to be settled by careful trials of large and small instruments, side by side, upon the planets Mars, Jupiter, and Saturn. If observers having the appliances at command will institute some further comparisons of the kind suggested, the problem might be virtually solved in a short time. Relying upon evidence of fragmentary character is scarcely fair, as the differences of eyesight and atmosphere come into play most prominently. The most valuable evidence would be that of an observer who used a number of telescopes of different apertures at one and the same station. Up to the present time it must be confessed that small instruments have somewhat the best of the argument; but if the unanimous testimony of our most trustworthy observers asserted the superiority of large telescopes on bright planets, it is hard to see how they could be disproved, as they alone have the effective means of judging the question on its merits.

W. F. DENNING.

SUBJECTIVE VISUAL SENSATIONS.[1]

THE activity of the cerebral centres which is independent of their common exciting causes, and which is termed "discharge," presents indications of the character and loss of their function which can be obtained from no other source. Foremost in interest and also in importance are the sensations of sight which occur without stimulation of the retina. Of these the most important are two. (1) Those which occur at the onset of epileptic fits, from the "discharge" in the brain influencing consciousness, through the visual centre, before loss takes place. (2) Those which occur as the precursory symptoms of the paroxysmal headaches which, from their one-sided distribution, have been called "hemicrania," "megraine" or "megrim," from the frequent vomiting, "sick headaches," and, from the inhibitory loss of sight, "blind headaches." These two classes form the subject of the lecture.

In what part of the brain does the process occur? The impulses from the retina reach the cortex of the brain first in the extremity of the occipital lobe, where, as Munk first showed, the half-fields are represented in strictly local definiteness. The left occipital lobe receives the impulses from the left half of each retina, produced by the rays of light from the right half of each field of vision. So, conversely, with the right occipital lobe. To each side, impulses proceed from a very minute area around the central point of the retina, the fixation point of the field. But we cannot conceive that the functional disturbance occurs in these centres, for the strict medial division in two halves is absolutely ignored by the subjective sensations. Moreover, the strange but certain facts of hysterical hemianæsthesia, in which there is inhibition of all the sensory centres of one hemisphere, present us with remarkable evidence of the higher visual function in each hemisphere. This is supported by some cases of organic disease, which cause an affection of sight similar to that of hysteria, and by more common cases of hemianopia from disease of the hemisphere, in which there is a precisely similar contraction of the remaining half-fields. The significance of all these is that the early conclusions of Ferrier are correct, and that, in addition to the lower, occipital half-vision centre, there is a higher centre in each hemisphere, situated in the region of the angular convolution. This theory of the double visual centres, consisting of a combination of the conclusions of Ferrier and Munk, was first stated by the lecturer in 1885, and has been confirmed by all the facts he has since met with. It is indispensable for the comprehension of morbid functional action, and, indeed, for that of normal vision, but is not yet recognised by physiologists, even as hypothetical.

The character of the function of this centre, so far as it can be discerned from the facts of its loss, are of great importance for the study of visual sensations. The two higher centres seem to be blended into one in function in a manner that is unique so far as our knowledge extends. If the centre on one side is functionless, there is loss of sight in the periphery of both visual fields; there is vision in the central third of the eye on the same side, and a far smaller central area on the opposite side. The only conclusion is the startling inference that either higher centre can subserve central vision in both eyes, but that peripheral vision depends on the co-operation of the function of both hemispheres. Between the central area for which either centre suffices and the peripheral area for which neither is competent but both are needed, there is an intermediate zone in which vision is subserved only by the opposite hemisphere when acting alone. This gradation of functional capacity enables some facts of subjective sensations to be comprehended which cannot otherwise be understood.

Moreover, the facts suggest that the function of these higher centres is quite different from that of the lower ones, and from that of other cerebral centres the action of which we can study. In the lower half-vision centres function is localised, so that destruction of part causes absolute loss of a part of the half-field, blindness of the corresponding part of the retina. But partial

[1] The Bowman Lecture, delivered before the Ophthalmological Society, by Dr. W. R. Gowers, F.R.S., June 13.

damage to the higher centre seems to lower the function of the whole, as if the function were diffused, and all its elements were represented, in varying degrees, in every part. This conception is so unfamiliar that it may seem inconceivable, and yet it harmonises with many of the facts of subjective sensations. Moreover, in a large part of the brain, local loss of tissue has only the effect of lowering function as a whole. It seems to be only where the sensory impulses reach the cortex, and motor impulses leave it, that the local distribution of function is definite, and limited damage has definite and lasting results.

The spectra perceived before epileptic fits vary widely. They may be stars or sparks, spherical luminous bodies, or mere flashes of light, white or coloured, still or in movement. Often they are more elaborate, distinct visions of faces, persons, objects, places. They may be combined with sensations from the other special senses, as with hearing and smell. In one case a warning, constant for years, began with thumping in the chest ascending to the head, where it became a beating sound. Then two lights appeared, advancing nearer with a pulsating motion. Suddenly these disappeared and were replaced by the figure of an old woman in a red cloak, always the same, who offered the patient something that had the smell of Tonquin beans, and then he lost consciousness. Such warnings may be called psycho-visual sensations. The psychical element may be very strong, as in one woman whose fits were preceded by a sudden distinct vision of London in ruins, the river Thames emptied to receive the rubbish, and she the only survivor of the inhabitants.

The colours seen are chiefly described as red, green, blue and yellow. A yellowish red-like flame is very common. In some cases red changes to green, a curious complementary relation, when we consider that the sensation is due to a primary process in the centre. One obtrusive fact, in these spectra and in those of migraine, is the frequency with which colours extend to the edge of the field of vision. In one case, each fit was preceded by the appearance of a green colour occupying the lower half of the field so completely that the patient said he seemed to be in a field of grass. It is often said that the periphery of the retina is not sensitive to colour, and that red and green are seen only in the centre. But long ago, Chodin and Landolt showed that colour vision extends to the periphery. The peripheral colour spectra led the lecturer to make a careful examination of the peripheral colour vision, especially in regard to area, to which it seems to be related in a greater degree than to illumination.

Red can be seen up to the margin of the field for white, an area in 6 cm. square; green cannot well be discerned within 5° of the margin, but yellow and blue can be seen up to the margin with 4 cm. square. The fields for each area from ·25 to 4 cm. square are concentric with the field for white.

One fact was ascertained which illustrates the mutual influence of the two visual centres. When both eyes are open the two fields correspond, except in the outer temporal third of each field. The nasal half of left field, for instance, extends to 55° of the outer horizontal radius of the right field, the end of which is at 90°. When both eyes are open, not only is the perception of colour intensified in the part where the two fields overlap, but the intensification goes on to the periphery, through the part in which there is no more retinal stimulation than when the right eye alone is open. Thus, in this radius, red is seen in 2 cm. square at 62° with right eye alone, but at 74° if the left eye is also open, although the left field does not extend beyond 55°. The colour is seen in 4 cm. square at 77° with the right eye only, and at the margin of the field only with 6 cm. square, but with both eyes open the 4 cm. square enables the colour to be seen up to the margin, instead of at 77°. There is thus greater sensitiveness in the centres to colour impulses proceeding from the peripheral region, where the field is single, if light from the other eye intensifies their action—a striking instance of their intimate co-operation.

The motor relations of the epileptic spectrum are instructive but too complex for brief description. It is common, in one-sided fits, for an object to appear at the edge of the field of vision on the side afterwards convulsed, and pass across, to disappear at the opposite side. Its appearance, *e.g.*, on the left is followed by movement of the head towards it, by the motor centres of the right hemisphere, but the head then follows the movement of the spectrum, by the action of the centres of the other hemisphere (sometimes with a conscious sense of irresistible compulsion), and then finally deviates strongly in the first direction, as the convulsion comes on, usually with loss of consciousness. A sense of vertigo may accompany the deviation. The eyes move before the head, and may be absolutely fixed when the head can be moved by the will. These phenomena throw instructive light on the relations of objective and subjective vertigo. Inhibition frequently precedes the epileptic spectra, but is always general, never partial, and neither the loss nor the spectrum is ever on one side only. If they appear on one side, it is only to move across the field, apparently as the result of the effect on the visual discharge of the associated motor nature of the epileptic process.

The visual sensations which precede the paroxysmal headaches of migraine differ very much from the warnings of epilepsy. Their general character is limited, but their forms are extremely varied. One has been well made known by the careful study of his own sensations by Dr. Hubert Airey, published in the *Philosophical Transactions* for 1870, reproduced by Dr. Liveing in his classical work on megrim. (Unpublished drawings by Dr. Airey, and several other series of drawings were exhibited. One curious set was made by a mechanical draughtsman who, from sixty to sixty-five years of age, frequently experienced visual sensations, similar to those of migraine, as isolated symptoms, without headache, and always depicted them as objective things, related to his own figure.) In this class of spectra, inhibitory loss of sight is almost invariable, but it is always partial, never general as in epilepsy, and it bears a definite relation to the spectrum. The phenomena are generally on one side, but occasionally medial, although never central, and they never correspond to one half of the field.[1] Even loss strictly limited to the medial line, as in hemianopia due to organic disease, is practically unknown, contrary to the common impression. The special feature of the "discharge" is an angled line of light, the "zigzag" spectrum, single or repeated, sometimes in many, as it were reflected, fading, lines. In round or oval form it has been termed the "fortification spectrum," from resemblance to the plan of a fortress devised by Vauban. The angled line may be of simple bright light or may present colours, red, green, blue, orange, which sometimes alternate in successive segments. It often seems made up of a multitude of minute brilliant points in rapid movement. When a single bright line, it may be banded on each side by a very narrow black line. This feature may be observed in the "phosphene" produced by pressure on the eye, even in the dark, when it is apparently due to a limiting line of loss of the "essential light of the retina," but its presence in a central spectrum raises the question whether this so-called "light of the retina" is not of purely central origin.

The central region is remarkably indisposed to discharge, but prone to inhibition. A medical practitioner, a careful observer, experienced first a spot of central dimness of sight, which enlarged, becoming darker in the centre and ultimately extended from top to bottom of the field, occupying the middle third, banded on each side by a double curve. Sometimes, when the spot had reached half-way to the top and bottom of the field, a bright zigzag line appeared on one side, which extended upwards and downwards, as the inhibitory loss increased, became brighter, but seemed to restrain the inhibition, which extended no further on that side, but was, as it were, reflected back and reached almost the extreme edge of the field on the other side. This illustrates the occurrence of the discharge secondary to inhibition, and limiting it. It is an instance of the way in which all half-field relations are absent in these phenomena. The common commencement is for an angled sphere, or stellate spectrum, to appear in the middle zone of one half of the field, and, expanding, form an oval within which vision is partially or completely lost. The edge is often coloured. The angles are especially developed towards the outer side of the field. Towards the centre of the field the expansion is less, the angles smaller, and the spectrum breaks. Sometimes one limb passes downwards, and the other towards the central point, but in the latter the angles gradually cease, and the spectrum never reaches the centre—an illustration of the resistance of the central region to discharge. In other cases, however, the ends of the broken oval may pass into the other half of the field, one on each side of the central point, enclosing this between them. When they reach the middle zone on the other side, a second star, like that from which the spectrum originated, may suddenly appear for a short time as a terminal feature. These characteristics show how remarkable must be the relation of the centres in which their cause occurs.

[1] By "field" is meant the area included by the boundary of the conjoined fields of both eyes, to which alone the central phenomena seem related.

An angled spectrum of curved course may also develop by progression through the middle zone, beginning below, and attaining its chief development in the upper half of that side, passing only a little way beyond the middle line above. In one case this was preceded by a transient angled star near the point of commencement, and its early stage was accompanied by inhibitory loss at the margin of the field, outside the region in which the discharge commenced.

Although discharge never occurs at the central point, it may occur around it, as a circular zigzag, surrounding a round object looked at—an instructive example of the fact that the discharge may be related to the central effect of actual retinal stimulation. Analogous to this "pericentral" spectrum, is one that takes the form of an arch above the central region, which may separate into two parts at the middle line. As an instance of the strong tendency there is to regard the spectrum as an objective thing, a member of the medical profession, when asked to draw that which he saw, sent a drawing of his eye surmounted by an angled corona. These forms again indicate disturbance in centres in which there is no half-field representation. Besides other forms, an angled spectrum sometimes appears near the outer temporal edge of the field, and extends outwards for a short distance and then curves downwards, never upwards. Such a peripheral spectrum always seems to the subject to begin at the extreme edge of the field and extend outside it. In one case it was drawn as attached to the junction of the upper and lower eyelids.

It cannot be doubted that, by the study of these subjective symptoms, much will ultimately be learned regarding the function and mode of action of the cerebral visual centres. Whatever the drawbacks to observation through the consciousness of another person, knowledge can be gained in no other way of the action of the higher centres of the brain, and the time must come when the physiological knowledge which can be gained only through the effects of disease and the disturbance of functional derangement, will receive more attention. The facts of these spectra, when studied in their detail, compel the conclusion that they occur in centres in which function is related to the conjoint fields, and in these to a central and a peripheral region and to a medial zone between the two: that the chief relations are central and peripheral; that outside the central region there is a one-sided relation, but that there is no distribution of function at all corresponding to the division of the fields at the medial line. The dominant relation is concentric, and the indications afforded by the absolute one-sided loss caused by destruction of one occipital lobe, has no reflection, positive or negative, in these results of spontaneous central activity.

HIGH-LEVEL METEOROLOGICAL STATIONS.[1]

ONE of the greatest drawbacks to a full understanding of meteorological phenomena is that the observations on which we base our knowledge are generally made close to the ground in the most restricted air-stratum; whereas the general atmospheric movements, both in velocity and direction, are much modified in the lower strata, and the air surrounding and in contact with the earth differs greatly both in temperature and humidity from the free air. The more strongly agitated upper strata react on the lower in many ways, and a knowledge of the movement of the moderately high atmospheric layers is of great importance for the theory of the general circulation of the atmosphere, and practically for our weather forecasts, since the forces which develop storms have their origin and sphere of action within two or three miles of the earth.

If the atmosphere were only in complete equilibrium, then the few irregular observations, as regards time and place, which have been made in balloons, would give some data on which to base general laws; but, in the actual condition of continual movements and changes in the atmosphere, this can never suffice, and the continuous observations required of all the elements, at all seasons and in all weathers, can only be made on mountains, even though the conditions there only approximate to those of the free air. In this way observations on mountains complete those of the usual low-level stations.

When the earth's surface rises in plateaux, the advantage of elevation above the sea—that is to say, the immersion in the upper strata—is almost entirely neutralised, because still our instruments are placed in air masses which are affected by contact with the earth. For this reason meteorological observatories should be located on high and isolated peaks. The erection of such stations and the discussion of their observations during the last fifteen years have contributed largely to the rapid progress of the science of meteorology.

The chief first order stations (those possessing self-recording instruments, or where observations are made on an extensive scale) which are located on mountain tops in the various countries, will now be briefly described.

The first summit station in the world was that established in 1870, jointly by the U.S. Signal Service and Prof. J. H. Huntington, on Mount Washington, N.H., 6280 ft. above the sea. Probably nowhere else in the world has such severe weather been experienced, the lowest temperature being here often accompanied by the highest winds, unlike the calms which prevail with intense cold at low levels. For instance, in February 1886, with a temperature of 50 degrees below zero, a wind velocity of 184 miles an hour was recorded on Mount Washington. The Government meteorological station on Pike's Peak, at an elevation of 14,134 ft., was for many years the highest in the world. Now both these stations are closed, so that there seem to be actually in the United States but two summit stations where meteorological observations are made throughout the year, viz.: The Lick Observatory, on Mount Hamilton, California—primarily astronomical—and the Blue Hill Meteorological Observatory in Massachusetts, situated at a very moderate elevation. Prof. S. P. Langley's important researches on the nature and amount of solar heat received by the earth were carried on in 1881 upon Mount Whitney, the summit of which is 14,500 ft. above the sea.

It is due to an American institution that the highest meteorological station in the world is now in Peru, where the Harvard College Observatory, several years ago, established an outpost at Arequipa. In 1893, Prof. Bailey succeeded in placing self-recording instruments on the summit of the neighbouring volcano of El Misti, 19,300 ft. high, when a former station on the side of Mount Chachani, near the snow-line, at an elevation of 16,650 feet, was abandoned. It is impossible for persons to remain at these stations, so they were provided with automatic instruments which should give a continuous record of the chief meteorological elements during two weeks. Several times a month one of the Observatory staff climbs the mountain in order to wind the clocks and change the register sheets, at the same time making a check reading of standard instruments. Breaks in the record occur, owing to unforeseen stoppage of the instruments, or inability to make the ascent at the appointed time.

France stands unrivalled in her superb chain of summit stations on the Puy de Dôme (4800 ft.) in Auvergne, on the Pic du Midi (9440 ft.) in the Pyrenees, on the Mont Ventoux (6250 ft.) in Provence, and on the Aigoual (5150 ft.) in the Cevennes, whose construction has cost the national and provincial Governments hundreds of thousands of dollars and years of time. They are generally defective in having no co-operating base stations, and their observations have not been published in detail. In 1890, M. Vallot, a devoted Alpinist and meteorologist, established several stations on and near Mont Blanc, from which records have been obtained each summer since. The highest of these stations, at the Rochers des Bosses, 14,320 ft., is provided with many self-recording instruments operating two weeks without attention, which are looked after by the owner or his guides each week or two during the summer. The Observatory of M. Janssen, sunk in the snow on the very top of Mount Blanc, 1460 ft. higher, is not yet in operation, but a meteorograph has been made for it in Paris, which will continuously record all the meteorological elements during a period of three months without attention. A similar instrument is being constructed at Blue Hill, by Mr. Fergusson, for Prof. Pickering's station on El Misti.

On the Eiffel Tower in Paris are instruments 980 ft. above the ground, which give more nearly the conditions prevailing in the free air than do any others permanently at this elevation. They record at the Central Meteorological Office, a quarter of a mile distant, side by side with similar instruments exposed near the ground.

Among the German and Austrian stations, that on the Sonnblick, a peak of the Austrian Alps, 10,170 ft. high, and the highest permanently occupied observatory in Europe, stands pre-eminent, having furnished very valuable results under Dr. Hann's direction.

[1] Abstract of a paper by Mr. A. Lawrence Rotch, read before the Boston Scientific Society.

Switzerland, which since 1873 had maintained stations in mountain passes, &c., has now on the Säntis (8200 ft.) in the canton of Appenzell, one of the best located and equipped summit stations in the world; and in Italy an observatory on Monte Cimone (7100 ft.) in the Apennines, near Lucca, has recently been completed.

On Ben Nevis, the highest mountain in Great Britain (4400 ft.), there is a remarkable station where during ten years an unbroken series of hourly observations has been maintained. There is a base station at sea-level, and the advantageous situation on the west coast of Scotland renders the results of the observations, which have been discussed by Dr. Buchan, of great value.

It is impossible to even enumerate all that has been gained from these high-level observations, but the chief results attained, or still sought, may be thus summarised: Determination of normal decrease of temperature and humidity with elevation; abnormal changes with elevation in cyclones (or areas of low pressure near the ground) and in anti-cyclones (or areas of high pressure near the ground); height to which these cyclones and anti-cyclones persist, and the circulation of the air around each at various levels.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Oxford.—At the Encaenia, or Commemoration of Oxford Founders, held on June 26, the honorary degree of D.C.L. was conferred upon Sir W. H. Flower, Prof. Michael Foster, M. Edward Naville, the distinguished Swiss Egyptologist, and Sir A. W. Franks, President of the Society of Antiquaries.

Sir J. E. Gorst has succeeded Mr. Acland as Vice-President of the Council for Education.

Mr. Herbert Hancock, Mathematical and Physics master in Bancroft's School, Woodford, London, has just been appointed to the headmastership of the Hipperholme Grammar School, an important science centre for the North of England.

At a Convocation of Durham University on Tuesday, June 25, the Sub-Warden announced that the new Charter had been received by which power is given to the University to confer degrees upon women in all faculties except Divinity. Among a large number of degrees conferred was that of Bachelor of Science on Miss Ella Mary Bryant, Durham College of Science, Newcastle.

In consequence of the shortly ensuing General Election, the annual meeting of the National Association for the Promotion of Technical and Secondary Education, and the Conference of representatives of Technical Education Committees, which had been arranged to take place in London on July 11, have been postponed.

On Thursday last a very successful and numerously attended conversazione was held at University College, London. The guests were received on the grand staircase by the President (Sir John Erichsen, Bart., F.R.S.) and Deans of Faculties. The various scientific departments of the College were thrown open, and many interesting exhibits contributed to the success of the evening. Among the latter were included the spectra of argon and helium, various electrical and physical experiments, living seaweeds and marine animals, new models of dividing nuclei, &c.

The University of London has conferred the degree of Doctor of Science, without examination, on Mr. Th. Groome, Professor of Natural History at the Royal Agricultural College, Cirencester, in recognition of the merits of his original researches and published papers.

The Berlin correspondent of the *Lancet* writes as follows:—"The publication of a rumour that the authorities intend to abolish the University of Jena, has caused a stir in the scientific world, the university being one of the oldest in Germany, and having often occupied a leading position. Financial reasons are said to have induced the authorities to arrive at this decision. The constitution of the University of Jena is somewhat peculiar. It is not under the jurisdiction of a single State, but belongs jointly to four States of Thuringia, viz., Saxe-Weimar, Meiningen, Coburg, and Altenburg. The Governments of those small States entirely control the affairs of the university. If, for instance, a new professor is to be appointed they must all consent to his nomination. To put a stop to the further propagation of this rumour, the official journals of the four united Governments declare that the continued existence of this venerable university is assured both by public grants and by large donations recently made by old pupils and others. This communication has been received with general satisfaction, particularly in the town of Jena itself, which is entirely dependent upon the university."

SCIENTIFIC SERIALS.

The Mathematical Gazette, No. 5 (May 1895).—This number opens with a paper read by Dr. C. Taylor at the annual meeting of the A.I.G.T. in January last, of which the title is "The Syllabus of Geometrical Conics." In it the writer passes in review what he has done in the subject since his first contribution to the *Messenger* in 1862. Amongst other reasons for writing at this date, Dr. Taylor states: "I have, as I think, arrived at something like finality in my own view of the way in which the subject should be approached." It is on this ground that we commend the author's paper to persons interested in the teaching of geometrical conics. They will derive profit from it. The second of the mathematical worthies noticed by Mr. Heppel is John Dee, noteworthy from his contributions to Billingsley's translation of Euclid. The notes, solutions of *Gazette* questions, solutions of examination questions, and questions for solution, which are all very useful for the readers addressed, are, with the enlarged form of the journal, greatly increased in number and variety. Several recent text-books are also the subject of judicious and discriminating criticism. The *Gazette* should certainly have a successful career.

American Journal of Mathematics, vol. xvii. No. 3.—On irrational covariants of certain binary forms, by E. Study, discusses the most important covariants of binary cubics and quartics and of some other special binary forms. After paying tribute to the methods of Cayley and Clebsch, the author gives his reasons for working the whole subject over again. By means of a carefully chosen system of notation, he presents his results, as he believes, in a form that will be useful to those who have to deal with the numerous applications of the binary quantics of the lowest orders. In some detail (pp. 185–215) he examines the cubic, and the quartic and octahedron, and points out several small numerical errors in previously obtained results. The same writer contributes an article on the connection between binary quartics and elliptic functions. This is an application of the theory developed in the previous article to elliptic functions. In it he compares the relations among the rational and irrational covariants of a quartic with the identities among the four theta-functions; by this means a new light is thrown upon the familiar formulæ, and at the same time a number of new results are derived, which make the theory in question, the author states, in a certain sense *complete*. Stress is laid upon the fact that all the results are obtained by means of *actual calculations*, and that no use is made of the method of indeterminate coefficients.—Semi-combinants as concomitants of affiliants, by H. S. White, opens up a new path apparently (pp. 234–265): "I consider all ground forms that are included in the conjunctive of the system, and those of them that satisfy invariant equations of suitable order, linear in their coefficients, I designate as *affiliant* ground forms." The paper shows that not only is every semi-combinant ground form an affiliant, but also every affiliant ground form is a semi-combinant. Three short notes follow, viz.: Simplification of Gauss's third proof that every algebraic equation has a root, by M. Böcher, a note read before the American Mathematical Society (*cf.* Nature, p. 189): note sur les lignes cycloïdales, by R. de Saussure; and note on lines of curvature, by T. H. Taliaferro.

SOCIETIES AND ACADEMIES.

London.

Royal Society, April 25.—"*Acokanthera Schimperi*: Natural History, Chemistry, and Pharmacology." By Prof. Thomas R. Fraser, F.R.S., and Dr. Joseph Tillie.

Specimens of the wood from which the Wa Nyika, Wa Gyriama and Wa Nyika arrow-poison is prepared have been examined by us and referred to the genus *Acokanthera*, and

leaves, flowers, and fruit, each taken from the same individual tree, having also been sent to us, we have been enabled to determine that the wood of the species *Acokanthera Schimperi*, Benth. and Hook. (*Carissa Schimperi*, A.DC.), is used by the Wa Nyika and other tribes inhabiting the coast regions near Mombasa in preparing their arrow-poisons.

The arrow-poisons of these tribes usually contains a crystalline glucosidal active principle, which, in its chemical properties and pharmacological action, is identical with the active principle also separated by us from the wood of *Acokanthera Schimperi*.

The complete recognition of the species of *Acokanthera* is of primary importance, because several supplies of the wood of unidentified species of *Acokanthera* sent to us from East Equatorial Africa yielded only a glucosidal active principle which was amorphous.

The characters of the crystalline active principle which we have separated from the wood of the fully identified species, *Acokanthera Schimperi*, Benth. and Hook., agree with those of the crystalline active principle ouabain, separated by Arnaud from the wood of the unidentified species of *Acokanthera*, provisionally named *Ouabaio*, obtained from North Somaliland, and also from the seeds of an unidentified species of *Strophanthus*, obtained from West Africa. As, however, the name ouabain is used for three quite different substances, two of which are amorphous, we would suggest that, in accordance with a usual custom, the crystalline active principle of *Acokanthera Schimperi* should be named acokantherin, and not ouabain.

The work accomplished by Arnott and by Haines in 1853, by Ringer in 1880, by Rochebraune and Arnaud in 1881, by Laborde in 1887, by Langlois and Varigny, by Gley and Rondeau, and by Gley in 1888, by Sailer in 1891, by Paschkis in 1892, and by Lewin in 1893, has been more fully described in this paper than in our preliminary notice of March 23, 1893.

A detailed examination of the pharmacological action of acokantherin has not led to the discovery of any important qualitative differences between its action and that of *Strophanthus hispidus* and of its active principle strophanthin, which was described by one of us in 1870, in 1872, and in 1890. As, however, a special interest must be attached to the effects upon the circulation, the experiments upon the heart, blood-vessels, and blood-pressure are described with more detail than those upon other systems.

The predominant action of acokantherin is that exerted upon striped muscle, and, because of this action, with possibly an action upon the intrinsic cardio-motor ganglia, the chief effect is produced upon the heart, while the influence exerted upon the cardio-respiratory centres in the medulla is relatively slight or secondary.

May 30.—"On the Effect of Pressure of the Surrounding Gas on the Temperature of the Crater of an Electric Arc Light. Preliminary Notes of Observations made at Daramona, Streete, Co. Westmeath." By W. E. Wilson.

Of late years it has often been assumed that the temperature of the crater forming the positive pole of the electric arc is that of the boiling of carbon. The most modern determinations give this point as about 3300°–3500° C.

Solar physicists have thought that the photosphere of the sun consists of a layer of clouds formed of particles of solid carbon. As the temperature of these clouds is certainly not below 8000° C., it seems very difficult to explain how carbon can be boiling in the arc at 3500° and yet remain in the solid form in the sun at 8000°. Pressure in the solar atmosphere seemed to be the most likely cause of this, and yet, from other physical reasons, this seemed not probable.

In order to investigate whether increased pressure in the gas surrounding an electric arc would raise the temperature of the crater, the author used a strong cast-iron box in the interior of which an electric arc light could be maintained. At the side of the box was inserted a glass lens, by which an image of the crater was formed at a distance of 80 cm. When this image was allowed to fall on the aperture of a Boys radio-micrometer, the deflections of this instrument showed any variations in the radiation from the crater. The author then describes the experiments made with this apparatus, and shows that by increasing the pressure of the gas in the box the temperature of the crater is considerably lowered intead of being raised, and he concludes that these experiments seem to show that the temperature of the crater, like that of a filament in an incandescent lamp, depends on how much it is cooled by the surrounding atmosphere, and not on its being the temperature at which the vapour of carbon has the same pressure as the surrounding atmosphere. That carbon volatilises in some form at comparatively low temperatures seems likely, from the way in which the carbon of incandescent lamp filaments is transferred to the glass. The pressure of the vapour of carbon in the arc may consequently be very small, and further it would seem that the supposition of high pressures in the solar photosphere, which has been referred to in the beginning of this paper, is not borne out by these experiments, and that carbon may exist there in the solid form at very high temperatures although the pressures are comparatively low.

June 13.—"Further Observations on the Organisation of the Fossil Plants of the Coal Measures. Part 3, *Lyginodendron* and *Heterangium*." By W. C. Williamson, F.R.S., and D. H. Scott, F.R.S.

The authors sum up their conclusions as follows:—

The vegetative organs of these genera show a remarkable combination of fern-like and cycadean characters. The leaves of *Lyginodendron*, which are now well known, are so like fern-leaves, not only in form and venation but in minute structure, that if they stood alone they would, without hesitation, be referred to Filices. Although many leaves simulate those of ferns in external characters (*Stangeria*, *Thalictrum*, &c.), none are known which at the same time show the characteristic anatomy of fern-leaves. Hence we are led to attach great weight to the characters of the *Lyginodendron* foliage. That of *Heterangium*, though less well preserved, was evidently of the same type.

In *Heterangium* the primary structure of the stem is much like that of a monostelic fern such as *Gleichenia*, but the leaf-trace bundles closely resemble the foliar bundles of a Cycad.

In *Lyginodendron* the whole structure of the *stem* suggests a Cycad, but with the remarkable peculiarity that the bundles here have the structure which in Cycadeæ is usually (though not always) limited to those of the leaf. The cycadean characters are too marked to be accidental, though the general anatomy of *Lyginodendron* is not inconsistent with a close relationship to ferns, for in *Osmunda* we have a monostelic fern, with a large pith, collateral bundles in the stem, and concentric ones in the leaf. The mere occurrence of secondary growth in a fern-like plant is not surprising, considering that it takes place in *Botrychium* and *Helminthostachys* at the present day.

In various respects *Lyginodendron* and *Heterangium* have points in common with Gleicheniaceæ, Osmundaceæ, Marattiaceæ, Ophioglosseæ, and Cycadeæ. The view of their affinities, which we suggest, is that they are derivatives of an ancient generalised race of ferns, from which they have already diverged considerably in the cycadean direction. Of the two genera, *Heterangium* appears to be geologically the more ancient, and certainly stands nearer to the filicinean stock. *Lyginodendron*, while retaining conspicuous fern-like characters, has advanced much further on cycadean lines. This view by no means involves the improbable assumption that these plants were the actual ancestors of existing Cycadeæ. How far their divergence from the fern stock had proceeded cannot be determined until we are acquainted with their organs of reproduction.

The existence of a fossil group on the border land of ferns and Cycads seems now to be well established. Count Solms-Laubach places his *Protopitys* in this position, which is probably shared by *Myeloxylon* and *Poroxylon*. Messrs. Bertrand and Renault have indeed endeavoured to derive the last-named genus from Lycopodiaceæ, and have extended the same view to *Lyginodendron* and *Heterangium*. In the latter cases their theory is completely negatived by the organisation of the leaves, and by many structural details.

The relation of the genera which we have described to those ancient gymnosperms, the *Cordaiteæ*, will form one of the most interesting palæobotanical problems of the future.

The paper is illustrated by micro-photographs and by camera-lucida drawings.

Geological Society, June 19.—Dr. Henry Woodward, F.R.S., President, in the chair.—On the occurrence of radiolaria in chalk, by W. Hill and A. J. Jukes-Browne. The authors noticed the rarity of records of Cretaceous radiolaria, and alluded to those which have been made, including those by Rüst and Sollas. They recently discovered spherical bodies resembling in form and general appearance certain calcified and partially destroyed radiolarian tests from some of the Barbadian

rocks; microscopic examination of these proved that many of them, at any rate, are radiolaria. The bodies occur in the nodules of the lower beds of the Melbourn rock at Melbourn, Royston, near Hitchin, Leagrave, near Luton, Pitstone and Tring, Watlington, the Richmond boring, the lower part of the "Grit Bed" at Dover, Sutton Waldron and Burcombe (Dorset), and in a nodular chalk which may be considered as the equivalent of the Melbourn rock from Bindon Cliffs, near Axmouth, Devon. Similar organisms have recently been found in the chalk marl of Lincolnshire, Yorkshire, and Norfolk, but have not been noticed in any other parts of the chalk. It was suggested that they occurred in many portions of the chalk-ooze, but were usually rapidly and completely dissolved, and contributed to that solution of silica which furnished the substance of flint-nodules; and the authors concluded that the preservation of traces of the radiolaria in the nodules of the Melbourn rock was due to some specially favourable conditions. A description of the changes undergone by Barbadian radiolaria was given to illustrate the instability of radiolarian tests. All stages were traceable, from the perfect siliceous test to a structureless ball or disc filled with calcareous matter, or a mere patch of clear crystalline material. A description of forms recognised in the nodules of the Melbourn rock was given.—The crush-conglomerates of the Isle of Man, by G. W. Lamplugh, with an appendix by W. W. Watts. The Skiddaw slates of the Isle of Man have everywhere undergone intense shearing, and on the north-west side of the main stratigraphical axis actual disruption of the bedding with the resultant formation of breccia or crush-conglomerate on a large scale has taken place. This structure attains its widest development on the north side of the central valley, though it is noted on a more limited scale in a few localities farther south. The sections described showed the gradual smashing into fragments of highly contorted strata until every trace of the original bedding is lost, and a "crush-conglomerate" with lenticular and partly rounded inclusions is formed. The rocks described in Mr. Watts's appendix were grouped in four classes. Firstly, the grits and slates which had been crushed but had not been converted into crush-conglomerates; secondly, the crush-conglomerates themselves, and the fragments which they contain; thirdly, the dykes of decomposed dolerite (greenstone) and fresh later dolerite which penetrate the conglomerate; fourthly, a portion of the crush-conglomerate metamorphosed by these intrusions. The chief point of interest was brought out by the examination of the fragments in the conglomerate. All stages of crushing could be traced, until the grit-fragments had a structure which was a mere miniature of the crush-conglomerate itself; that is to say, if the crush-conglomerate be regarded as made of "fragments" of hard rocks enclosed in crushed "matrix" of soft rocks, a host of intermediate varieties with varying resistances will occur.—The chalky clay of the Fenland and its borders; its constitution, origin, distribution, and age, by Sir Henry H. Howorth, M.P., F.R.S. The distribution of the clay (so often termed chalky *boulder* clay) was noticed. The paucity of foreign stones was noted as compared with natives, and the similarity of the matrix of the chalky clay to the material of the older deposits of the neighbourhood. The author maintained that the contents of the clay indicate movement of material from west to east in some places, as shown by Jurassic fossils in the East Anglian chalky clay, and from east to west in others; in fact, that movement took place in sporadic lines diverging from the Wash and the Fens. He appealed to the amount of disintegration that had taken place to furnish the material for the clay, the shape of the stones in the clay, and the distribution of the clay itself, as evidence against the action of land-ice or icebergs, and maintained that there was no evidence of submergence at the time the clay was formed; and criticised the attempts made to explain the formation of the clay by water produced by the melting of ice.—On the occurrence of *Spirorbis*-limestone and thin coals in the so-called Permian rocks of Wyre Forest; with considerations as to the systematic position of the "Permians" of Salopian type, by T. Crosbee Cantrill. In South Staffordshire a thick series of red rocks—the so-called Lower Permian—overlies the ordinary yellow and grey coal measures, and underlies the Triassic rocks. They consist of sandstones, marls, calcareous conglomerates, and breccias, having a general red or purplish-red colour. Sinkings have shown that these red rocks must be regarded as of Upper Coal Measure age, because their included fossils have an Upper Coal Measure *facies*. The rocks contain bands of limestone characterised by the presence of *Spirorbis pusillus*: those parts of the series which have not yielded Coal Measure fossils being apparently similar lithologically to those which have yielded them. The evidence furnished by the deposits of the Forest of Wyre (= Enville) district also led the author to regard the red rocks associated with *Spirorbis*-limestone and coals as Upper Coal Measures, exhibiting a gradual passing away of Coal Measure conditions and the incoming of those of new red sandstone times.

Linnean Society, June 20.—Mr. C. B. Clarke, President, in the chair.—Mr. F. Enock exhibited and made some remarks upon a living specimen of an aquatic hymenopterous insect, *Polynema natans*, Lubbock.—Messrs. E. Baker and C. Reid exhibited some rare plants from the limestone hills, Co. Kerry, including *Pinguicula grandiflora*, Lam. contrasted with *P. vulgaris*, and *Saxifraga Geum* contrasted with *S. umbrosa*, with a view of determining their value as sub-species or geographical races.—Mr. Carruthers exhibited some feathers of a cuckoo taken at Whitchurch, Shropshire, on May 23 last, amongst which were some moulted feathers which were held connected with the new feathers which had replaced them by means of the barbed seed capsules of a sub-tropical grass, *Cenchrus echinatus*.—On behalf of Mr. S. Loat, there was exhibited a cuckoo's egg, taken from the nest of a hedge-sparrow, together with five white eggs of that species, an abnormality not often met with. An examination of these eggs under the microscope showed that, in regard to the texture or grain of the shell, they agreed with eggs of the hedge-sparrow, and not with those of the robin, of which white varieties are not so rare.—Mr. George West then gave the substance of a paper on some North American *Desmidieæ*, describing the characters of several new species with the aid of specially prepared lantern slides.—Mr. A. Vaughan Jennings gave a detailed account of the structure of the Isopod genus *Ourozeuktes*, upon which a most instructive criticism was offered by the Rev. J. R. Stebbing, who was present as a visitor; some further remarks being offered by Mr. W. P. Sladen.—Mr. F. N. Williams communicated the salient points in a critical paper which he had prepared, entitled "A Revision of the Genus *Silene*." On behalf of Mr. E. R. Waite, Prof. Howes gave an abstract of a well illustrated paper on "The Egg-cases of Port Jackson Sharks," and exhibited several spirit specimens in further elucidation of the subject.—This meeting terminated the session.

PARIS.

Academy of Sciences, June 24.—M. Marey in the chair.—On the gradual extinction of an ocean-roller at great distances from its place of production: formation of equations of the problem, by M. J. Boussinesq.—New studies on the fluorescence of argon and on its combination with the elements of benzene, by M. Berthelot. With the help of M. Deslandres, the author has made a more complete spectroscopic examination of the emerald-green light produced by the fluorescence of argon under the influence of the silent electric discharge. The significance of the various rays observed or photographed is discussed. Finally, the conclusion is drawn that this fluorescence is definitely due to a condensation compound of argon; it points to the probable existence of a complex state of equilibrium in which argon, mercury, and the elements of benzene are concerned.—On the campholenic lactones, by MM. Berthelot and Rivals. The lactones have heats of formation greater than those of the isomeric acids.—On the heats of solution and neutralisation of campholenic acids, by M. Berthelot.—Reduction of silica by carbon, by M. Henri Moissan. With a current of 1000 amperes at 50 volts, the author has obtained characteristic crystals of silicon, but always mixed with carbon silicide. At the high temperature attained, carbon from the crucible reduces the silica of the charge.—Observations on a note, by MM. Barbier and Bouveault, on the products of condensation of valeric aldehyde, by M. C. Friedel.—On the integration of linear equations by the aid of definite integrals, by M. Ludwig Schlesinger.—On the determination of the ratio of the two specific heats for air, by M. G. Maneuvrier. A new method and new apparatus are described. The experimental determination of the ratio of the specific heats has yielded the following numbers:—Air, $\gamma = 1\cdot3924$; carbon dioxide, $\gamma = 1\cdot298$; hydrogen, $\gamma = 1\cdot384$ under the ordinary conditions of temperature and pressure.—On the propagation of sound in a cylindrical tube, by MM. J. Violle and Th. Vautier. An account of the conduction of musical sounds over long distances by pipes of wide diameter.—On the refraction and dispersion of ultra-violet radiations in some crystallised substances,

by M. G. Adolphe Borel.—On the variations of "écrouissage" of metals, by M. Faurie.—On punching, by M. Ch. Fremont. An experimental inquiry into the conditions affecting the amount of play necessary between a punch and its bed. The results lead to the conclusions: (1) That the maximum effort in punching metals is independent of the clearance space in the ordinary practical conditions; (2) that the clearance space or play is a function of the thickness of the metal to be punched, and not of the diameter of the punch; (3) that it is also a function of the elongation of the metal, but in a less proportion; (4) that the play allowed ought to be about a fifth of the thickness of the metal punched. A figure is given illustrating the form of punch best adapted for piercing perfect holes.—Properties of solid carbonic acid, by MM. P. Villard and R. Jarry. Carbon dioxide solidifies and melts under a pressure of 5·1 atmospheres at $-56{\cdot}7$ C. In free air, the solid has the temperature $-79°$; ether does not lower this temperature, as has been hitherto supposed, but methyl chloride and solid carbon dioxide produce a temperature of -85 C. At a pressure of 5 mm. the solid has reached a temperature of -125.—On M. Guye's hypothesis, by M. A. Colson.—On the alcohols derived from a dextro-rotatory turpentine, eucalyptene, by MM. G. Bouchardat and Tardy.—Condensation of the unsaturated alcohols of the fatty series with dimethylketone.—Synthesis of aromatic hydrocarbons, by MM. Ph. Barbier and L. Bouveault.—Double compounds of the fatty and aromatic nitriles with aluminium chloride, by M. G. Perrier.—Action of the air on raisin must, by M. V. Martinand.—On the preservation of wheat, by M. Balland.—On the sexual dimorphism of the Nautilus, by M. A. Vayssière.—On the variations of apparent clearness with the distance, and on a law of these variations as a function of the luminous intensity, by M. Charles Henry.—Seismic observations made at Grenoble, by M. Kilian.—On the dissolved gases at the bottom of Lake Geneva, by MM. André Delebecque and Alexander Le Royer.—The effects of the synodic and anomalistic revolutions of the moon upon the distribution of pressures in the season of winter, by M. A. Poincare.—On the subject of the treatment of the bites of venomous serpents by chloride of lime and by antitoxic serum, by M. A. Calmette.

Amsterdam.

Royal Academy of Sciences, May 25.—Prof. Van de Sande-Bakhuyzen in the chair.—Prof. J. C. Kapteyn showed how the following three laws may be deduced from observations: (1) the law according to which the linear velocities of the stars are distributed; (2) the law according to which the number of stars per unit of volume varies with the distance from the sun; (3) the law according to which the absolute stellar magnitudes (magnitude at unit of distance) are distributed. The hypotheses on which the author's conclusions were based were as follows: (*a*) the real movements of every degree of magnitude of the stars in space are equally numerous in every direction; (*b*) the law of the distribution of stellar velocities does not vary with the distance from the sun; (*c*) the function representing this law has but a single maximum.—Prof. Engelmann treated of reciprocal and irreciprocal conductivity of muscles, with special relation to the theory of the heart.—Prof. Van der Waals treated of the relation between the critical temperature and the critical pressure for a mixture (tacnodal curve).—Prof. H. Behrens described some cases of artificial dichroism. Strong dichroism were observed on flax and hemp fibres after having been dyed with congo-red or benzo-azurine. A similar result was obtained with the majority of the tetrazo-dyestuffs used for dying cotton; whereas, by the application of naphthol orange, croceine scarlet, and other similar dyestuffs, no dichroism was developed. Only three basic dyestuffs were found to be capable of making flax dichroic. Among other fibres the straw fibre comes next to flax and hemp; the cotton and the wood fibres stand lower in the scale; silk requires to be dyed a deep blue with an acidulated solution of benzo-azurine, and on wood the phenomenon of artificial dichroism has not been produced by any of the colouring matters named above. Flax and hemp are strongly polarising, and can be rendered strongly dichroic, while in cotton these two qualities are found in a smaller degree; but silk, ranging above straw in polarisation, falls far below cotton as to artificial dichroism. The phenomenon seems to be of a complex nature, not explained by assuming a combination of ordinary absorption with ordinary double refraction.—Prof. Van der Waals presented a paper by Prof. W. H. Julius, entitled "On an arrangement for protecting measuring instruments from the ordinary vibrations of the ground."—Prof. Kamerlingh Onnes presented, (1) on behalf of Prof. W. Einthoven, an isolation arrangement against vibrations of contiguous bodies; (2) on behalf of Dr. J. P. Kuenen, the influence of gravitation upon the critical phenomena of simple substances and mixtures.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—A Manual of Botany: Prof. J. R. Green. Vol. 1. Morphology and Anatomy (Churchill).—Architecture for General Readers: H. H. Statham (Chapman).—The Manufacture of Explosives, 2 Vols.: O. Guttmann (Whittaker).—The Cell: Dr. O. Hertwig, translated by M. Campbell, and edited by Dr. H. J. Campbell (Sonnenschein).—Studies in the Evolution of Animals: Dr. E. Bonavia (Constable).—Electrical Laboratory Notes and Forms: Prof. J. A. Fleming (*Electrician* Company).—Ostwald's Klassiker der Exakten Wissenschaften, Nos. 60, 61, 62 (Leipzig, Engelmann).—A Manual of Book-keeping: J. Thornton (Macmillan).—Geographical Journal, Vol. v. (Stanford).

Pamphlets.—Il Porto di Venezia: Prof. L. Primo (Verona, Drucker).—The Genesis of California's First Constitution (1846-49): R. D. Hunt (Baltimore).—Enumeración Sistemática y Sinonímica de los Peces de las Costas Argentina y Uruguaya: Dr. C. Berg (Buenos Aires).—Origine e Diffusione della Stirpe Mediterranea: G. Sergi (Roma, Società Editrice Dante Alighieri).

Serials.—Journal of the Royal Microscopical Society, June (20 Hanover Square).—Chambers's Journal, July (Chambers).—Good Words, July (Isbister).—Sunday Magazine, July (Isbister).—Humanitarian, July (Hutchinson).—English Illustrated Magazine, July (198 Strand).—Zeitschrift für Physikalische Chemie, xvii. Band, 2 Heft (Leipzig, Engelmann).—National Review, July (Arnold).—Natural Science, July (Rait).—Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, March and April (St. Pétersbourg).—The Reliquary and Illustrated Archæologist, July (Bemrose).—Contemporary Review, July (Isbister).—Geographical Journal, July (Stanford).—Journal of the Royal Agricultural Society of England, Vol. 6, Part 2 (Murray).—Fortnightly Review, July (Chapman).

CONTENTS.

THURSDAY, JULY 11, 1895.

THE TEACHING OF PATHOLOGY.

The Elements of Pathological Histology. By Dr. A. Weichselbaum. Translated by W. R. Dawson. London: Longmans, Green, and Co., 1895.

THOSE who have watched the progress of pathological teaching, in this country especially, must have recognised that during recent years its scope has become much wider, or that at least there is a tendency towards broader conceptions. Cohnheim made an attempt to cast off the narrow fetters of Morbid Anatomy, and to instil into his pupils that wonderful enthusiasm which he himself felt for General Pathology, or, as we may term it, "Morbid Physiology." His "Vorlesungen uber Allgemeine Pathologie" still form a monumental record of what he has achieved, and his method must and should be the ideal of every teacher of pathology. Strange to say with his death things reverted into the old groove, and until recently, pathological teaching restricted itself almost exclusively to Morbid Anatomy. "Nec silet mors" is the motto of the Pathological Society: it is not appropriate, because pathology deals not merely with death; its soul and essence, however morbid, is "life." Bacteriology, now a recognised branch of pathology, in spite of all the harm it has wrought, has achieved this, that it has carried us away from the dead-house to the laboratory, and has awakened in us the spirit of experimental inquiry.

Bacteriology should be regarded, however, as an adjunct to pathology, *i.e.* so far as it applies to disease; beyond that it belongs to botany. Every bacteriologist should be a pathologist, and every pathologist should possess an adequate knowledge of physiology as well as a complete mastery over morbid anatomy. The day is to be regretted when we follow the footsteps of our continental brethren, and become mere specialists in the art of growing bacteria and of immunisation. It is not intended to give the erroneous impression that morbid anatomy is not pathology—it still is, as ever it was, the most important partner from the student's as well as the investigator's point of view and for practical purposes; but this must be insisted upon, that the morbid physiology of the body and of disease has been too much neglected. This becomes evident when we look through our text-books and manuals of pathology. Year after year we have fresh treatises on morbid anatomy and histology, or on bacteriology, but there is, if we except Cohnheim's classical work, hardly a book on the pathology of disease and its processes. If we wish to learn this, we have to turn to our standard works on medicine or to the journals. The present volume, the subject of this review, deals exclusively with morbid histology and bacteriology, and for that reason, however valuable it may be, it may be asked whether there was the need for Dr. Dawson to give up so much time to its translation. We have a sufficient number of similar works already; why give us a stone when it is bread we want? Prof. Weichselbaum's name is sufficient to lead us to expect a useful book on bacteriology, and a satisfactory one on morbid histology; more we cannot look for from that source. A careful perusal of the translation justifies our expectations.

Of 441 pages, more than eighty are devoted exclusively to bacteriology, *i.e.* to the description of bacteriological methods, and to a *résumé* of the general principles. If we keep in mind that under each organ also subsequently all the various infective and microbic lesions are carefully discussed, it seems to us that the author has given undue prominence to this, which is, after all, a small part of his subject.

It is difficult to serve two masters, and the result must be that for bacteriological methods and principles we shall continue to consult special works: they are numerous, and it would be difficult to find works of greater usefulness than Prof. C. Frankel's excellent text-book or the elaborate compilation of Dr. Heim. The directions given for bacterial staining or cultivation are too meagre to be of much use to the beginner. Gram's well-known method, *e.g.*, is described thus: "Sections are placed for half an hour in aniline gentian violet, then for two or three minutes in iodine and potassium iodide, and then in alcohol, which is changed as it becomes coloured. Finally they are cleared and mounted." One can imagine the poor tyro mournfully contemplating the result of those instructions. We therefore adhere to the opinion that in works intended for students, too much should not be offered between the covers; but, if a comprehensive treatise is intended, fulness and completeness of directions and instructions are imperative. The descriptions of the micro-organisms, though short, are succinct and good, so far as they go; but the German edition having appeared in 1892, new discoveries and altered views are wanting, and the briefness is often exasperating.

Now as to the purely anatomical or histological part of the book, it also suffers from shortness, and we must confess that we have works in the English language which are sure to occupy a higher position than this translated importation. Useless Dr. Dawson's work certainly is not; the beautiful illustrations and a chapter on blood examination, short though it be, recommend it. Many of the illustrations are new and original, and are exactly the kind of representation wanted to bring out the salient points in a histological specimen. The English publishers also have done all they could to give the work a good appearance, and altogether it is a pleasant book to possess. It is essentially an annotated picture-book; but as a picture-book it is excellent, and will be of great use to those who consider the study of morbid anatomy and histology a form of "Anschauungsunterricht"; and, indeed, much can be learnt from good pictures. One point this work brings home to us in a painful manner, viz. the decline of pathological anatomy. Bacteriology swamps everything. On the continent, professorial chairs of pathology are occupied by bacteriologists, and the instruction of hygiene is also given over to bacteriologists. The result is that sound pathological anatomy is pushed steadily into the background. So far in this country, fortunately, we have suffered less; in principle, at least, we still consider bacteriology merely a fraction or an element of pathology, but already the spectre has risen, and unless we take care, we also shall be ruled by the bacillus, and find contentment in the haven of mediocrity

which so-called bacteriological research opens up to those who, incapable of doing real pathological or physiological work, have leisure to practise bacteriology as a "fireside" game.

In conclusion, a word in praise of the translator and editor: he has done his work excellently, so well, in fact, that one cannot help regretting that he used his gifts and expended his labours on a book hardly worthy of so much conscientious energy and patience. The translation is better than the original in arrangement, type and general "get up." Since it is pleasing to most to possess a nice book, and one which is at the same time instructive, in spite of some remarks which may appear severer than they are meant to be, we may recommend it safely as an addition to the student's library.

A. A. Kanthack.

THE NATURAL HISTORY OF AQUATIC INSECTS.

The Natural History of Aquatic Insects. By Prof. L. C. Miall, F.R.S. London: Macmillan and Co., 1895.

PERHAPS no country possesses so many amateur naturalists as England, at least in proportion to its population, and it is not without significance in this direction that many of our best professional men of science have not thought it undignified to furnish sound information on their special subjects in a popular and yet accurate manner. The present work is a good example of this, and Prof. Miall deserves praise for the admirable account he has put together of the insect inhabitants of our lakes, ponds, and watercourses.

Of course it has not been without forerunners. One of the last works of that well-known writer on popular science, the late Rev. J. G. Wood, was entitled "The Brook and its Banks," and covered much the same ground; but one may say, without any disparagement, that his book was more picturesque or anecdotal natural history than strictly scientific.

Again, Prof. Miall, like every subsequent writer on entomological subjects, is greatly indebted to the laborious researches of Swammerdam, Réaumur, Lyonnet, and others of the early naturalists, but in every case this is freely acknowledged, and he adduces their works as models of patient investigation on the living animal, particularly worthy of emulation at the present time, when attention is almost exclusively paid to phylogeny and classification, to the neglect of the actual life history, where so much still remains to be discovered. Some essential matters are briefly treated in an introductory chapter, such as the equilibrium of aquatic insects, the tension of the surface film of water and its effect on small objects, and also the question of the original habitat of insects, whether terrestrial or aquatic, which Prof. Miall confidently decides as the former, mainly from the general presence of tracheæ and functionally active spiracles even in purely aquatic insects, showing that such as are fitted for breathing only dissolved air are those that deviate from the general and primitive rule. The chief aquatic Coleoptera are taken first, and certain curious structures in the larva and imago of several families somewhat fully described. Among these we comment on the mouth organs of the larva of Dytiscus, which have been a subject of controversy from the time of Swammerdam and De Geer up to Meinert, Schiödte and Burgess, whose description has been verified by Prof. Miall, and also the well-known tarsal clasping suckers of the adult male, the real structure and action of which was first pointed out by Lowne. The method of respiration in the adult Hydrophilus is well explained, and the extraordinary arrangement for obtaining air from cavities in submerged roots adopted by the larva of Donacia, as discovered by Siebold. Flies with aquatic larva receive considerable attention, no less than 122 pages being devoted to these extremely interesting creatures, which from their transparence, in many cases, have long been favourite objects with microscopists. The development of the Gnat, Chironomus, Simulium, Eristalis, and numerous others is fully gone into, and the amateur naturalist will find plenty of occupation, and derive no little benefit, by following out their structure with this book as his guide. There is a short account of that very beautiful aquatic hymenopterous insect Polynema, which, according to Ganin, deposits its eggs in the eggs of a Dragon-fly; and another form, Agriotypus, said to be parasitic on a Caddis-worm. Caddis-flies (Trichoptera), Sialis, the alder-fly of anglers, the stone-flies, may-flies, dragon-flies, pond-skaters, water-boatmen, and all the rest of the host of insects which pass a large part of their existence in the water, are dealt with in due order, and the descriptions are frequently supplemented with bibliographies, which will be useful to those who require further information on special points. A word must be said for the illustrations, which in large part have been drawn by Mr. A. R. Hammond for this work; they are extremely clear and well executed—quite a relief, indeed, from the old clichés usually considered good enough by publishers to adorn a work of this kind. Altogether, the "Natural History of Aquatic Insects" is a very good and useful specimen of its class.

OUR BOOK SHELF.

The Royal Natural History. Edited by Richard Lydekker, F.R.S., &c. Volume iii. (London: Warne, 1895.)

THE third volume of this excellent "Natural History" finishes the mammals, and commences the birds.

Among the former the Cetaceans, the Rodents, the Edentates with the pouched mammals, and the Monotremes are described at appropriate length. The information is generally up to date, and the illustrations are good. To the notices of the occurrence of Sowerby's whale on the coasts of England and Scotland, may be added that of its being captured some years ago in Brandon Bay, Kerry, the head of the specimen being in the Dublin Museum. The immense group of the Rodents is judiciously treated, most of the more important facts of their history being given. Only six pages are devoted to the egg-laying mammals, and there is no figure of the duckbill's egg.

The chapters on the perching birds and Picariae are contributed by Mr. H. A. Macpherson and Dr. Bowdler Sharpe. "The number of the existing species of birds being in all probability considerably over ten thousand," the authors are obliged to treat of them even in a more condensed form than were the mammals; still the order of Passeres, which includes by far the majority of known birds, is fairly treated, and most of the well-known or interesting birds are alluded to. Dr. Sharpe confesses

his inability to give a diagnosis of the Picariæ, that is in the logical sense, but claims that the group as selected by him possess "certain common features not found among the Passeres." In the last chapter in this volume, he treats of the Jacamars to the Toucans.

Cours Élémentaire d'Électricité. By M. B. Brunhes. Pp. 265. (Paris: Gauthier-Villars et Fils, 1895.)

THE experimental laws and general principles belonging to the study of technical electricity are set forth in this book in an elementary, but strictly scientific, manner. The book reproduces the author's first-year course of theoretical electricity at the Institut industriel du Nord de la France, and its contents furnish just the kind of foundation needed by students of electrical engineering. In several respects, the treatment differs from that generally followed in text-books; hydrodynamic analogues are entirely omitted, and the word potential is not employed, voltage, or E.M.F. between two points, being used to express potential difference.

Off the Mill: Some Occasional Papers. By G. F. Browne, B.D., D.C.L., Bishop of Stepney. Pp. 271. (London: Smith, Elder, and Co., 1895.)

ALPINE climbers, and others who find delight in mountain-peaks and glaciers, may like to read the papers on Alpine subjects reprinted in this volume. The papers originally appeared thirty years ago, and they offer to the present generation of mountaineers an interesting picture of the way in which climbs were then made. The ice-caves in the neighbourhood of Annecy form the subject of one of the papers appealing to scientific readers.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Cyclonic Indraught at the Top of an Anticyclone.

BETWEEN June 7 and 12 an anticyclone, with maximum pressure of 30·20 to 30·30 inches, passed slowly from the north-west across southern New England. The isobars formed well-defined ovals, with their longer axes running from south-west to north-east. It was difficult to locate the centre of the anticyclone because the isobars were broken on the side toward the ocean: but, by drawing a line through the stations showing the maximum pressure, the crest or ridge of the anticyclone could be easily located up to the 11th, after which it passed off the coast and its position became somewhat uncertain, although the pressure continued above normal over southern New England until the night of the 12th.

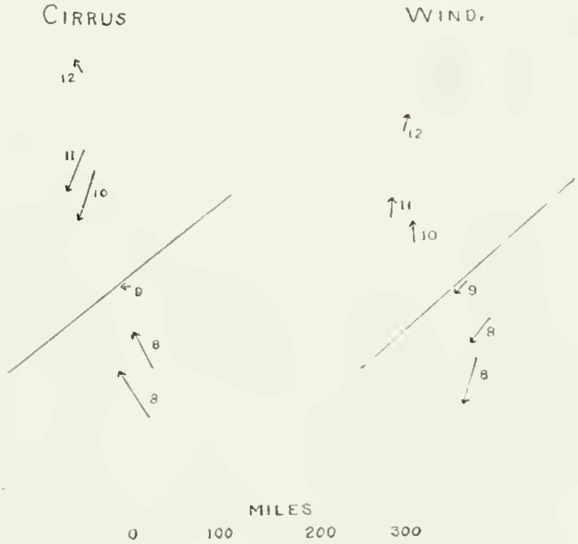
The interest attaching to the anticyclone lies in the fact that cirrus observations obtained on both sides of the line of maximum pressure indicate an indraught at the top of the anticyclone of the same nature as that observed at the bottom of cyclones.

The anticyclone passed nearly centrally over the Blue Hill Meteorological Observatory. As it approached from the north-west, the cirrus clouds on the 8th were observed moving from the south south east. As the line of maximum pressure passed over the observatory on the 9th, the cirrus movement shifted to the north-east, from which direction it was observed on the 10th and 11th. This change corresponds almost exactly with what would be observed in the surface wind should a trough of low pressure pass over Blue Hill from the same direction. On the 12th the cirrus shifted to south, and on the 13th to the west, with the approach of a cyclone from that direction.

The direction of cirrus movement and the mean direction of the wind is recorded at the observatory in degrees of azimuth beginning with the south point. The first is measured with a nephoscope, and the second recorded by a Draper anemoscope. The following table gives the cirrus and corresponding wind observations between the 8th and 10th, no cirrus observations being obtained on the 7th. The velocities of the cirrus were obtained by multiplying the observed relative velocities by a factor to reduce to absolute velocities. This factor was determined from direct measurements of cloud heights and velocities carried on for some time at this observatory. The last column in the table contains the directions in degrees of azimuth of the line of maximum pressure in the anticyclone, taken from the maps of the United States Weather Bureau.

	Cirrus.		Wind.		
	Dir. from.	Veloc. Miles.	Dir. from.	Veloc. Miles.	Line of max. press.
June 8, 8 a.m.	329	48	203	29	50
,, 8, 8 p.m.	320	34	225	23	45
,, 9, 8 a.m.	243	6	233	18	47
,, 10, 5 p.m.	213	34	13	12	60
,, 11, 2 p.m.	245	34	21	14	70
,, 12, 8 a.m.	340 ?	16 ?	47	13	?
,, 12, 5 p.m.	341	18	8	22	?

The changes in the direction of the cirrus and of the surface wind, as related to the line of maximum pressure, is shown graphically in the accompanying diagram. The line of maximum



pressure is indicated in each case by the long slanting line. The arrows fly with the cirrus and with the wind, and the length of the arrows indicate the velocity, though on a different scale in the two cases. The small figures near the arrows give the dates of observation.

Repeated observations of this kind, here and elsewhere, ought to throw some light on the causes of cyclones and anticyclones. If an indraught prevails at the top of the anticyclone of the same nature as the indraught at the earth's surface in a cyclone, it seems difficult to avoid the conclusion that there is an area of low pressure in the upper air above anticyclones, notwithstanding the fact that studies of mountain observations by Hann and others lead to an opposite conclusion. In the present case the inward gradient above appears not to have extended entirely to the outer limit of the anticyclone as indicated by the observations on the 12th.

Direct observations of the anticyclonic inflow must, however, be rare: first, because of the infrequency of cirrus in the proper positions, and the general absence of exact methods of measuring the slow motions observed; second, because there is usually a strong eastward drift in the upper air, which greatly interferes with the anticyclonic circulation, and generally overrides it, so that it only becomes strongly marked under stagnant conditions of the general atmosphere; third, the upper air isobars are usually distorted by strong contrasts of temperature in the area of the anticyclone. But notwithstanding these drawbacks, I am confident that with the increasing attention given to cloud observations, cases like the present will be frequently

observed. With a great many observations the anticyclonic inflow can be brought out by a system of averaging, as shown in the *American Meteorological Journal* for August 1893.

H. HELM CLAYTON.

Blue Hill Meteorological Observatory, June 17.

Effects of a Lightning Flash in Ben Nevis Observatory.

WHENEVER a thunder-storm passes the summit of the Ben, there occurs almost invariably a discharge from metallic bodies in the Observatory, as the cloud is passing away. A flash of greater or less extent is given off the stoves, accompanied by a sharp crack. In January 1890 there was an exceptionally severe flash; "one of the observers was almost knocked down when sitting writing, and the telegraph wire was fused, and all communication stopped for five days." But more destructive than any previous flash was that which occurred this year on June 19, when the Observatory narrowly escaped being destroyed by fire. Between two and three o'clock on that afternoon, repeated clicks on the telegraph instrument were heard by one of the assistants who was sitting in the office; he had been carefully noting the times at which the clicks occurred, when suddenly the whole office was filled with a brilliant flash and deafening roar. A pillar of smoke was discharged from the telegraph instrument and from the stove-pipe, filling the room. So severe was the flash that the assistant, who was quite deafened by the report, thought that his hair had been singed. A second slighter discharge took place immediately after, when the writer had entered the office to commence the fifteen hours observations. The discharge hurled two boxes and a small picture, that were in the vicinity of the lightning protector, across the kitchen, and blew off the button and outer casement of the electric bell in the visitors' room. The solder on the kitchen chimney outside, a copper fastening of the lightning conductor, and many portions of the telegraphic wire and apparatus were fused, and the woodwork of the Observatory was scorched in several places. The great flash occurred at 14 hours $57\frac{1}{2}$ minutes, and the hourly barometric reading was taken at 15 hours, as usual. There was a very heavy fall of snow at the time, equivalent to 0·470 inches of rainfall for the hour, but in the confusion the writer omitted to take the rain-gauge with him, and had to return for it. This was a fortunate incident; for it was only on leaving the office for a second time, that he observed smoke and flame issuing from behind the panelling between the kitchen and the office. Assistance was secured, and the fire—which was in a very awkward and dangerous place—was overcome in good time, and the damage done was very slight.

The damage done to the telegraphic apparatus, however, was serious, and Mr. Crompton, engineer of the Post Office telegraphs, has supplied me with the following information.

The lightning protector was badly fused, the plates showing a patch of fusion as large as a sixpence. This saved the cable from serious damage. All connecting wires within the building were rendered useless. The majority were so heated as to melt the insulation off, and, in one or two cases, the copper conductors were melted by the discharge. In one case, the fusion set fire to the woodwork.

The coils of Neale's sounder were fused and rendered useless. The keys suffered worst of all, the left pedal or "tapper" bearing the strongest evidence of the severity of the discharge. The back contact (platinum), the brass extension holding the same, and the steel spring (platinum-tipped) above, all being fused into one solid amalgam. The pillar, to which the zinc leading wire from the battery was connected, had a large patch of fusion near its base, and the front platinum contacts of the same (left-hand) pedal were consumed entirely. There were small traces of fusion on the right pedal, but of a trifling character. The line wire connected to the left-hand terminal of the key had been fused close to the terminal. The interior of the instrument case was considerably blackened, as also the greater portion of the keys, as a result of the "arc" caused by the discharge at the moment of fusion.

The vacuum protector at Ashintee, the base of the cable, also the plate protector in Fort William Post Office, were fused, but only slightly, the main discharge having expended itself on the summit. The Low Level Observatory instrument and protector were unimpaired, and communication between there and Fort William Post Office was carried on as usual after the removal of the fault in the Post Office protector.

The registering aneroid shows a slight upward kick at the time, but the curve is otherwise fairly steady; the temperature was 31·7 F., the wind south-south-east and light. Heavy snow was falling at the time, which, with a fall on the 17th, made a total depth of nine inches on the summit. St. Elmo's Fire was very strongly felt and heard until after seventeen hours.

WILLIAM S. BRUCE.

The Kinetic Theory of Gases.

IT seems to me that Mr. Burbury's and Prof. Boltzmann's last letters will enable us to reconcile all the *main* differences of opinion which were brought to light in our recent correspondence in the columns of NATURE. From Prof. Boltzmann's letter it appears that the Minimum Theorem can only be applied with absolute certainty to gases whose molecules are not too closely crowded together. Thus the proof that an aggregation of molecules tends *continuously* towards the Boltzmann-Maxwell distribution depends quite as much on assumptions as to the mixing of the molecules *between* collisions as on consideration of what happens *at* collisions. We cannot prove for certain that densely crowded assemblages of molecules such as solids and liquids tend to assume this distribution, and this is just as it should be, for when a substance is capable of existing simultaneously in two states, the distribution cannot be unique. For the same reason the proof does not apply to molecules moving about in a *continuous* medium such as the ether. So far from this limitation being a weak point in the proof, it precludes the theorem from proving too much, or from leading to results which may not accord with experience.

If we do not know that solids and liquids satisfy the Boltzmann-Maxwell distribution, we, nevertheless, know that they are subject to the Second Law of Thermodynamics. It cannot be said that any dynamical "proof of the Second Law" that has yet been given, is so conclusive as the mere statement of the Law itself, but the proof of the Minimum Theorem subject to "Condition (A)" leads to a result somewhat analogous to the statement that when two or more bodies at unequal temperature are brought into thermal contact, their entropy tends to increase. For let the probability of the coordinates on momenta of the molecules of one body lying between certain limits be proportional to F (*all* the coordinates and momenta being included in the multiple differential by which F is multiplied). Let the corresponding probability for a second body be proportional to f. Then when the two bodies are placed in thermal contact, we know of no relation connecting the two simultaneous probabilities, and we may therefore assume them to be independent, so that condition (A) is satisfied, at any rate initially. The theorem then asserts that at all subsequent instants of time, the value of the Minimum Function will be not greater than its initial value, and therefore it either remains stationary or decreases every time the process is repeated. Thus far we can get if no further.

The application of the Second Law depends largely on the distinction between *available* and *unavailable* energy. When we construct a thermodynamic engine for converting heat into work, we introduce just the kind of external disturbances that Mr. Burbury requires every time that the "working substance" is placed in contact with either the "source" or the refrigerator."

G. H. BRYAN.

An Abnormal Rose.

I HAVE in my garden at Reigate a *white* Moss rose-tree, every blossom on which is white except one which is half white and half red, divided diametrically in nearly equal portions.

The colours are not shaded one into the other, but are perfectly distinct, and one petal is half red and half white, the edge of the colouration being quite sharp.

I am told that one similar blossom was produced earlier in the season.

I imagine this is an attempt to revert to its ancestral colour, but by what mechanism such a partial result has been accomplished seems difficult to understand.

NEWNHAM BROWNE.

THERE are several varieties of rose that sport or revert in the manner described by Mr. Newnham Browne. The "York and Lancaster" rose is a familiar example. In this, the recognised or genuine condition is red and white striped; but the proportions of white and red are rarely exactly the same in any two flowers

on a bush, and very frequently some are wholly red and some, perhaps, wholly white, though I am not sure on this point. Many other cross-bred plants exhibit this inconstancy, which is supposed to be due to an imperfect blending of the elements of parentage. That the sporting is irregular and inconstant is not to be wondered at, when we consider that a plant is not an individual in the sense of possessing only one set of organs. Any vegetative bud of a plant is capable of producing any and all of the organs of the whole plant, or, if detached from the parent plant, to develop into a similar organism, with all its attributes. Given, then, a cross-bred variety, which is not constant, or "fixed," as florists term it, any vegetative bud may give rise to the cross or to one or the other of the parents.

W. Botting Hemsley.

Mineralised Diatoms.

Nearly twenty years have elapsed since you allowed me to announce in Nature the unexpected discovery of mineralised diatoms in the London clay of Sheppey.

Subsequent investigations demonstrated the existence of these unique microscopic fossils on the same geological horizon at several widely separated localities in the south-east of England; leading to the assumption that the band of diatomiferous earth was continuous throughout the formation.

Herne Bay was one of the places at which, in accordance with expectation, search was followed by success. Revisiting this place, a few days ago, for the first time since the discovery, I readily found the fossil diatoms as abundant as before in some recently fallen blocks of clay about half-way between Herne Bay and Oldhaven Gap. As there has been much waste of land at this spot during the interval, it is interesting to observe the presence of these diatoms in the newly exposed clay, giving support, as it does, to the hypothesis of their general distribution at a definite level throughout the London clay.

Perhaps some readers of Nature may be going to that part of the coast before long, and will then take the opportunity of verifying my observations.

W. H. Shrubsole.

SIR JOHN LUBBOCK AND THE TEACHING UNIVERSITY FOR LONDON.

THE address in which Sir John Lubbock solicits the suffrages of the Electors of the University of London has aroused feelings of surprise and regret among the friends of higher education in London, owing to the unfortunate nature of the references made to the Teaching University question. Six paragraphs out of ten are devoted to this important subject, and it seems almost incredible that so far from recognising that the Gresham Commissioners' scheme has enlisted a considerable measure of support in the University (*cf.* vol. l. 269; li. 298), Sir John Lubbock refers only to the views of its opponents, and, in accepting them, makes the remarkable statement:

"Feeling that Convocation ought to be consulted on a matter so vitally affecting the University, I would strongly urge, and do my best to secure, that the scheme when arranged should be submitted to Convocation for their approval, to be signified as at a Senatorial Election, and would oppose the Bill unless this were conceded."

Now it must be borne in mind that the Report of the Gresham Commissioners has met with a degree of approval from educational authorities and institutions, which not only far exceeds that extended to any previous attempt to solve the vexed question of University reform in London, but has been sufficiently unanimous to lead to the introduction of the "University of London Act, 1895," in the House of Lords by the late Government. This Bill, in accordance with the general tenour of the resolutions passed by the various institutions named in the Report as constituent colleges of the teaching University, enacted (clause iii. para. 1):

"The Commissioners will have power to make statutes and ordinances for the University of London in general accordance with the scheme of the Report hereinbefore referred to, but subject to any modifications which may appear to them expedient after considering any representations made to them by the Senate or Convocation of the University of London, or by any other body or persons affected."

And further (para. 2):

"In framing such statutes and ordinances, the Commissioners shall see that provision is made for securing adequately the interests of non-collegiate students."

Convocation in January last had the opportunity of exercising its veto in meeting assembled as provided by the Charter of the University on the scheme of reconstitution proposed by the Commissioners, which had previously received the general approval of the Senate. Instead of insisting on this right, it preferred to bring itself into line with the other institutions affected by the scheme, by adopting a resolution in terms almost identical with those employed in the Bill. Only so recently as May, it declined to reconsider this attitude by a majority of two to one, yet it is clear that the Bill, if again brought forward, is to meet with opposition from Sir John Lubbock, if re-elected, unless an amendment is inserted providing that the completed scheme shall be submitted to Convocation for approval in a manner expressly excluded under the terms of the present Charter, viz. by means of a referendum.

It is difficult to imagine by what process of reasoning this seemingly gratuitous proposal can be reconciled with the functions of a statutory, that is a judicial and executive, Commission. Convocation is but one of the bodies affected by the scheme, and in common with the others, it can, under the terms of the Bill, present its case for modifications in the scheme to the Commissioners before the statutes are framed, and like them can appeal against the statutes during the forty days they must lie on the table in both Houses of Parliament before they become operative. Such an amendment could only have the effect of wrecking the latest and most satisfactory scheme of University reform, since no other institution affected by the scheme could be expected to agree to such an unprecedented proposal. Nor is it likely that any person fitted to occupy the position would consent to serve on the Commission, and devote his time and best energies to the difficult and delicate work of adjusting the relations between these institutions, with the knowledge that the statutes and ordinances eventually framed would be subject to the approval of any irresponsible, non judicial body, let alone one of the institutions closely affected.

For the most part, Sir John Lubbock has held aloof from the controversy on the Teaching University question. Once only does he seem to have taken sides. It is on record that he voted with the majority when the Senate in June of last year passed a resolution expressing general approval of the proposals of the Gresham University Commission, with which action his present attitude is wholly inconsistent. It would be interesting to know whether his descent on the other side of the fence is in any way connected with the absence of opposition to his candidature on the part of the opponents of the scheme. Be this as it may, this uncalled for proposal to subordinate the interests of higher education in London to the pleasure of Convocation, ascertained not after debate, but by a referendum, is not to pass without protest, and we are glad to note that the following letters have already appeared in the press. The first is from Prof. Michael Foster, Sec.R.S., and President of Sir John Lubbock's Parliamentary Election Committee.

"Shelford, Cambridge, July 4, 1895.

"Dear Sir John,—As you know, I am wholly opposed to your view that the scheme for the University of London to be proposed by the Statutory Commissioners ought to be submitted to Convocation for approval. You also know that this difference of opinion, important as it is, does not prevent my desiring that you should continue to

represent the University of London in Parliament. I find, however, that your letter addressed to me is understood to show that I agree with all the opinions expressed by you in that letter, and in justice to myself I must make known to my fellow electors and others how wholly we disagree on the above point, and how much I regret the attitude you assume in the matters in question.

"Yours very truly,

"Sir John Lubbock, Bart." "M. FOSTER.

The second has been addressed to Sir John Lubbock by the President and a number of Fellows of the Royal Society:—

"July 6, 1895.

"Dear Sir John Lubbock, The interests of learning and of education are so closely bound up with the future development of the University of London that we hope you will not regard us as interfering between yourself and the Electing Body of the University if we venture to express our regret at some of the opinions you have put forward in your Election address.

"You state that you would do your best to secure that the scheme for the reorganisation of the University, when arranged, should be submitted to Convocation for their approval, to be signified as at a Senatorial Election, and would oppose the Bill unless this were conceded.

"You must allow us to point out that this proposal would confer upon Convocation a right, which is without precedent, to supervise the acts of a Commission entrusted with the reorganisation of the University of which Convocation itself is a part.

"The scheme of the 'Gresham Commissioners' has been approved not only by all the institutions concerned, but by the great body of educated public opinion. It is, however, certain that very grave difficulties will arise if the ultimate fate of the scheme is to depend upon the voting papers of Convocation.

"We, therefore, believe that the proposal you support, if adopted, will result in the failure of another attempt to establish a Teaching University in London, and will indefinitely postpone the solution of a question which, after prolonged discussion, seemed to be on the eve of settlement.

"We are, yours faithfully,

"KELVIN (P.R.S.), JOHN EVANS (Treas.R.S.), M. FOSTER (Sec.R.S.), JOSEPH LISTER, RAYLEIGH, DOUGLAS GALTON, T. G. BONNEY, T. E. THORPE, HORACE LAMB, J. H. POYNTING, ARTHUR W. RÜCKER, E. FRANKLAND, N. STORY MASKELYNE, HENRY E. ROSCOE, P. H. PYE-SMITH, J. NORMAN LOCKYER, JOHN ERIC ERICHSEN, WILLIAM RAMSAY, G. CAREY FOSTER."

In his address, Sir John Lubbock states that the opinions of the present Government on the University question have yet to be made known. In view of the fact that the Commission, whose report has been so generally approved, was appointed during Lord Salisbury's last term of office, this attitude ought not to be doubtful.

THE ELECTRICAL MEASUREMENT OF STARLIGHT.

THAT the light of a star is able to produce at the surface of the earth a measurable effect, other than the action on a photographic plate, is a fact which was published in these pages in January last year. The light of stars and planets produces two effects—the one photographic and the other electric. The first (which has, of course, been known for many years) is slow in its operation; the second (which was discovered only a year ago in Mr. Wilson's observatory at Daramona, Westmeath) is almost instantaneous.

In order to obtain the electrical effect, a photoelectric cell of extremely great sensitiveness to light is employed. Such a cell is constructed with selenium, aluminium, and the liquid œnanthol. If we take a strip of clean aluminium (say half an inch long, one-tenth of an inch wide, and thick enough to be fairly stiff) lay it on an iron plate which is heated by a Bunsen flame, and place on the end of the strip a very small particle of selenium, this selenium will melt and form a small black globule of liquid. Let the flame be now withdrawn, and the globule of melted selenium spread over the end of the aluminium strip, by means of a hot glass rod, so that it forms a thin uniform layer of area about ·1 of an inch square on the end of the strip, and let this dark layer cool to a few degrees below its melting point (about 217° C.). Now apply heat again to the under surface of the iron plate until the aluminium strip becomes nearly hot enough to re-melt the layer of selenium. In this process the colour of the layer will gradually change from black to a greyish brown. When it is just on the point of melting, withdraw the heat and blow over its surface; this will instantly check the tendency to melt, and will leave the surface of the selenium in the state in which it is most sensitive to light. If this strip (or rather its selenium-covered end) is immersed in a glass tube containing acetone or œnanthol, and connected with one pole of a quadrant electrometer, whose other pole is connected with a platinum wire sealed into the glass tube, we have a photoelectric cell, in which the action of light falling on the selenium layer results in giving the selenium a positive electric charge and the liquid a negative one, the former charge being conveyed to one pole of the electrometer by the aluminium plate, and the latter to the other pole by the platinum wire sealed into the cell.

Roughly speaking, the difference of potential produced in such a cell as this by ordinary diffused daylight is something between one-third and one-half of a volt.

Such were the seleno-aluminium cells used in the measurement of starlight in January 1894, the liquid in them being œnanthol. This liquid was found to be better than acetone (which had been previously used), not only because of the greater ease with which it can be sealed up in glass tubes, but because it does not act chemically on selenium, which acetone seems to do sooner or later. But it is obvious that a cell formed in this way contains an element of inconstancy; for, the strip of aluminium will at the same time convey to the insulated pole of the electrometer the positive charge generated by light in the selenium and a portion of the negative charge imparted to the liquid, so that the effective E.M.F. is less than it should be; and, again, there will be currents circulating perpetually between the selenium and the back of the aluminium strip, and such currents deteriorate the cell. Hence it happened that such cells always fell off in strength after about six hours. They sufficed, however, to show very easily measurable electromotive forces from the light of the planets, and even from the light of Sirius.

Shortly after January 1894, a very notable improvement was made in the construction of the cells, this improvement resulting from the perception of the cause of deterioration above explained. Instead of a strip of aluminium as a base for the selenium layer, the end of an aluminium wire, about one millimetre in diameter, was used. This wire was enclosed in a glass tube (A, B, in the figure on p. 247), into which it fitted tightly, one end of the wire being flush with an end of the tube. On this end was deposited the layer of selenium, with the same process of heating as that already described. The other end of the aluminium wire inside the glass tube was connected with a fine platinum wire, P, which emerged from the second end of the tube, and which formed the selenium pole of the photoelectric cell.

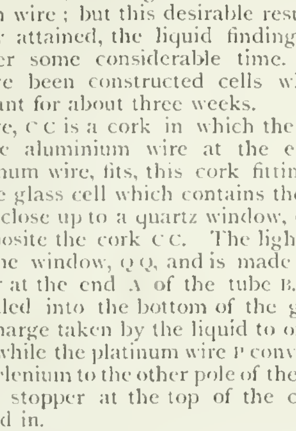
In this way the liquid is kept out of contact with the

aluminium wire, and the deteriorating local currents in the cell are avoided, if the glass tube *exactly* fits round the aluminium wire; but this desirable result has not yet been perfectly attained, the liquid finding its way into the tube after some considerable time. However, in this way have been constructed cells which have remained constant for about three weeks.

In the figure, C C is a cork in which the glass tube, B, containing the aluminium wire at the end A and the attached platinum wire, fits, this cork fitting tightly into the side of the glass cell which contains the liquid. The tube B passes close up to a quartz window, Q Q, cemented to the cell opposite the cork C C. The light of the star is received on the window, Q Q, and is made to fall on the selenium layer at the end A of the tube B. A platinum wire, P′, is sealed into the bottom of the glass cell, and conveys the charge taken by the liquid to one pole of the electrometer, while the platinum wire P conveys the charge taken by the selenium to the other pole of the electrometer; S is a ground stopper at the top of the cell, where the liquid is poured in.

This cell is fitted into a holder which can be fixed to a telescope in place of the eyepiece; and this cell-holder allows of the adjustments which are necessary to bring the point A to the position of the image of a star.

This is the form of photoelectric cell with which, in conjunction with Prof. Fitzgerald and Mr. W. E. Wilson, I measured the electromotive forces of the lights of



Jupiter, Saturn, Vega, Arcturus, Regulus, Procyon, and some other stars last April, in Mr. Wilson's observatory at Daramona, Westmeath. The telescope used was Mr. Wilson's 2-feet reflector.

In order to give a notion of the sensitiveness of the cell to light, I may say that if an ordinary paraffin candle is held at a distance of 9 feet from the window Q Q, it will produce an electromotive force of about ·03 volts; or, to put the matter differently, suppose an ordinary quadrant electrometer, of Clifton's pattern, charged so that a Daniell cell gives a deflection of 400 divisions on the ordinary scale (placed at a metre distance); then the light of the candle at 9 feet falling on the photoelectric cell would give a deflection of twelve divisions, and the deflection varies inversely as the distance of the candle.

Now the light of Vega as concentrated in the 2-feet telescope gives a slightly greater deflection than the (of course unconcentrated) light of the candle; so that we are evidently dealing with easily measurable quantities.

The cell is sensitive to all the rays of the spectrum, but the maximum effect is produced by the yellow. It is sensitive to rays considerably below the visible red and beyond the blue.

The light of Arcturus was found to give 0·82 of the E.M.F. produced by the candle at 9 feet; the light of Saturn 0·56, which was also about the value of the light of Regulus. Unfortunately neither Sirius nor Capella, nor any star in Orion, nor any in the Great Bear, was available for our observations; but these we hope to include, before long, in the list of measured stars.

It will be observed that in this electrical measurement of starlight we do not measure *currents*, but *electromotive forces*—we do not use a galvanometer, but an electrometer; and an electrometer of small capacity was specially constructed for these experiments, with the aid of the Government grant dispensed by the Royal Society.

It is not desirable to allow the light to generate *currents*: the electrical charges must be allowed to flow back into the cell, so that it may not be temporarily deteriorated during the observations. Hence the preference for the electrometer.

The space at my disposal will not allow of my entering into many details; but I may mention, in particular, the importance of having the whole of the sensitive surface in the cell covered by the light of the star. It matters not to the value of the E.M.F. produced how far behind the focal image of the star the sensitive surface, A, is placed—provided that the image of the star just covers the surface A. This is essential in all photoelectric cells, and also in thermopiles; and the neglect of this condition may partly explain the failure of attempts to obtain thermoelectric indications from the stars and planets, although we should scarcely expect success from methods which aim at measuring merely a very limited portion of the radiation (viz. the heat, or infra-red). The photoelectric cell integrates the whole energy of the radiation on the sensitive surface; and the *square* of the observed E.M.F. is the measure of this incident energy.

It is interesting to know how the *photelectric* measures, so far as they have gone, compare with the *photometric* measures of "magnitudes" hitherto employed by astronomers. In the latter, if B and B′ are the "brightnesses" of two stars of the magnitudes m and m' respectively, we have by definition

$$\log_{10}\frac{B}{B'} = \frac{4}{10}(m' - m), \quad \ldots \ldots \quad (1)$$

This equation defines merely the difference of the magnitudes, and the definition is quite arbitrary. The essential things are B and B′. How are they measured? The photoelectric method says that they are E^2 and E'^2, the squares of the electromotive forces generated in a given cell by the lights of the two stars. The photometric method says that they are measured by the thicknesses of certain interposed glass prisms which extinguish the lights, or by polarising apparatus which render the shades of the transmitted lights "equal." Hence we may expect, perhaps, a fair amount of agreement between the two methods, if we are comparing two or more stars of the same colour. Thus, in the photoelectric method, we have for any two stars

$$m' - m = 5\log_{10}\frac{E}{E'}, \quad \ldots \ldots \quad (2)$$

Applying this to Arcturus and Regulus, and taking the magnitude of the former as ·2, we find the magnitude of Regulus to be 1·33. In Miss Clerke's "System of the Stars" (Appendix), Regulus is quoted as 1·4, Arcturus being ·2.

Comparing in the same way Procyon and Regulus, the latter being taken as of magnitude 1·33, the magnitude of Procyon would be ·46. Miss Clerke quotes Procyon as of magnitude ·5.

But no agreement between the two methods is to be expected when two stars of different colours are compared. The photometric method of equalisation seems to be just as meaningless as the ordinary "grease-spot" method of attempting to equalise a blue and a red light! In this case the only intelligible comparison of two lights consists in measuring the energies which they radiate per

unit time per unit area at a given distance—just, for example, as Newton's Second Axiom defines two masses to be "equal" when the same force produces the same acceleration in both; an equality which is *real* if the substratum at the basis of all bodies is the same, but merely *conventional* if it is not.

If the distance of a star is known, we can determine its intrinsic energy, *i.e.* the quantity of energy which it radiates into all space per unit time.

Thus, let I be the intrinsic energy of a star whose distance from the earth is R; let E be the electromotive force of its light as measured by the cell; let i, r, e be the analogous quantities for a candle or any other chosen source of light; and let A and a be the areas of the aperture of the telescope and the selenium surface in the cell. Then we have

$$\frac{I}{i} = \frac{R^2 . E^2 . a}{r^2 . e^2 . A} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let us take, for example, a result which Prof. Boys recently told me that he had obtained. He found, in conjunction with Mr. Watson, of South Kensington, that if the light of a standard candle was observed across a valley and almost in the line of sight of Arcturus, the light of the candle and that of the star seemed to be equal when the candle was at a distance of five-eighths, or ·625, of a mile.

Now, let x be the distance at which the candle light seems to be as bright as that of the star. Then

$$\frac{I}{R^2} = \frac{i}{x^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

And if D and d are the diameters of the telescope aperture and the circular layer of selenium in the cell, we have from (3)

$$x = r\frac{e . D}{E . d} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Put, now, $r = 9$ feet, $e = 10$, $E = 8·2$, $D = 24 \times 25$ millimetres, $d = 2$ m.m., as in our experiments, and we find

$$x = 3300 \text{ feet, nearly}$$
$$= ·62 \text{ miles.}$$

This agrees remarkably well with the observation of Prof. Boys. George M. Minchin.

FUNERAL OF PROFESSOR HUXLEY.

In accordance with his own wish, the late Prof. Huxley was buried at the Marylebone Cemetery, Finchley, last Thursday afternoon. The coffin came up from Eastbourne in the morning, and the numerous mourners assembled at the cemetery to meet it. Wreaths from members of the family, and from friends and fellow workers of the great naturalist whose loss we mourn, covered the coffin. The Royal College of Science, with which Huxley was connected so many years, sent a large wreath, and there were also wreaths from Lady Hooker, Mrs. Tyndall, the members of the staff at the Royal Gardens, Kew, Mr. Herbert Spencer, Sir Henry Thompson, Sir Henry Roscoe, Messrs. Macmillan, and the Editor of Nature, among others.

The funeral service was performed by the Rev. J. Llewelyn Davies, an old friend of Prof. Huxley's, now rector of Kirby Lonsdale, but formerly vicar of Marylebone, where he was for a long time Huxley's neighbour.

The family was represented by Mrs. Huxley, the two sons, Mr. Leonard Huxley and Mr. Henry Huxley, and three daughters, the Hon. Mrs. Collier, Mrs. Waller, and Mrs. Eckersley (the remaining daughter, Mrs. Roller, is in Switzerland with her husband, who is ill), Mrs. Heath (a niece), and two sons-in-law, the Hon. John Collier and Mr. I. W. Waller.

No announcements of the funeral were sent out, and the large number of distinguished men who attended, and the various learned Societies that sent representatives, did so on their own initiative. The Royal Society was officially represented by Lord Kelvin, Sir John Evans, Prof. Michael Foster, and Sir J. Lister, many of the Fellows also being present. The Geological Society was represented by Dr. Henry Woodward, Dr. Blanford, and Prof. Bonney. Dr. Frankland, Mr. Crookes, Dr. Thorpe, and Dr. Gladstone were the representatives of the Chemical Society. The mourners from the Royal College of Science included Prof. Rucker, Prof. Norman Lockyer, C.B., Prof. Tilden, Prof. Judd, C.B., Prof. W. C. Roberts-Austen, C.B., Prof. Howes, Prof. Farmer, Dr. Wynne, Mr. J. W. Rodger, and Mr. Woodward. Major-General Sir J. F. D. Donnelly, K.C.B., Major-General Festing, Captain Abney, C.B., Mr. T. Armstrong, Mr. F. R. Fowke, and Mr. A. S. Cole represented the Science and Art Department; Sir William Flower, K.C.B., Dr. A. Gunther, Mr. George Murray, Mr. C. E. Fagan, Prof. Jeffrey Bell, and Mr. F. A. Bather, the Natural History Museum; Prof. Armstrong, Prof. S. P. Thompson, Prof. Perry, and Prof. Ayrton, the City and Guilds Institute; Mr. Stanley Boyd, Mr. H. F. Waterhouse, Mr. J. F. Pink, the Charing Cross Hospital Medical School; Mr. J. J. H. Teall, Mr. F. W. Rudler, and Mr. E. T. Newton, the Geological Survey. In addition to the Fellows of the Royal Society not included in the above, there were present Prof. E. Ray Lankester, Dr. Dallinger, Sir Joseph Hooker, K.C.B., General Strachey, Dr. Lauder Brunton, Dr. Sclater, Prof. Carey Foster, Prof. G. H. Darwin, Sir James Paget, Dr. Burney Yeo, Prof. H. Marshall Ward, Prof. Seeley, and Mr. F. Darwin. Among the other mourners were Mr. Walter Troughton, representing Mr. Herbert Spencer, who was prevented by illness from being present, Dr. T. K. Rose, Mr. W. Darwin, Mr. A. H. Heath, Mr. S. Highley, Mr. W. S. Stewart, Major-General Sir Richard Pollock and Mr. D. Pollock, Mr. Alma Tadema, Mr. W. E. H. Lecky, Mr. and Mrs. Humphry Ward, Mrs. Tyndall, Mrs. W. K. Clifford, Mr. Henry James, Mr. Mark Judge, Mr. H. Saunders, Dr. Semon, Mr. F. Macmillan, Mr. G. L. Craik, Mr. Clodd, Mr. G. Griffith, Lady Staveley Hill, Mr. Paynter Allen, Mr. John Boyes, Mr. Spencer Walpole, Mr. Woodd Smith, Dr. J. Johnson, Mr. James Hulme, Mr. Stanley Edwards, Dr. Glover, Mr. T. B. Windsor, the Rev. D. D. Jeremy, Dr. J. Malecki, Mr. J. Spiller and Mr. and Mrs. Briton Rivière.

The funeral was at first announced to take place at 3 o'clock, whereas the time fixed upon was 2·30. Owing to a delay in the train, a number of workers in science, from the Midlands and the North of England, did not arrive at the cemetery until the ceremony was over, and thus, to their deep regret, they were deprived of the melancholy satisfaction of being present when the remains of an esteemed master and friend were laid to rest.

The memory of Huxley will always be cherished among men of science, and it is imperative that there should be a permanent memorial of some kind to show the world how great is their regard for him. The memorial should be a truly national one, and not limited to any particular institution. We understand that the Dean of Westminster is willing that a tablet shall be erected in the Abbey if desired, and this is one of the forms which the memorial might take. Sir William Flower suggests another form, in a letter to the *Times* of Monday. He writes:

"In the great hall of our national Museum of Natural History the noble statue of Darwin will hand down to posterity the image of the man as he appeared to all who knew him in life. Near this will soon be placed another statue remarkable for the accuracy with which the striking personality of Owen is represented, as all who see it now at the Royal Academy Exhibition can testify.

Surely this group of the great naturalists of this country and this century must be completed by the one we have just lost, in some respects the greatest of the three. The statues of Pitt and Fox stand side by side in Westminster Abbey. Huxley and Owen, often divided in their lives, would here come together after death in the most appropriate place and amid the most appropriate surroundings."

What is now wanted is a representative committee to take the matter up; we are confident that an appeal for funds would meet with a ready response, and we are glad to know that steps are being taken in this direction. A circular signed by Dr. Foster and Sir William Flower has been issued, calling a meeting at the rooms of the Royal Society this afternoon.

NOTES.

THE meeting at which the Prince of Wales presided in St. James's Palace on Tuesday, ought to further the interests of the British School at Athens, in support of which it was held. A distinguished and representative company was present, among them being many well-known men of science. The Prince of Wales has concerned himself with the existence and welfare of the School from the time of its foundation in 1883, and we are glad to notice that in his remarks to the meeting he drew attention to the fact that the scantiness of the means provided was out of all proportion to the valuable archæological work carried on. The School only has a precarious annual income of £500, whereas the French School at Athens has an assured income of over £3000 a year, and the German School more than £2000 a year. Owing to this state of affairs, it is quite impossible for the British School to enter into competition with such undertakings as the explorations of the Germans at Olympia, the French at Delphi, the Americans at Argos, or the Greeks at Eleusis and Epidaurus. The sum required to bring England approximately into line with other nations is at least £1500 a year. Fortunately, as the Prince of Wales remarked at the meeting, there are hopeful signs that matters will soon be placed on a more satisfactory footing. A petition for support addressed to the late Government, met with a ready response; and before leaving office Sir William Harcourt took steps to use some portion of the public funds devoted to the encouragement of scientific investigation for the support of the School, and it is understood that the present Ministers are willing to confirm the action of their predecessors. One of the colleges at Cambridge, which has been most severely tried through the agricultural depression, has generously made an annual appropriation out of its reduced funds, and three colleges at Oxford have voted annual grants. The public schools are also moving in the matter. The Prince of Wales suggested that perhaps some of our City Companies, whose funds are devoted not only to local charities, but which have extended their sphere to the support of educational and scientific institutions, may see their way to encourage research in Greece; and he hoped that our colonies, which are so intimately bound up with our own culture and our higher national aspirations, will recognise the fact that all the privileges of the Athens School are open to their qualified students, and will make some effort towards securing its adequate efficiency. Lastly, he appealed to the liberality of private individuals, and expressed himself convinced that the appeal would find a response throughout the country. Every year excavation, both in Greece and elsewhere, is becoming more important to science. The following resolutions, confirmatory of the object of the meeting, were carried unanimously:—(1) "That the British School at Athens has already done excellent work during the nine years of its existence, and is well deserving of increased support." (2) "That this meeting pledges itself to use every effort to place the School upon a sound financial basis, so that in point of dignity and efficiency it may worthily represent this country among the other foreign institutes in Athens."

PROF. CURTIUS, of the University at Kiel, has been appointed successor to the late Prof. Lothair von Meyer at Tübingen.

PROF. DANIEL C. EATON, well known in botanical circles by his work on ferns, has just died at New Haven, U.S.

WE learn that M. J. Deby, one of the leading authorities on diatoms, whose magnificent collection was recently acquired by the British Museum, is dead. He was in his seventieth year, having been born at Lacken, in Belgium, in 1826.

To the list of honours given last week should have been added Sir Bernhard Samuelson, M.P., F.R.S., who has been made a Privy Councillor, and Dr. H. D. Littlejohn, who has been made a knight. On Thursday last, Mr. Thornley Stoker, President of the Royal College of Surgeons in Ireland, and Dr. Christopher Nixon, were knighted by the Lord-Lieutenant of Ireland.

THE date of the annual meeting of the Society of Chemical Industry, which is this year to be held in Leeds, has been postponed from July 17 to July 31, in consequence of the General Election. It is not thought that any material change will have to be made in the programme.

PROF. SCHWARZ has been elected a Correspondant of the Paris Academy, in the Section of Geometry; Baron von Müller has been elected to the late Prof. Pringsheim's place in the Section of Botany, and Prof. Engelmann succeeds Ludwig in the Section of Medicine and Surgery.

WE are glad to be able to announce that the Italian Meteorological Society, which was temporarily dissolved after the death of Padre Denza, has again been reorganised, under the presidency of Count Vigodarzere, who is the proprietor of an observatory at Fontaniva. The central observatory will be at Moncalieri, as before, and we look forward to a continuation of the useful work carried on formerly by the Society.

WE are informed that King's College, London, will open next October a department for training teachers for Secondary Schools. There will be a two-years' course of technical studies combined with the preparation for the B.A. degree of the University of London. Detailed instruction in the art of teaching particular subjects will be given by the Professors of the College. Six Exhibitions of £15 are offered. Names of students should be sent in before September 16.

A REUTER correspondent at St. John's reports that the steamer *Kite* left there for Greenland on Tuesday to bring home the Peary Arctic Expedition. It is expected to return on October 1. The party on board includes Prof. Salisbury, of Chicago University, who goes to study the glaciers and geology of the region; Prof. Dyche, of the State University, Kansas, who will collect specimens of the fauna and flora; and Mr. Boutillier, of Philadelphia, who represents the Geographical Society.

THE influence of the Royal Gardens at Kew is felt in widely different regions of the world, through the men who are trained at the Gardens and sent out to various Botanic Stations. Three new appointments of men who have benefited by the Kew training, are notified in the current Kew *Bulletin*: they are Mr. C. H. Humphries, who has been made Curator of the Botanic Station of Aburi, on the Gold Coast; Mr. J. C. Moore, who has been appointed Curator of the Botanic Station at St. Lucia, in the Windward Islands, West Indies; and Mr. H. McMillan, who goes as Head Gardener to the Royal Botanic Gardens at Peradeniya, Ceylon.

MR. A. B. BASSET has sent us a letter referring to the proposed changes in the size of the pages of the Royal Society's publications. He directs attention to chapter xii. section ii. of the Statutes

of the Society, empowering any six Fellows to convene a special general meeting, and suggests that such a meeting should be summoned, and the following resolutions submitted to it: (1) That this meeting is of opinion that the present form of publishing the *Transactions* should be continued. (2) That this meeting is of opinion that the present form of publishing the *Proceedings* should be continued. The resolutions are drawn up separately, so as to obtain the votes of Fellows who approve of a change being made in the form of one kind of publication, but disapprove of any change as regards the other.

IN the recent death of Prof. Verneuil, France has lost one of her most eminent surgeons. His name is intimately connected with the history of contemporary surgery. At first, Assistant of Anatomy, Prosector, as well as Professor of Anatomy to the Faculty of Medicine, he devoted himself to anatomical and physiological studies, and left his mark by important works, chiefly on the heart, and on the anatomy and physiology of the venous system. Later, he formed part of that noted phalanx which, under the auspices of Lebert, with Robin, Broca, Follin, introduced histological studies into France. From this time date a series of original memoirs, notably on the demoid cysts of the face, and on the scrotal enclosure, in which he expounded new views, and established the scientific theory which is now generally adopted. Later still, when hospital surgeon and professor in the Faculty of Medicine, he introduced important methods of operation. Animated by the most ardent love of science, he knew how to communicate his enthusiasm to those around him; he had all the requisite qualities of a founder of a school. His activity showed itself by a great number of communications to learned societies of which he was a member.

THE extensive science laboratories and buildings recently opened at Lille are described in detail in the *Revue Générale des Sciences*. The buildings comprise a physical institute, an institute of natural science, and an institute of chemistry, erected at a cost of £65,000. The cost of the whole work was nearly £140,000, and this has been borne by the Municipal Council and the Academy at Lille, assisted by a gift of £4000 from M. Philippart. The town of Lille has guaranteed an annual grant of £800 for twenty years, to be used in the interests of higher education, and has shown the greatest interest in the work of the new institute. The department of chemistry is divided into two parts, in which general chemistry and applied chemistry are respectively dealt with; and in each section laboratories are provided for research as well as for instruction. The physical department occupies a separate building, in which accommodation is provided for experiments of extreme delicacy as well as routine work. On account of the great stability now demanded by many physical investigations, all the research laboratories are on the ground floor; for the same reason, numerous large isolated pillars of masonry have been provided, and strong slate slabs have been fixed into the corners of the laboratories. The natural science building provides accommodation for geology, zoology, and botany; and a room is reserved for the Geological Society of the North of France. Every facility for study under good conditions appears to be offered by the new laboratories, and higher education in France will derive benefit from the increased opportunities now offered it at Lille.

THE Third International Agricultural Congress will take place at Brussels from September 8 to 16; hence it will clash with the meeting of the British Association at Ipswich, which begins on September 11. The Congress will be held under the patronage of the King of the Belgians, and embraces twelve sections. In the section of agricultural education the subjects for discussion include rural schools, fields for experiment and demonstration, the possibility of devising an international programme of superior agricultural study, and the professional training of farmers' sons by interchange of the young people of different districts. The section of agricultural science will embrace chemistry and physiology as applied to agriculture; the utilisation and conservation of natural manures; agricultural meteorology; experiment stations and laboratories of control for manures, foods, and seeds. The third, fourth, and fifth sections deal respectively with co-operation, legislation, and currency. The section of animal production will discuss practical questions relating to stock-breeding, selection and crossing, the improvement of breeds, and the feeding of stock in times of drought. The veterinary section will concern itself with the organisation of veterinary sanitary police and the contagious diseases of animals, including pleuro-pneumonia, anthrax, and tuberculosis. The section of plant production is to discuss the selection of seed, the cultivation of malting barley, "sidération," the cultivation of peaty and mossy soils, drainage, and irrigation. The ninth section—southern agriculture and colonisation—embraces grape and silk culture; the cultivation of flowers for perfume, of oil-yielding plants, and of coffee, tea, and sugar-cane; the agriculture of the Congo and of Tunis; and the conditions of countries to which emigrants might be sent. The tenth section takes in forest economy, the eleventh deals with pisciculture, and the twelfth with agricultural industries, such as dairying, brewing, and bee and poultry culture.

THE results of a competition organised at Paris last month, by the *Petit Journal*, are of some scientific interest. Sixty thousand carrier pigeons from all parts of France, and from some places in Belgium, were released from the Eiffel Tower at known intervals and times. The first pigeon travelled a distance of 150 kilometres ($93\frac{1}{4}$ miles), with a velocity of 76 kilometres (47 miles) per hour. The highest average rates of flight ranged between this and 43 miles per hour for a distance of 264 miles. These rates are low compared with previous records. A distance of 600 miles has been covered at an average rate of 50 miles an hour, and in June 1860, a pigeon travelled from Blois to Dijon, a distance of 290 miles, in 4h. 46m., which gives a rate of about 60 miles per hour. There is also evidence that much higher average velocities than these have been reached.

DR. J. HANN, Secretary of the Vienna Academy of Sciences, laid before it, on the 20th ult., an investigation on the daily range of the barometer on clear and cloudy days, especially on mountain summits. It was known that at ordinary stations the daily barometric range in clear and cloudy weather only exhibited a difference in the single daily oscillation, while the double daily oscillation remained unchanged. But a similar investigation for mountain stations had not yet been made. With this object the author undertook the tedious operation of calculating the daily barometric range at a number of mountain stations for the summer season, and found that at these the double daily oscillation remained the same in both kinds of weather. At the earth's surface the daily curve showed a much greater amplitude in clear than in cloudy weather, and a totally different epoch. The average form of the daily curve for the mountain stations is represented by the formula: $0.48 \sin (353^{\circ} + x)$ on clear days, and $0.26 \sin (101^{\circ} + x)$ on cloudy days. On clear days the maximum of the single daily oscillation occurs at 6h. 30m. a.m., while on cloudy days it occurs at 11h. p.m. The author also found that the differences in the daily range on clear and cloudy days corresponded entirely to the differences which exist over the land, as compared with those over adjacent seas.

A RETURN has been issued showing the number of licensed experiments performed on living animals during 1894. The total number of persons holding licences during the year was 185, and of these 56 performed no experiments. The tables given afford evidence that licences and certificates have been granted and allowed only upon the recommendation of persons

of high scientific standing, and that the licencees are persons who, by their training and education, are fitted to undertake experimental work and to profit by it. All the experimental work has been conducted in suitable places: the number of experiments performed was 3104. In more than one-third of these the animal suffered no pain, because complete anæsthesia was maintained from before the commencement of the experiment until the animal was killed. More than fifteen hundred of the remaining experiments were of the nature of hypodermic injections or inoculations. In about five hundred experiments the animal was anæsthetised during the operation, but was allowed to recover. These operations, in order to insure success, are necessarily done with as much care as are similar operations upon the human subject; and the wounds being dressed antiseptically, no pain results during the healing process.

The Geologists' Association will visit the coast of Antrim and the Mourne Mountains this summer (July 29 to August 3). The programme includes the examination of sections in sedimentary rocks ranging from the Ordovician to the Chalk, pre-Devonian gneisses, and the basalts, rhyolites, and drusy granites of the Tertiary eruptive series. The illustrative papers by Messrs. McHenry and Lloyd Praeger will shortly be issued as a pamphlet, in advance of publication in the *Proceedings*. The country to be visited is classic, and additional interest is added to it by the recent publication of two papers in the *Geological Magazine*. The first of these, in the June number, by Mr. McHenry of the Irish Geological Survey, describes valuable evidence as to the age of the trachyte (rhyolite) of the district. In a section at Templepatrick Quarry, the acid lava is seen, by the arrangement of its columnar and flow-structure, to have flowed over the surface of the Chalk, sweeping the overlying gravel before it, and piling it up against the denuded edge of a mass of basalt belonging to the *earlier* of the two basic series. As fragments of the trachyte occur elsewhere in gravels overlain by the *later* basalts, it may be said to be of "mid-basaltic" age. The second paper, in the July number, is by Prof. Cole, and deals with the nature of the acid rocks poured out from the Tardree volcano, which are said to equal in variety the better known rhyolites of Hungary.

The numbers of the *Botanical Gazette* for May and June contain a translation, by Mr. G. J. Peirce, of Prof. Strasburger's paper on the "Development of Botany in Germany during the Nineteenth Century." In the latter number there is also a very instructive article, by Mr. J. M. Coulter, on the "Botanical Work of the American Government." At present four distinct divisions of botanical work are organised under the Department of Agriculture, although other divisions also do a certain amount of work that may fairly be called botanical. These four divisions are those of botany, vegetable physiology, and pathology, agrostology, and forestry. The Division of Botany, under the general supervision of Prof. F. V. Coville, of Cornell University, is engaged in strictly scientific work, such as the working out of local floras, the examination of seeds, investigation of weeds, &c. To this department the Government appropriates, during the present year, 33,800 dollars. The division of vegetable physiology and pathology (26,300 dollars) is concerned with investigations into the phenomena of the growth of plants, and into the diseases of cultivated plants. Its chief is Prof. B. T. Galloway, University of Missouri: but investigations on behalf of the department are carried on also at the following centres:—University of Nebraska, University of Michigan, University of Illinois, Kansas Agricultural College, University of Copenhagen. The function of the Division of Agrostology (15,000 dollars) is to deal with forage plants as well as grasses, to instruct and familiarise the people with the habits and uses of these plants, to conduct investigations relative to their natural history and adaptability to different soils and climates, to introduce promising native and foreign plants into cultivation, and to identify grasses and forage plants. Its chief is Prof. F. Lawson-Scribner. The Division of Forestry, under the charge of Mr. B. E. Fernow, has at present chiefly been occupied with the study of character and value of different timbers.

The current number of the *Journal de Physique* contains the second part of the paper, by M. P. Curie, on the magnetic properties of bodies at different temperatures (see Nature, June 6, 1895, p. 134). The present paper deals with iron, nickel, and magnetite. In the case of iron, measurements have been made at temperatures between 20° C. and 1360° C., and for field strength of from 25 to 1350 C.G.S. units. The observations on nickel and magnetite were only made at temperatures above that at which the great change in the magnetic properties of these bodies takes place. The values obtained with iron up to about 756° C. agree with those previously obtained by Dr. Hopkinson. Above this temperature the author finds that the curves showing the relation between the intensity of magnetisation (I) and the strength of the field are straight lines passing through the origin for temperatures between 750° and 1280° F. decreases more and more slowly. At first (I) decreases to half its value for a rise of temperature of a few degrees, but between 950° and 1280° the susceptibility is almost a constant, only decreasing very little as the temperature rises. At a temperature of about 1280 the susceptibility suddenly increases by about 50 per cent., and then again gradually decreases up to 1365°. The author, with some hesitation, gives the following explanation of this behaviour: "Up to a temperature of 860° iron behaves like any other paramagnetic body. At a temperature of about 860°, however, it begins to change into a second allotropic form, this transformation being complete at about 920°, and the iron remaining in this condition up to 1280°, and behaving like such a body as oxygen or palladium. Finally at 1280° the iron changes suddenly back to its first condition." The attractiveness of the above theory can only be appreciated by a study of the author's curves, for if the curve showing the connection between the logarithm of the susceptibility and the logarithm of the temperature is plotted, it is found that the curve between 750° and 860° would, if prolonged, form with the curve above 1280° a curve in all respects similar to the curves obtained in the case of nickel and magnetite. With nickel the author finds that the temperature of the magnetic transformation is about 340°. After this temperature the susceptibility is independent of the strength of the field, and decreases regularly and very rapidly as the temperature rises. In the case of magnetite the chief magnetic transformation takes place at a temperature of 535°. At temperatures between 550° and 1370° the susceptibility is independent of the strength of the field, and decreases regularly, and between 850° and 1360° varies inversely as the absolute temperature. The value of K (see previous note, *loc. cit.*) being given by the expression $K = \frac{0 \cdot 0280}{T}$ where T is the absolute temperature. From the differences exhibited by the behaviour with change of temperature of diamagnetic and paramagnetic bodies, the author considers that these two properties must be attributed to different causes.

Last week the *Pharmaceutical Journal* began the first of a new and enlarged series (the fourth). The journal, which is now in its fifty-fifth year, has done much to promote pharmaceutical organisation and progress.

The second part of the Report of the International Meteorological Congress held at Chicago in 1893, has just come to us from the United States Department of Agriculture (Weather Bureau). The papers included in the Report were communicated

to the sections of history and bibliography, agricultural meteorology, and atmospheric electricity and terrestrial magnetism. Part iii. will comprise climatology, instruments and methods of observation, and theoretical meteorology.

THE most important articles in the *Kew Bulletin* for April to July, are one on the various sugar-cane diseases in Barbadoes, one on maple sugar, containing information with regard to the growth of the sugar-maple in the United States; and one on anbury, club-root, or finger-and-toe, describing the mode in which this disease is produced in a number of species of Cruciferæ by the attacks of the parasite *Plasmodiophora Brassicæ*, and the best modes of counteracting it.

THE new quarterly number of the *Journal* of the Royal Agricultural Society contains a paper on "Cross-bred Sheep," by Mr. H. J. Elwes, in which many facts of biological interest are recorded. The value of a first cross between two pure breeds is insisted upon, whilst due importance is attached to the dangers which beset the breeder should he venture beyond the first cross. Mr. Elwes is in a position to draw upon the results of long practical experience in the cross-breeding of sheep. The general improvement which the sheep of this country have undergone within recent years is attributed to the increasing resort to the services of pure-bred sires, but much remains to be done by those breeders who possess the necessary skill, patience, and energy. Another paper of scientific interest is one by Prof. G. T. Brown, C.B., on "Ringworm of Calves," which is illustrated with five original drawings. It is demonstrated that the living spores of the fungus of ringworm may be transmitted from one animal to another by means of lice. Prof. Edgar M. Crookshank contributes a popular paper on "Microbes in Health and Disease," and economic botanists will find much that is interesting in Mr. Glenny's paper on "The Onion and its Cultivation." This issue also contains a schedule of such native wild birds as are "undoubtedly beneficial to agriculture." Altogether, 38 species are enumerated, and details are given concerning their food, nests, and eggs.

THE additions to the Zoological Society's Gardens during the past week include an Anubis Baboon (*Cynocephalus anubis*, ♂), a Leopard (*Felis pardus*), two Two-spotted Paradoxures (*Nandinia binotata*), a Sharpe's Wood Owl (*Syrnium nuchale*) from Accra, Gold Coast, presented by Mr. W. H. Adams; two Red-crested Cardinals (*Paroaria cucullata*) from South America, presented by Dr. G. Fielding Blandford; a Small Hill Mynah (*Gracula religiosa*) from India, presented by Mr. W. Norbury; a Brown Capuchin (*Cebus fatuellus*) from Brazil, presented by Mr. W. L. Gibbs; a Spiny-tailed Monitor (*Varanus acanthurus*) from Roebuck Bay, West Australia, presented by Mr. Saville Kent; a Campbell's Monkey (*Cercopithecus campbelli*) from West Africa, an Egyptian Uromastix (*Uromastix spinipes*) from Egypt, deposited; two Mantchurian Cranes (*Grus viridirostris*) from North China, purchased; two Mule Deer (*Cervus macrotis*), a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SPECTRA OF PERIODIC VARIABLE STARS.—The recent spectroscopic observations of δ Cephei by Belopolsky (NATURE, vol. li. p. 282), and of β Lyræ by Pickering, Lockyer, and others, have shown that we have still a great deal to learn as to the causes of the light-changes in variables of short period other than those of the Algol type. In these inquiries, it has become clear that a study of the light-curves must go hand-in-hand with that of the spectroscopic changes, and we therefore welcome the publication, by Dr. Schur, of new light-curves of δ Cephei, η Aquilæ, and β Lyræ (*Ast. Nach.* 3282–83). The observations were made at Strassburg in the years 1877–85 by Argelander's method, an opera-glass providing the requisite optical aid.

In the case of δ Cephei, the observations and light-curve agree very well on the whole with those of Argelander and Schonfeld, but the interval from minimum to maximum is reduced by Dr. Schur from 1d. 14·6h. to 1d. 13·7h., and the period derived is 5d. 8h. 47m. 38·047s., or 1·027s. less than that of Argelander. There does not seem to be any ground for the idea that the length of the period is sensibly changing. Dr. Schur also obtained distinct evidence of a standstill in the light-curve in the descent to minimum. The period arrived at for η Aquilæ is about 4s. less than that of Argelander, namely, 7d. 4h. 13m. 59·318s. A very decided "hump" is shown on the descending side of the light-curve: this is not merely a halt like that in the case of δ Cephei, but an actual increase of light, commencing about 3d. 20h. after minimum, and reaching an abortive maximum about twelve hours later. The interval from minimum to maximum is 2d. 6h. The observations of β Lyræ give a light-curve of which the general form is almost identical with that given by Argelander, but the agreement of individual minimum with calculated times is not very good. To bring these into better agreement, Argelander's formula is corrected to the following: Epoch 424, Bonn mean time, 1855 Jan. 6, 15h. 28m. + 12d. 21h. 47m. 23s.·72 E + 0s. 315938 E² − 0·0000121 1s. E³.

The paper gives full details of the observations and their reduction, and its value is increased by a plate showing the forms of the light-curves of the three variables in question.

THE NICE OBSERVATORY.—Vol. iv. of the *Annales* of the Nice Observatory is a monument to the industry of the director and staff of the magnificent observatory founded by M. Bischoffsheim. M. Perrotin, the director, contributes an elaborate investigation of the inequalities of the first order in the elements of Vesta, produced by the action of Jupiter, employing interpolation methods. M. Javelle furnishes full particulars of 505 new nebulæ discovered by him during 1890 and 1891 with the great equatorial of 15 inches aperture. The positions of these were determined by micrometric measures of distances from comparison stars, and awaiting accurate meridian observations of these, provisional positions for 1890 have been computed. Some of these objects are easily visible, but the majority of them are rather difficult, and others are at the limit of visibility of the Nice refractor. Star clusters have been rigorously excluded from the catalogue.

The meridian work at the observatory is particularly directed to the double stars of the Dorpat catalogue, and the already numerous stars which have been used as comparisons in the observations made with the equatorial. The period covered by the present publication is 1888 April 5 to 1889 December 23.

From May 1887 to December 1892, 26 new minor planets were discovered at Nice by M. Charlois, the last 11 by photography. A vast number of observations of these and other minor planets have also been made by M. Charlois, full details of which are recorded in the present volume. Observations of 10 comets are also included.

FOUCAULT'S PENDULUM EXPERIMENT.—The experimental demonstration of the earth's rotation, devised by Foucault in 1851, has recently been repeated at the De La Salle Training College, Waterford, on a somewhat smaller scale than in the original experiment. The weight of the pendulum bob was 19 lbs., and it was suspended by a wire 37 feet 6 inches in length. To set the pendulum in vibration, the usual method of burning the string by which the bob is tethered was employed. Thirty-three observations of the hourly motion of the pendulum plane were made during February and March of the present year, and the mean result was 11° 48′, the calculated value being 11° 53′ 37″. The time of the earth's rotation, or length of the sidereal day, thus deduced is 24h. 7m. 30s., an amount only about 11m. in excess of the true time. Foucault's observations gave 23h. 35m. 57s. as the time of rotation. Particulars of the Waterford experiment, and an explanation of the principles involved, are given by Dr. M. F. O'Reilly in *Engineering*, July 5.

THE SUN'S PLACE IN NATURE.[1]

VIII.

TWO objections, however, have been made to these hypothetical two swarms. It has been urged that the secondary swarm which we saw moving in a closed orbit round the primary one would soon spread out into a line along the orbit, so that there would always be some parts of it mixed up with the constituents of the parent swarm. That is a perfectly fair objection, supposing we are dealing with millions and billions of years, but I think that those who have made it do not know the history of astronomy. Let us take, for instance, the history of the November swarm which cuts the earth's orbit, so that in certain Novembers, generally about thirty-three years apart, we get this swarm of meteorites passing through our atmosphere, getting burnt out in that passage, and giving us one of the most magnificent sights which it is possible for mortals to see—a whole hemisphere of sky filled with shooting stars. Some of you may remember such a phenomenon as that in the year 1866, some of us are hoping to see the recurrence of it in 1899, for which we have not long to wait. But the fact that we only get this appearance every thirty-three years shows that, at all events, that swarm of meteorites to which the phenomena are due has not changed during our life-time—nay, it has not changed during the last thousand years, for man has known of that November swarm for more than a thousand years, and we have only known of the variability of Mira for 300 years; so that you see such an objection as that is entirely out of court, because it lacks the historical touch.

Another objection which has been urged is that there are certain irregularities in the light-curves of these bodies; that Mira, for instance, does not always come up to the same amount of brightness at its maximum, and perhaps, for all we know, does not always go down to the same low magnitude when it is at its lowest. That also is perfectly true, and on this account: there is no reason why we should suppose that these phenomena of the waxing and waning light of the body are produced by the movement of one body only; suppose, for instance, that there is some cosmic eye a billion miles away from our solar system, so beautifully and exquisitely wrought, so delicate in its construction, that it can see an increase in the light of the sun every time a big comet goes round it. Now we know from our own experience of comets that it would be absolutely impossible for that delicately constructed eye to see anything like a constant variability in the light of the sun under these conditions, because sometimes the brightest comets which come to us are absolutely unpredicted, they come at irregular times. It must also be pointed out in connection with this objection that there are other obvious causes for considerable variations in the light, both at the maximum and at the minimum. You remember that I showed you those beautiful spiral nebulæ of which Dr. Roberts has given us such magnificent photographs; suppose them to represent the parent swarms, and that another minor swarm tries to pass them; it is impossible to imagine that the minor swarm would exactly pass through all the intricacies of those magnificent spirals, and go and come through it precisely on the same path. It would be certain that in consequence of perturbations, the secondary swarm would sometimes go through a denser portion, at other times through a less dense portion, and then you see that would be quite sufficient to give us a considerable difference of luminosity.

I have another interesting series of diagrams, which will show you that almost any amount of variability and *irregular* variability in the light curves of these bodies may be explained on very simple grounds, supposing we acknowledge that we are dealing with the movements of more than two bodies. For instance, suppose we have one cause at work which gives us a maximum and minimum, and another cause which gives us two very much smaller maxima and minima occurring at a different period represented in Fig. 34 in the upper part of the diagram.

If we add these two together, we get the irregular light curve shown below the two simple curves in the diagram. But the amount of irregularity may possibly only reveal the amount of our ignorance, and when the time comes when we can isolate these two causes, and thus see how the addition of them should be made, we shall find that every part of this curve is really the result of a most beautiful law. I am very glad to say that quite recently Mr. Maxwell Read, of the Harvard Observatory, has put forward this very same suggestion, so that we may hope that it will soon be worked out on pretty broad lines.

But suppose for a moment that this view of two bodies is not accepted. What have we got in place of it? Well, we have to explain all the phenomena of variability by one body. That has been attempted more or less happily. Suppose, for instance, we have the case of a body waxing and waning quite regularly; you have only to say that body is like a soup-plate, and rotates on an axis, so that sometimes you see the face, sometimes only the edge. But that is not very satisfactory, because we do not know any stars which are like soup-plates. Another way is to say that the stars which are variable have great dark patches on one side of them, great bright patches on the other. Well, of course you can get a variation of light by such a scheme as that; but we have not observed that, we are simply inventing, merely suggesting ideas to nature that I fancy nature will tell us by and by are quite erroneous. For instance, I have shown you the facts with regard to β Lyræ. What is the explanation put forward for the variability of that star? Simply this, that it is a surface of revolution, the ratio

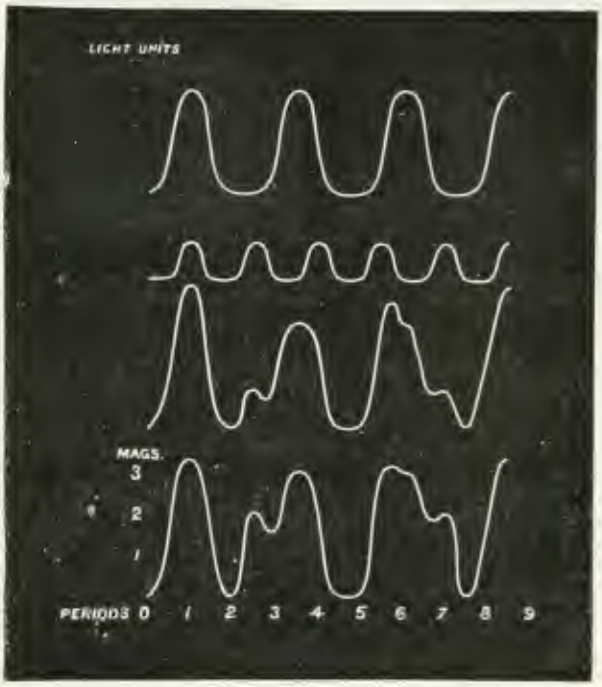


FIG. 34. Indicating how apparently irregular light-curves may be due to the summation of two regular light variations.

of the axes being 5 to 3, *i.e.* elliptic beyond any experience of ours with regard to any other bodies; there is a dark portion at one end of the axis symmetrically situated. This thing then has to turn and twist with its axes and the black spot, and so on, and at the end of the chapter you are to have such a light curve as that of β Lyræ. That you see is blown into thin air by the spectral facts. I think you will acknowledge that these things are irrational, because they have no true basis of fact, and we must remember that in all this work we must deal strictly with the facts in accordance with the rules of philosophising; *i.e.* we must never have a complicated explanation until we are perfectly certain that a simpler explanation will not do, and the simplest explanation of all is that which occurs most frequently in the region of facts. That puts the soup-plate theory with regard to variable stars entirely out of court. Further, remember that supposing those gentlemen who still hold to the one-body theory, one star, one variability, object to the possible explanation of variability by the meteoritic hypothesis, they will find it very much more difficult to explain the departure from regularity by any geometric system, because a geometric system must certainly be

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 207).

more rigid than any other, and therefore any irregularity under it would be almost impossible.

Closely associated with this reference to double swarms in the case of variable stars are the phenomena of so-called "new stars." Indeed, the whole conception of the meteoritic hypothesis arose from a consideration of those bodies which sometimes quite suddenly make their appearance in the heavens. We have had during the last thirty years five of these new stars, and it was during the appearance of one in the constellation Cygnus in 1876 that I was led to the views which I still hold with regard to their origin.

One of the most remarkable features of these new stars is the rapidity with which they lose their brilliancy, and it was this

1 2

Fig. [illegible] The region in the heavens where Nova Aurigæ was observed (1) after its appearance; (2) when brightly visible (nearly in the centre).

which led me in 1877 to write, in connection with Nova Cygni (Nature, vol. xvi. p. 413, 1877): "We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceedingly slow.

"Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy: in those small meteoric masses which, an ever-increasing mass of evidence tends to show, occupy all the realms of space. . . . The Nova now exists as a nebula, so far as its spectrum goes, and the fact not only goes far to support the view I have suggested, as against that of Zollner, but it affords collateral evidence of the truth of Thomson and Tait's hypothesis of the true nature of nebulæ."

we get the greater light formed at the moment when two swarms, one revolving round the other, are nearest together.

Fortunately for science, one of these new stars appeared in 1892; it is known as Nova Aurigæ, and two photographs will give us an idea of the sort of thing which an astronomer sees in the heavens when the discovery of a new star is announced. The photographs show a portion of the constellation of Auriga, and a star which is very clearly seen in the photograph taken very soon after this star had burst upon us, is absent from one taken a few months later.

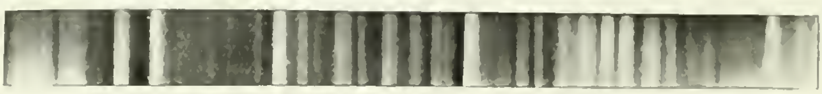
Since the spectroscope was first applied to the stars, five new stars have been observed and spectroscopically examined. One appeared in Corona Borealis in 1866, one in Cygnus in 1876, and one in Andromeda in 1885; then came the one in Auriga in 1892, to which reference has already been made, and last of all was one in the southern hemisphere, discovered in 1893. The first three of these were observed by eye only, but in the two recent ones we have the immense benefit of photographic records.

It was therefore a very interesting point when a new star came along, to see whether there was any additional light thrown by it upon the problem of two bodies; and further, upon one of the points in which, if the meteoritic hypothesis failed, it was worth absolutely nothing at all. If there was any truth in the idea of the light of these bodies being produced by the clash of meteor-swarms, when the clash was over the swarms should go back into their native obscurity, or condition of low temperature, and should, if they were seen at all, put on the spectrum of sparse swarms in other parts of the sky: that is, they should put on the spectrum of a nebula.

That, you see, was a very crucial point; it was a point which could be settled by the spectroscope, provided always we had one of these marvellous bodies at such a distance from us that we could still observe it spectroscopically, and see what the different changes really amounted to.

Already in the case of Nova Cygni, the spectrum had been observed to change from a rather complicated one of bright lines and flutings to a very simple one, similar to that of a planetary nebula. The observations did not, however, furnish any direct evidence that more than a single body was concerned in the outburst.

The appearance of Nova Aurigæ, however, furnished a splendid opportunity of testing the many theories which have been at various times advanced to account for the phenomena. This Nova was discovered at Edinburgh by Dr. Anderson, who was modest enough to announce his discovery by sending an anonymous post-card to Dr. Copeland, the Astronomer Royal for Scotland, on February 1, 1892. It was then a star of the fifth magnitude, and on confirming the true nature of the newly-discovered star by means of the spectroscope, Dr. Copeland made the news public. Information was received at most observatories on February 3, and on the same evening two photographs of the spectrum were taken at South Kensington. During the next two or three weeks the star fluctuated considerably in brightness, though being generally on the down grade; and by April 26 had fallen to the



K H G F

Fig. [illegible] Photograph of the spectrum of Nova Aurigæ, taken at South Kensington, February 7, 1892.

Returning to the subject of new stars in 1887, in a general discussion of the meteoritic hypothesis, I saw no reason to change my views, and an inquiry into the spectroscopic phenomena led me to state that: "New stars, whether seen in connection with nebulæ or not, are produced by the clash of meteor swarms, the bright lines seen having low temperature lines of elements, the spectra of which are most brilliant at a low stage of heat."

A very detailed investigation of all the new stars which had been observed up to 1890 formed the subject of a communication to the Royal Society, and it was shown that the hypothesis would explain the fluctuations of light, the changes of colour, and the spectroscopic appearances.

To make a very long story short, it was suggested that the phenomena of new stars were produced by exactly the same cause as that which was at work in the variable stars in which

10th magnitude, so that it could only be picked up at all in the very largest telescopes. Thanks to the photographic records of the stars, it was possible to learn something of the earlier history of the new star. It had really been photographed by Prof. Pickering two months before its existence was known.

Fig. 30 shows us a photograph of the spectrum of this wonderful star itself, and it will be seen that in the case of all the chief lines we get a bright line and a dark line side by side. There are the hydrogen lines; that is, in the spectrum of that body we were dealing with the giving out of hydrogen, and the absorption of hydrogen. Now, the same set of particles cannot be producing bright and dark lines at the same time. We were then obviously dealing with two sets, and the first photograph, therefore, which was taken of the spectrum of this strange body, put beyond all question the fact that we were really dealing with two

bodies, and not with one. That was very important; but you will see from the photograph, that it is very unlike the spectrum of nebulæ, so that it required a certain amount of faith when the spectrum was observed to be such as you see it here, to suppose that after a certain time, when the action which produced the greater luminosity was reduced and the light toned down, we should eventually get the spectrum of a nebula.

Well, as a matter of fact, the Nova reappeared in August 1892, and was observed to have increased in brightness from the 16th magnitude in April to about 9th magnitude. What, then, was the spectrum? It had almost completely changed; and among the first to observe the new spectrum was Prof. Campbell, of the Lick Observatory. This observer then stated that "the spectrum resembles that of the planetary nebulæ." In the following month the spectrum was also observed by Drs. Copeland and Lohse, and their observations seemed to them to "prove beyond doubt that Nova Aurigæ is now mainly shining as a luminous gas nebula." The most striking evidence on this point, however, is that afforded by the photographic investigations of Von Gothard. He not only shows us the photographic

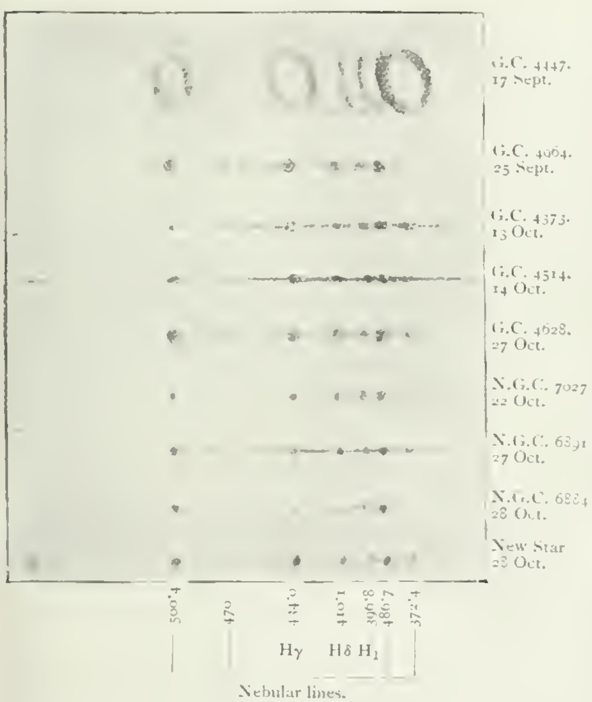


FIG. 37.—The spectrum of the new star in Auriga, as compared with the spectra of planetary nebulæ (Gothard).

spectrum of the new star at this stage of its history, but gives us also the spectra of several nebulæ to compare with it; and it is evident that we were certainly dealing, in the case of the Nova, with the same spectrum as in the nebulæ. Dr. Gothard, at least, was satisfied on this point, and stated that "the physical and chemical state of the new star resembles at present (September and October 1892) that of the planetary nebulæ."

So you see we get, first of all, the hard fact that the spectrum indicated the existence of two bodies; and then the very much harder fact for some, that, after the war was over, we got back to the condition of the nebulæ. I need not tell you that there is not universal agreement on this point, and chief among those who do not yet acknowledge it are Dr. and Mrs. Huggins. Writing of their observations of February 1893, they say: "We wish to speak at present with great reserve, as our knowledge of the Nova is very incomplete; but we do not regard the circumstance that the two groups of lines above described fall near the positions of the two principal nebular lines as sufficient to show any connection between the present physical state of the Nova and that of a nebula of the class which gives these lines."

But I may say, at all events, that I have the great authority of the names of Campbell, Copeland, and Gothard, who state that they have certainly observed the spectrum to be that of the nebulæ, and in reply to Dr. Huggins, Prof. Campbell says: "If the spectrum is not conceded to be nebular, I must ask what else we should expect to find in that spectrum, if it were nebular?" The answer to that is, that you would not expect to find anything else because it is all there already. In fact, out of nineteen lines observed or photographed by Prof. Campbell in the spectrum of the Nova, eighteen correspond perfectly with nebular lines. "Therefore the spectrum is nebular, and the fact that the lines have remained broad, or may have remained multiple, does not militate against the theory."

Further, there is even telescopic and photographic evidence of the fact that Nova Aurigæ became a nebula. Dr. Max Wolf's photographs of the Nova and its surroundings in 1893, resulted in the discovery of a number of new diffuse nebulæ in its vicinity, "and there even appeared to be traces of nebulous appendages proceeding from the star itself."

Another new star appeared in the southern constellation Norma in 1893. This was discovered on October 26, on a photograph taken at Arequipa, Peru, on July 10, 1893. Fortunately the photograph was one showing the spectra of stars instead of the simple images of the stars themselves, and the spectrum was seen to be identical with that of Nova Aurigæ. Even more important were the observations of Campbell in February and March 1894, when the star was about 10th magnitude. As the result of his work, he stated that "there can be no doubt that the spectrum of Nova Normæ is nebular."

J. NORMAN LOCKYER.

(*To be continued.*)

THE FLUORESCENCE OF ARGON, AND ITS COMBINATION WITH THE ELEMENTS OF BENZENE.[1]

M. BERTHELOT read the following paper, containing observations by M. Deslandres and himself, before a recent meeting of the Paris Academy of Sciences:—

I have thought it useful to study more closely the conditions of the combination with benzene under the influence of the silent discharge and those of the special fluorescence which accompanies it.

M. Deslandres, whose great competence in spectroscopic questions the Academy is well aware of, has been kind enough to help me in these new determinations, made with higher dispersion, and rigorously determined by photography. It is my duty to thank him here for this long and laborious work.

We must remember that the combination of argon with the elements of benzene, under the influence of the silent discharge, is a slow process; according to the present research, it is accomplished with the help of mercury, which intervenes under the form of a volatile compound. The use of very frequent discharges appears not to modify the general characters of the reaction.

At the beginning, nothing is seen in daylight, and it is only in a dark room that one perceives a feeble violet glow, similar, in its intensity, to that which the discharge develops generally in gaseous systems. At the end of an hour, when in a dark room, a green glimmer is seen, which occupies the middle of the interval between the spirals of the platinum band wound round the discharge tube, the luminous spectrum gives two yellow lines at λ 579 and 577, a green line at λ 546, and a green band at λ 516·5. These different lines will be defined by-and-by.

The photographic spectrum, taken during this time, with an hour's exposure, shows the principal bands of nitrogen, as well as a blue line λ 436, a violet line λ 405, and an ultra-violet line λ 354: these latter being more feeble than the bands of nitrogen.

During the following hours, the green glow constantly increases, the yellow lines and line λ 546 increase, and the band λ 516·5 diminishes. At the end of eight hours, the bands of nitrogen have almost entirely disappeared in the photograph: without doubt it is because the corresponding nitrogen has been absorbed by the benzene.

Seven additional hours of sparking bring the fluorescence to a brilliant emerald colour, visible in broad daylight; the intensity of this phenomenon, as I have already had occasion to say,

[1] Translated from *Comptes rendus*, June 24, pp. 1386-1390.

not to be compared with the fluorescence developed by the discharge in any known gas. The yellow and green lines can be seen and measured in the spectroscope in full daylight.

The photographs give the following lines wave-lengths, λ λ 579, 577 and 540; 436, 405, 354, 313 and 312 (ultra-violet): one can see two violet lines besides, 420 and 416, scarcely visible, and the lines 385 and 358.

The spectrum observed at the end of fifteen hours was maintained during thirty consecutive hours.

Although advantage has been taken of photography for the registration of these phenomena, care must be taken not to confound such effects, observed in the daytime and under normal pressure, with the glows developed by the discharge in very rarefied gases, such as are generally observed in a spectroscope.

The meaning of these lines is as follows:

The line λ 579 is simply one of the lines visible in daylight, and under normal pressure, which I had described in *Comptes rendus* (t. cxx. p. 800), pointing out that it was probably double. The lines λ λ 580·1 and 577·1 described in the spectrum of rarefied argon, by Mr. Crookes (Jan. 24, 1895), must be compared with them.

Line λ 540 is also described (547) in my preceding note, and answers to a strong line 545·0 attributed to the spectrum of rarefied argon by Mr. Crookes. M. Deslandres has recognised the same lines in the spectrum of rarefied argon, which he had prepared by means of lithium. I have verified, by juxtaposition, the coincidence of the last line of rarefied argon with that of my tube.

I have also announced line 436, found again in the photograph, and very close to 434·5 of rarefied argon (Crookes). The lines 420 and 416 coincide with the very strong lines 420·1—416·8 and 415·00 of rarefied argon (Crookes). The line 405 can be identified with the line 404·4 of Crookes (argon). I have verified the coincidence. Line 385 coincides with Crookes' strong line 385·15 (argon). Line 354, with a group of strong lines at 354·7—353·4 of argon (Crookes). Line 358 with Crookes' group of strong lines 358·7—357·5 (argon).

λ 516·5 is a hydrocarbon band; 313 and 312 are the lines of the vapour of mercury vapour.

None of these lines, as I have already stated, coincide either with the line of helium (587·5) or with the principal line of the aurora borealis (557), although the latter is very near to a strong line of argon (555·7). If the actual fluorescence is not the same as that of the aurora borealis, still its development, and the nearness of the preceding lines, establish a probable relation between this meteor and the existence of argon in the atmosphere.

Here a very important circumstance presents itself. While examining the table of argon lines, given by Mr. Crookes, certain lines were seen to coincide with certain lines of the vapour of mercury. The same coincidence is found in the straight lines visible in daylight, under the normal pressure, in the fluorescence developed during the reaction of benzene on argon. Such are, according to M. Deslandres, the yellow lines 579 and 577; also the very characteristic green line 546, the blue line 436, the violet line 405, the ultra-violet line 354. On the contrary, the lines 420, 416, 385, 358, belong to argon only, the lines 313 and 312 to mercury.

M. Deslandres attributes the common lines to the presence of the vapour of mercury, either in rarefied argon, or in the fluorescent light obtained with benzene under normal pressure.

Nevertheless, as no known gas gives this fluorescence, or these lines, under normal pressure in operating with mercury, it is not possible to explain their production merely by the presence of the vapour alone. Otherwise it would not be understood why they did not show themselves in pure argon, in the presence of mercury under normal pressure, and that they would not produce themselves during the first moments of discharge, either with argon saturated with benzene, or with sulphide of carbon over mercury, or with nitrogen under the same conditions, where it combines with benzene and sulphide of carbon. On the contrary, with argon saturated with benzene, they develop themselves only at the end of several hours, and after the progressive transformation of the benzene into a series of compounds more and more condensed. It is one of these compounds which, immediately it is formed, unites itself both with argon and mercury, [illegible] perhaps by reason of their common character of monatomic molecules. The fluorescence begins when there still exists a notable quantity of liquid benzene in the tubes; it is then accompanied by a diminution of gaseous volume.

This fluorescence continues a very long time, even after there is no apparent benzene; at last the fluorescence ceases to be visible in the daylight, in consequence of the very prolonged action of the discharge, which at last extinguishes the green tint, and brings back this gaseous system to a glow analogous to that of ordinary gases, this happens doubtless in consequence of the destruction of the last traces of benzene (or the products of intermediate condensation), which maintained the equilibrium of the dissociation of the system.

Once the green fluorescence is well established, the compounds which develop it are stable by themselves; even after twelve hours' break, if the apparatus has not been disturbed, it suffices to pass the discharge, to see that the fluorescence re-establishes itself with all its brilliancy in an instant. But it ceases so soon as the electric current is stopped.

But if the gas is separated from the condensed matter, the phenomenon cannot be immediately produced, either on the one or on the other. The gas alone, subjected to the action of the discharge, puts on almost immediately a special violet fluorescence, visible in darkness, and which generally precedes the development of the beautiful green fluorescence. Nevertheless this does not reproduce itself then, which seems to indicate that the condensed matter contains one of the products necessary to the equilibrium. If, on the other hand, new argon is reintroduced into the tube containing the condensed matter (free from visible benzene), the green fluorescence does not reproduce itself: but after some time, near the surface of the mercury, there appears, where the sparking is most intense, a local green tint, which gives the special lines, although not very distinctly. This appearance is doubtless due to the existence (or to the regeneration) of a trace of benzene, more or less modified. In fact, if a few more drops of liquid benzene are added in the tube which contains the condensed matter and the new argon over mercury, half an hour is enough to make the green tint reappear in all its brilliancy. But if there is an excess of benzene, several hours are required for its reappearance.

These various observations, added to the limited character of the absorption of argon, demonstrate the existence of a complex state of equilibrium, in which at the same time argon, mercury, and the elements of benzene, or rather a compound condensed from it, are concerned.

THE REFORM OF OUR WEIGHTS AND MEASURES.

THE Report of the Select Committee appointed to inquire whether any, and what, changes in the present system of weights and measures should be adopted, has been published as a Parliamentary paper.

Evidence from witnesses representing official, commercial, manufacturing, trade, educational, and professional interests was received by the Committee, and numerous corporations, School Boards, and other public bodies sent resolutions in favour of the adoption of the metrical system.

All the witnesses expressed a strong opinion as to the complicated and unsatisfactory condition of the present weights and measures in use, and of the distinct and serious drawback to British commerce, especially in the foreign trade, which this system entails, differing as it does from the system (metrical) now adopted by almost every European nation, as well as by far the majority of non-European countries with which this kingdom trades. The evidence also showed that the home trade would be benefited if more simple and uniform standards of weights and measures than those now existing were adopted.

Moreover, strong evidence was brought forward as to the serious loss of time incurred by English school-children in having to learn the complicated system of tables of existing weights and measures, and the urgent need of the adoption of a simpler system. It was stated that no less than one year's school time would be saved if the metrical system were taught in place of that now in use.

Evidence from competent witnesses proved to the satisfaction of the Committee that a compulsory change from an old and complicated system to the metrical had taken place in Germany, Norway and Sweden, Switzerland, Italy, and many other European countries without serious opposition or inconvenience: that this change was carried out in a comparatively short period: and that as soon as the simple character of the new system was understood it was appreciated by all classes of the population.

and no attempt to use the old units or to return to the old system was made.

In the United States, where a system founded on the English units exists, a Commission is at present engaged in an investigation of the same character as that with which the Committee was charged, and the Federal Government has this year passed an Act rendering the metrical system compulsory for pharmaceutical purposes.

The Committee believes that the adoption of the metrical system by England would greatly tend to render that system universal.

It is recommended :—

(*a*) That the metrical system of weights and measures be at once legalised for all purposes.

(*b*) That after a lapse of two years the metrical system be rendered compulsory by Act of Parliament.

(*c*) That the metrical system of weights and measures be taught in all public elementary schools as a necessary and integral part of arithmetic, and that decimals be introduced at an earlier period of the school curriculum than is the case at present.

SCIENCE IN THE MAGAZINES.

THIS month's *Contemporary Review* is remarkably rich in articles of scientific interest. Mr. Herbert Spencer's third paper on professional institutions deals with the "Dancer and Musician." So far back as 1857, Mr. Spencer showed that, excluding movements which are reflex and involuntary, muscular movements in general are originated by feelings in general. "As a consequence of this psycho-physical law, the violent muscular motions of the limbs which cause bounds and gesticulations, as well as those strong contractions of the pectoral and vocal muscles which produce shouting and laughter, become the natural language of great pleasure." From the ways in which children manifest their joy were evolved the expressions of elated feeling with which peoples meet their conquering chief or king, and eventually the natural displays of joy came "to be observances used on all public occasions as demonstrations of allegiance, while, simultaneously, the irregular jumpings and gesticulations with unrhythmical shouts and cries, at first arising without concert, gradually by repetition became regularised into the measured movements we know as dances, and into the organised utterances constituting songs. Once more, it is easy to see that out of the groups of subjects thus led into irregular ovations, and by-and-by into regular laudatory receptions, there will eventually arise some who, distinguished by their skill, are set apart as dancers and singers, and presently acquire the professional character." In support of this interpretation evidence obtained from many nations is adduced, and the separation and secularisation of the twin professions of dancing and music are traced. Mr. G. F. Scott-Elliot writes in the same review on "The Best Route to Uganda." He is in favour of a route following the line of the African lakes. The route enters the Zambesi at Chinde, and continues up the Zambesi and Lower Shiré as far as Chiromo, from whence a railway of approximately 120 miles would be required across the Shiré Highlands to Matope, from which point the Upper Shiré is navigable, and goods can be carried to the north end of Lake Nyassa. Here another railway would be required from Karonga to South Tanganika (240 miles). From the north end of Tanganika a line would run to Kagera. The Kagera river rises on the easterly flanks of the mountains to the east of Tanganika, and eventually falls into the Victoria Nyanza. A cataract is said to exist on the river, but even if this is so, and a length of line is required to avoid it, the cost of the whole line would only be about £1,700,000, or one-half that necessary for the Mombasa railway. Other considerations point clearly to the Lake route as the better of the two suggested lines. Prof. Lombroso contributes a paper on "Atavism and Evolution." He gives a number of instances of what he regards as atavistic phenomena in social life. "England," he says, "has succeeded in establishing a form of monarchy the most liberal in Europe ; and is working out without disturbance the aims of Socialism. But, at the same time, she not only maintains the privileges of her Peers, but actually dresses them up, as well as her judges, in the wigs and robes of the Normans ; and still uses, on ceremonial occasions, the language of her ancient conquerors. . . . Then this very positive and practical nation insists on retaining a system of weights, measures, and coins, which is opposed to that of all modern Europe, and is an obstacle both to commercial exchange and to scientific research." He classifies recent inventions which are shown to be old as evidence of atavism, and explains the duplication by the dislike with which, according to him, human nature regards novelties. Too rapid advance in the arts provokes reaction and causes the tide of progress to ebb when it should be flowing. A sensible article on the "Physiology of Recreation" is contributed by Mr. Charles Roberts, in the course of which he gives the following classification of physical recreations according to their physiological value. Outdoor: running, athletics, games, skating, skipping, &c. ; riding, rowing, swimming, walking, cycling, marching. Indoor: fencing and other military exercises with arms, boxing and wrestling, dancing, billiards, dumb-bells, machine gymnastics, trapeze and high gymnastics, singing and reading aloud, playing musical instruments. Recreations of a leisurely sort, physiologically considered, are: Outdoor: natural history, gardening and farming, carpentry and other technical work. Indoor: reading ; chess, draughts, and cards ; music. Another paper in the *Contemporary*, entitled "The Origin of Man and the Religious Sentiment," by A. Fogazzaro, invites criticism from the standpoint of evolution.

Prof. Case, Professor of Moral and Metaphysical Philosophy in Oxford University, champions the cause "Against Oxford Degrees for Women," in the *Fortnightly*. He holds that the admission of women to University examinations has brought out the difficulties of teaching mixed classes, and that a mixed University is not desirable, especially at Oxford. Let women have facilities for higher education, by all means, thinks Prof. Case, but let these opportunities be afforded by a University especially founded for women. Mr. Grant Allen writes on "The Mystery of Birth," in the same review, the object of his article being to raise the question, "Is there any real and essential difference between the transmission of functionally-acquired modifications to offspring, and their registration or persistence in the individual organism?" Disciples of Weismann, and biologists generally, will be interested to know that Mr. Allen proposes "to throw back upon assimilation, in its widest sense, the burden of the mystery hitherto attached to the reproductive function."

The *Reliquary and Illustrated Archæologist* has among its articles one by Mr. H. W. Young, on the discovery of an ancient burial-place and a symbol-bearing slab at Easterton of Roseisle. A large number of flint instruments, such as arrow-heads, axes, scrapers, &c., found associated with the remains, make the discovery interesting and important, especially in relation to the geology of the "Laich of Moray."

Natural science predominates in *Science Progress* this month. The pathological results of the Royal Commission on Tuberculosis are discussed by Dr. Sidney Martin, and Mr. Arthur Keith uses Dr. Dubois' *Pithecanthropus Erectus* as a text for a helpful review of human fossil remains. The geology of the Sahara forms the subject of a paper by Mr. Philip Lake. As in July 1894, Mr. Chree shows, in an extremely valuable table, the recent values of the magnetic elements at the principal magnetic observatories of the world. In an article entitled "A Type of Palæozoic Plants," Mr. A. C. Seward directs attention to the histological structure and affinities of the genus *Calamites*, and finally Dr. W. D. Halliburton describes the formation of lymph.

Among the articles in *Knowledge*, we notice "The Sugar Cane," by Mr. C. A. Barber ; "Scorpions and their Antiquity," by Mr. Lydekker, illustrated by two fine pictures of the giant sand-scorpion of Namaqualand, reproduced from photographs, and "The Great Nubecula," by Mr. E. W. Maunder. There are also articles on the field of diameter of the field of view of a telescope, Dr. Roberts' photographs of star-clusters and nebulæ, the cause of earthquakes, and on Prof. Fraser's experiments to find a cure for snake-bites.

Blackwood's Magazine contains a paper in which Colonel Knollys dwells upon public school and Army competitive examinations. He holds that the imperfections of the training at our public schools, and the character of some of the examination papers, are responsible for the cramming now so common with candidates for the Army. Two other articles, in which our readers may be interested, are "Mountaineering Memories," by Mr. H. Preston Thomas, and "The Territorial Waters and Sea Fisheries."

A passing notice must suffice for the remaining articles of scientific interest in the magazines and reviews received by us. The *Century* has an article on "Picturing the Planets," by Prof. J. E. Keeler ; the article is illustrated by views of Jupiter,

Mars, and Saturn, obtained at the Lick Observatory. To the *English Illustrated*, Mr. Grant Allen contributes another "Moorland Idyll": and the inhabitants of "The Monkey House in the Zoo" are described and illustrated by Mr. F. Miller. In the *Humanitarian*, Mr. J. G. Raupert has a pseudo-scientific article upon "Some Results of Modern Psychical Research": and in *Chambers's Journal*, there are articles worth reading on death from snake-bite in India, the Carstairs Electric Light Railway, and citric acid. Geographers will be interested in a paper on "England and France in the Nile Valley," contributed by Captain F. D. Lugard to the *National*. Here we may also mention that the *Geographical Journal* contains a valuable paper in which Dr. H. R. Mill describes his bathymetrical survey of the English lakes. *Good Words* has an illustrated article upon the manufacture of coal-gas, but neither *Scribner* nor the *Sunday Magazine* have articles calling for comment in these columns.

THE RELATION OF BIOLOGY TO GEOLOGICAL INVESTIGATION [1]

I.

The Character and Origin of Fossil Remains.

In prosecuting the study of the fossil remains of animals and plants, the investigator may have either one or the other of its two leading objects in view; but each being so closely related to the other, it is always essential that they should be pursued with direct relation to each other. In the first case, the leading object to be attained is the extension of our knowledge of the animal and vegetable kingdoms far beyond that which may be acquired by the study of living animals and plants; and in the second case, it is to apply that knowledge to the study of structural and systematic geology. The object in the first case is purely palæontological; in the second, it is not only to acquire palæontological knowledge, but to apply it to various branches of geological investigation.

There are seven different natural conditions in which fossil remains are recognisable, three of which relate to substance, three to form, and one to both. To those relating to substance the terms permineralisation, histometabasis, and carbonisation are here applied; to those relating to form, the terms moulds, imprints, and casts; and to the one relating to both form and substance, the term pseudomorphism.

The term permineralisation applies to that condition of fossil remains of animals which differs least from their original condition as parts of living animals; such, for example, as bones of vertebrates, shells of molluscs, tests of crustaceans, &c. The term histometabasis is applied to that condition of fossilisation in which an entire exchange of the original substance for another has occurred in such a manner as to retain or reproduce the minute and even the microscopic texture of the original. Pseudomorphism of fossils is so nearly like that of mineral crystals, that this term is equally applicable to both. It consists in the replacement of the original substance of the fossil by a crystallisable or crystallised mineral, such, for example, as calcite, pyrite, quartz in the form of chalcedony, &c., the original form of the fossil being perfectly retained. The term carbonisation is applied in this connection only or mainly to such masses of vegetable remains as coal, lignite, and peat. Moulds are cavities in sedimentary rocks which were originally occupied by fossils, the latter having been subsequently removed by the percolation of water containing a solvent of the fossils but not of the rock. Imprints do not differ materially in character from moulds, the former term being usually applied to impressions left in the rock by thin substances like leaves of plants, wings of insects, &c., after their removal by decomposition. Sometimes, however, the moulds of shells and other fossils have been reduced to the character of imprints by the extreme pressure to which the strata containing them have been subjected. Casts are counterparts of fossils, having been produced by the filling of moulds with a substance other than that of the original fossil. These are the principal conditions in which fossils occur, or by which they are represented, but one occasionally finds specimens which indicate certain conditions that are not fully recognised in the foregoing descriptions.

[1] [illegible] (Abstract of a series of eight [illegible] published in the Report [illegible] United States National Museum.)

Sedimentary Formations, their Character and Limitation.

There has been much difference of custom among geologists as regards the use of the term formation, some applying it to the smallest assemblages of strata which possess common characteristics, while others designate by the same term those series of formations for which the word system has been generally used. That is, some apply the term formation to local or limited developments of strata, while others apply it to such systems as the Devonian, Carboniferous, Cretaceous, &c. This term has generally been confined to the stratified rocks, but by a few authors it has been applied to the eruptive, and also to the great crystalline, rock masses. In this paper, however, the use of the term formation is not only confined to the stratified rocks, but it is restricted to those assemblages of strata which have common distinguishing characteristics, whether they have little or great geographical extent, or whether they aggregate a few feet or thousands of feet in thickness. That is, the use of the term is confined to those assemblages of stratified rocks of sedimentary origin[1] to which many authors have applied the term group, and others the term terrane.

The foregoing remarks concerning the characterisation of formations have been made with special reference to those which are more or less fossiliferous. It sometimes happens, however, that fossils do not exist, or are not discovered, in certain formations which are evidently of sedimentary origin. This may have been due in some cases to the uncongeniality, as a faunal habitat, of the waters in which the formation was deposited, and in others to their failure to receive any fossilisable remains of animals and plants from the land. In other cases, the absence of fossils may have been due to their destruction or obliteration. The latter has probably been the case with many metamorphic rocks and with the great pre-Cambrian series of stratified rocks generally. In all these cases the formations, while they may possess more or less distinct physical characteristics, lack the chief characteristics of sedimentary formations, namely, the biological.

The occurrence of an unfossiliferous sedimentary formation as a member of an otherwise fossiliferous series is unusual, but in such a case its definition and limitation would be effectually accomplished by the underlying and overlying formations. In the case, however of a great unfossiliferous series of stratified rocks like the pre-Cambrian it is necessary to adopt a method for their study and classification based wholly upon physical data, after the fact that they are pre-Cambrian has been determined from biological data. Such a method of classifying and characterising those unfossiliferous stratified rocks as they occur in North America has been proposed by Prof. R. D. Irving[2] and afterwards elaborated by others. This great series of rocks, as it is developed in America, has such distinguishing general characteristics and such magnitude and geographical extent, that some geologists have thought it worthy of being assigned to a special division of study, but because no certain traces of organic forms have been discovered in them, they have, so far as it is now known, only the indirect relation to biological geology that has just been referred to. Still it is not improbable that those strata were once fossiliferous, and that the great series was once made up of formations similar to those which have been already defined, but it does not necessarily follow that the divisions which are now recognisable by physical characteristics correspond to those formations. It is probable that they more nearly correspond to systems or to the larger divisions of systems as they are recognised in the great scale of the fossiliferous rocks of the earth.

The following conclusions concerning formations are deducible from a consideration of the available facts:—

While formations are physical objects and have only a physical existence, their proper characterisation is chiefly biological.

They are characterisable mainly by the fossil remains of aquatic faunas.

Neither their physical nor biological limits are sharply defined except as a result of accidental causes.

Their geographical limitations are indefinite except those which were occasioned by shore lines.

[1] To avoid frequent repetition, the terms sedimentary formation and stratified formation are used interchangeably when applied to formations as defined above. The terms sedimentary rocks, stratified rocks, and fossiliferous rocks are also used interchangeably, but with a somewhat more general meaning than is intended by the two former terms.

[2] Irving, R. D.: "Classification of the Early Cambrian and Pre-Cambrian Formations." (Seventh Ann. Rep. U.S. Geol. Survey, pp. 371-399.)

They do not necessarily bear any close relation to one another as to geographical area, thickness, or the duration of time in their accumulation.

Although they are thus unequal to one another, they constitute the only available physical units for local or regional stratigraphic classification.

Because of their limited geographical extent they cannot be used as units of the universal classification of the stratified rocks.

THE RELATION OF FOSSIL REMAINS TO STRUCTURAL GEOLOGY.

There are two methods by which the study of fossils may legitimately be applied to geological investigation, and the following statement of the character of these is in part explanatory of the results that may be obtained by their aid. For convenience, one of them may be termed empirical and the other philosophical, because in the one case results are obtained by experience, and in the other by reasoning upon the various results thus obtained. Still, discrimination between these two methods cannot usually be sharply drawn, because, while all geological investigation is largely empirical, it is always more or less philosphical. Such a division of the subject, however, besides being a convenience, gives an opportunity to emphasise the fact that a large proportion of the work that is done in structural geology is based mainly upon the empirical observation and collection of biological data.

Both these methods are not only important but indispensable, the one not less so than the other. Both may be, and aften are, used together, but the empirical method is more largely used in practical field studies than in others, because in such studies fossils are to a large extent treated as characteristic tokens of formations, or as arbitrary means of identifying them and distinguishing them from one another. Such identification necessarily constitutes one of the first steps in the practical study of structural geology, but the subsequent study of the fossils thus empirically used is necessarily more philosophical.

The philosophical method of treating fossil remains, however, is largely applicable to systematic geology or those branches which pertain to the universal chronological classification of the sedimentary formations and to their correlation in different parts of the world. The naturalist studies fossil remains as representatives of the long succession of progressively and differentially developed organic forms which, during geological time, have existed and become extinct, and of which succession the now existing forms of life constitute only the terminal portion. It is the results of such studies as these that the geologist uses in the philosophical studies referred to.

Of the two ways in which formations are naturally characterisable, one is physical and the other biological. Physical characterisation may be direct or general, that is, it may be by identity of kind or kinds of rock of which the formation is composed, or by its possession of that more general or indefinite property or condition which indicates homogeny.

Formations are biologically characterised only by the fossil remains of animals and plants which lived while they were in process of deposition, and the more intimate the natural relation of any of those animals and plants to the physical conditions which produced a formation, the more characteristic of it are their remains. This implies that, while no kind of fossil remains is to be rejected in practical studies of structural geology, there is much difference in the value of the different kinds for this purpose. These differences in value will be specially discussed later on.

Much has been written on methods of distinguishing between formations of marine and non-marine origin, and the legitimate inferences that may be drawn from them, respectively, as to the physical conditions which prevailed while they were accumulating. It is desirable here to present some remarks upon the relative value in practical geological field work of the fossils found in marine and non-marine formations, respectively.

That the fossil remains of marine faunas are far more valuable as indicators of the chronological divisions of the geological scale and of the correlation of its divisions in different parts of the world than are those of non-marine faunas, is apparent to every one who is familiar with even the general facts of biological geology, but it does not follow, and it is not true, that the latter are intrinsically less valuable than are the former in field studies of practical geology. For this practical work, both marine and non-marine fossils are treated by the empirical method already explained, and both are found to characterise the respective formations in the same manner.

Certain conditions, however, give each an advantage over the other under different circumstances. For example, the geographical range of the non-marine invertebrate fossil faunas, especially those of fresh water, having been sharply defined by shore lines, the species which constituted them are to that extent more characteristic of the formations in which they occur than is the case with marine faunas. Certain species of the latter faunas, as already shown, usually ranged beyond the limits of the area which was occupied by each fauna as a whole.

Non-marine formations, as a rule, occur singly in a series of marine formations, in which case the vertical as well as the geographical range of their invertebrate species is sharply defined. It is true that in the interior portion of North America there is a continuous series of fresh-water formations, and that certain of the species range from one into another. These, however, are notable exceptions to the rule referred to, and they at most only make such non-marine faunas equal to the average marine fauna as regards exceptional vertical range of species. Again, non-marine formations usually have the advantage of the presence of remains of plants and of land vertebrates and invertebrates, which in marine formations are usually so extremely rare as to be unavailable.

On the other hand, marine faunas embrace such a wide diversity of forms as compared with the non-marine, and their progressive and differential evolution from epoch to epoch has been so much greater, that they offer as faunas much more abundant means for the characterisation and identification of formations. It is clear, however, that the opinion which some geologists have expressed or implied, that the fossil contents of non-marine formations are of little value in practical geological investigation, is not well founded. The following conclusions sum up the case :

Formations being the only true units of local or regional stratigraphic classification, their correct identification is the first, and an indispensable, step in the practical field work of structural geology.

Although formations as such have only a physical existence, their biological characteristics are always the best, and often the only, means of their identification, and therefore the exhaustive study of fossils is of paramount importance in connection with all practical investigations of that kind.

The value of fossils in this respect is as purely practical as is that of any other aid to geological investigation, and it may be made available without reference to their great value in other respects.

Although all fossil remains are valuable for this practical use, those of aquatic faunas are more valuable than any others.

Remains of non-marine faunas are of similar value for this purpose to those of marine origin.

THE RELATION OF BIOLOGY TO SYSTEMATIC AND HISTORICAL GEOLOGY.

It has been made apparent in the preceding sections that each case of structural classification of stratified rocks based upon formations as physical units is independent of all others, and that its application is necessarily of limited geographical extent, because formations are themselves thus limited. It therefore follows that the structural geology of any district or region, embracing even an extensive series of formations, may be practically and thoroughly investigated, as regards both scientific accuracy and economic requirements, independently of that of any other district or region, especially of those regions which are not adjacent. It is now to be shown how the multitude of series of formations thus locally classified throughout the world have been grouped into a universal system of classification in connection with a scale having its divisions arranged in chronological order.

When the fossil faunas and floras which characterise each of a given series of sedimentary formations are compared with those which severally characterise the formations of the next preceding and succeeding series, and the whole are systematically compared with living faunas and floras, there is to be observed among those fossil forms, when studied through an unbroken vertical range of formations, an order of successive changes and modifications indicative of a general advance in biological rank, and also an indication of structural relationship. Furthermore, when the faunas and floras of a given series of formations are compared with those of other series in other parts of the world, it frequently appears that there is a close similarity between those of a certain portion of each series which indicates their correlation. In such cases an order of biological rank is to be observed

similar to that which was observed in the original case. It also frequently occurs that the range of rank is found to be greater in one or both directions than is to be observed in other cases. By such means a knowledge of the order of faunal and floral, as well as of stratigraphical, succession far beyond that which could be obtained in any one region, has been acquired.

It is upon such empirical facts as these that the early geologists based their investigations concerning the chronological arrangement of the sedimentary formations of the earth, and the grand result of which was the adoption of a general scheme and the construction of a corresponding scale for their classification. This scale, which in its present condition is a masterpiece of inductive reasoning, necessarily originated in Europe, because it was there that geology was first systematically studied, and it is there also that its adaptation is more complete than elsewhere.

Although the scale now in use was established before the truth of the progressive evolution of organic forms was accepted by naturalists, and when all differences between those forms was believed to be due to special creations, general progression in average biological rank during geological time was perceived by the early geologists, as well as by those of the present day; but with them it was the perception of a progressive succession in rank of faunal and floral groups of great assemblages of organic forms, and not the recognition of the principle of evolution. Therefore they sought methods of explaining the facts and conditions which they observed with reference to the geological scale which they had established that should accord with the theological views which then prevailed, and which were largely of a supernatural character. Indeed, in the absence of the now prevalent natural method of explaining these facts, the supernatural method of the early geologists seems to have been necessary.

The following deductive propositions which now remind a naturalist of the articles of a creed more than of a statement of scientific principles, are presented as indicating the fundamental ideas held by the early geologists in connection with the construction of the geological scale, and as illustrating the state of prevalent opinion among leading geologists upon biological subjects in their time. It is true that no one author has ever published these propositions in the exact form in which they are here presented, but they have been formulated from the published utterances of numerous authors, and from personal recollections of an active participation in geological work during a number of years, immediately preceding the great revolution in methods of biological thought and investigation which has been referred to. These propositions are:—

(1) That every species of animals and plants, both living and extinct, was specially created, and that they are, and always have been immutable. That genera, and also the higher groups into which both the animal and vegetable kingdoms are systematically divisible, are categories of creative thought, and that they also are immutable.

(2) That although secular extinction of certain species, and [illegible] genera, occurred during every stage of the geological scale, at the close of each stage, except the Tertiary, all life upon the earth was simultaneously destroyed, and that at the close of each sub-stage life was at least in large part destroyed.

(3) That, at the close of each stage coincidently with, and the [illegible] ordained instrument of, the complete extinction of life, there was a universal physical catastrophe, and that the close of each sub-stage was, at least in part, physically catastrophic.

(4) That all life for each successive stage was created anew.

(5) That the life of each stage embraced specially ordained [illegible], or more general, types which were distinctive of and [illegible] to it, and that their distribution was world-wide.

(6) That there was a special ordination of characteristic types for each sub-stage, which received world-wide and simultaneous [illegible] within its narrow time limits.

(7) That no identical, and few similar, specific forms were [illegible] for any two or more stages.

(8) That the world-wide distribution of the distinctive types of animals and plants which were ordained to characterise any [illegible] was effected in connection with the act by which [illegible] faunas and floras were created; or that [illegible] having a world-wide distribution the [illegible] faunas and floras was preserved by the introduction of [illegible] that is, closely similar but distinct [illegible].

(9) That [illegible] the average biological rank of each [illegible] of the next preceding one.

(10) That upon the fossilisable parts of the animals and plants which were created for each stage, and upon those designed to characterise each sub-stage, was impressed not only their own structural features, but recognisable evidence of their chronological ordination.

These propositions represent only those views of the pioneer geologists which pertain to biological geology. Other views which were held by them are unassailable, even in the light of the present advance of science, and their biological views are not introduced here for the purpose of disparagement, but to show that they gave origin to certain erroneous methods which are in part retained as an inheritance by some palæontologists, even though they ostensibly accept the principles of modern biology.

The foregoing propositions relate to what were regarded by the early geologists as fundamental ideas in the construction of the geological scale, while the following relate to those ideas which are now held to constitute its true basis because they only accord with natural laws. These are therefore essentially a counter-statement of the preceding propositions; but the principal object of their preparation is to point out the true relation of biology to systematic, historical, and correlative geology. They consist largely of the statement of certain of the principles involved in the theory of organic evolution, but they are by no means intended as a full statement of those principles, nor are they presented for the purpose of either discussing or defining them as such. That is, the statements are made not for the purpose of formally enunciating these principles, but for the purpose of making practical application of them to the subject in hand. Such of these have been selected for statement and comment as are believed to be accepted by all naturalists who admit the truth of organic evolution, and such application is made of them as will necessarily commend itself to all geologists who admit that truth and its applicability to biological geology.

These propositions are not intended to embrace the whole range of biological geology, but only such of its leading principles as are discussed in these essays. Therefore a certain lack of immediate relevancy will appear in the order in which they are stated.

(1) All species of animals and plants have originated genetically from pre-existing forms, and therefore all are more or less mutable as regards their reproduction. These, together with the various divisions higher than species into which the animal and vegetable kingdoms are divisible, have respectively acquired their distinguishing characteristics by differential and gradually progressive evolution. The extinction of all species and other divisions of the animal and vegetable kingdoms which has taken place during geological time, has always been by natural means and in accordance with natural laws. It has generally been secular and gradual, but in many cases locally or regionally accidental. No universal extinction has ever occurred.

(2) Coincident with the progress of evolution, notwithstanding the retardation, inertion, and even degradation that have occurred along certain lines, there has been during geological time a general average advancement in biological rank of animal and vegetable forms, evidence of which is afforded by certain characteristics of their fossil remains. The evidence of this general advancement constitutes the ultimate standard of measures of geological time as a whole, and the principal means of ascertaining the order of full succession of the events which attended the production of the stratified rocks of the earth.

(3) The chronological features which fossils possess are not of a special character as such, but they are among those upon which their biological classification is based, all of which features have resulted from both progressive and differential evolution.

(4) The average rate of progressive evolution for the different branches or divisions of both the animal and vegetable kingdoms has not been the same for each in all parts of the world, nor the same for all in any one part of the world, during all the time they have coexisted.

(5) The rate of differential evolution among the forms constituting certain divisions of the animal and vegetable kingdoms was greater than that among those constituting other divisions; and it was greater for some of the members of a given division under certain conditions than it was for other members of the same division under other conditions.

(6) The succession of gradual mutations, in the development of the leading classificatory features which characterise certain groups of fossil forms, was not necessarily concurrent with consecutive portions of time.

(7) The progress of secular extinction of species and other divisions of the animal and vegetable kingdoms, including the types which specially characterise the various stages and sub-stages of the geological scale, was accelerated by adverse changes of environing conditions, and were retarded by a continuance of congenial conditions. The final consummation of the extinction of the types was naturally often, and perhaps usually, caused by catastrophic changes of conditions which occurred within the limited areas to which they were reduced by approaching secular extinction.

(8) The geographical distribution of species within the time-limits of the stages and sub-stages of the geological scale, and consequently that of the distinguishing types which the species constitute, has been effected by natural means. Such means included not only locomotory and mechanical dispersion within those time-limits from one original centre which was then the terminus of an evolutional line, but, at least in the same cases, survival in various regions by separate evolutional lines from the faunas of preceding stages and sub-stages was also included.

(9) The animal and vegetable life of each stage of the geological scale was in the aggregate different as to its forms from that of all others, and each stage and sub-stage was further specially characterised by certain generic, and also more general, types or peculiar groups of species. These types, however, were not necessarily confined within absolute time-limits.

(10) Although movements and displacements of the earth's crust have from time to time occurred over large portions of its surface, arresting sedimentation or changing its character and causing great destruction of life, there has never been a universal catastrophe of that kind. On the contrary, during all the time that disastrous conditions prevailed in any given area, conditions congenial to the existence and perpetuity of life prevailed in other and greater areas.

The second of the two sets of propositions show that certain of the views held by the early geologists, notably those which assumed the universally sharp definition of all the divisions of the geological scale, were radically wrong. Still, it is evident to every one who is familiar with modern geological literature that those views have continued to exert an adverse influence upon the biological branch of geological investigation long after they have been formally rejected, even by those who continued to be influenced by them. The early geologists adopted methods of investigation which were consistent with their biological views, but it has been shown that from the present standpoint of biology certain of those views were so fundamentally wrong that the methods which were based upon them are quite out of place in modern investigation. Still, those methods of our energetic predecessors have come down to the present time with such force and with such evidence of the general correctness of the scale which they had established by them, that it has been difficult for their successors to adopt the modification of methods which has been necessitated by the great subsequent revolution in biological thought and methods of investigation.

The facts which have been stated show that, while the scale which the early geologists established is a wonderful production of human reasoning and the best possible general standard which can be adopted before a comparatively full investigation of the geology of the whole earth has been made, it is not, and cannot be except in a general way, of universal applicability. That is, while the respective stages and sub-stages of the scale are recognisable only by means of their characteristic fossil remains, it has been shown that any of those characteristic forms are so liable to range from one stage or sub-stage to another, that it is impossible to sharply define the limits of stages, and often impossible to distinguish sub-stages in one part of the world as they are known in another part.

(*To be continued.*)

SCIENTIFIC SERIALS.

Bulletin de l'Académie des Sciences de St. Pétersbourg, V[e] série, t. ii. No. 2, February 1895.—We notice in the proceedings of the meetings, that the full account of Baron Toll's observations in the New Siberia Islands will soon be published by the Academy. In the meantime the explorer has visited Switzerland in order to study glacier ice, and has found there further proofs, supported by A. Forel, in favour of the masses of ice which he has found in New Siberia (buried under clays containing fossil stems of *Alnus fruticosa* fifteen feet long), really being remains of the ice-sheet which covered the islands during the glacial period.—The yearly report of the Academy, which contains, among other matters, the obituaries of L. Schrenck, A. Middendorff, I. Schmalhausen, and P. Tschebycheff, whom the Academy has lost during the last year.—The positions of 140 stars of the star cluster 20 Vulpeculæ, according to measurements taken from photographic plates, by A. Donner and O. Backlund (in German). The measurements were taken on two plates, one of which had been exposed for twenty minutes only, and the other for one hour, and the accord between the two is most satisfactory, the average difference being 0·00s. in right ascension, and 0"·02 in declination, while the difference between the measurements on the photographic plates, and the direct measurements of Schultz, attains on the average −0·040s. in R.A. and −0"·55 in D.—On the differential equation $dy/dx = 1 + R(x)y$, by N. Sonin.—On a new entoptric phenomenon, by S. Chirieff.—Note on the last mathematic conversation with P. L. Tschebycheff, about his rule for finding the approximate length of a cord, and the means of extending the method to curves of double flexure (all three in Russian).—The ephemeride of the planet (108) Hecuba, by A. Kondratieff.

Vol. ii. No. 3, March 1895.—Yearly reports of the Philological Section of the Academy, and of the committees: for the Baer premium, which was awarded this year to the Tomsk Professor Dogel, for his researches into the histology of the nervous system, and to Prof. Danilevsky for researches into the comparative study of parasites in blood, and the Lomonosov premium, which was awarded to A. Kaminsky for his work on the yearly march and geographical distribution of moisture in the Russian empire in 1871–90.—On the Perseids observed in Russia in 1894 (in French), by Th. Bredikhine. The observations were made by several observers at Odessa and at Kieff. It must be remarked that the observers have had difficulty in observing the meteors, the course of which made a sharp angle with the direction of the vertical line; and this circumstance is probably not without some influence upon the determination of the radiant point. The meteors observed on July 24, 26, and 27, seem to belong to a meteoric stream other than the Perseids. Combining the results of this year's observations (which are given in full in thirteen tables) with the observations of the preceding year, and calculating the elements for each of the radiants, the author sees in them a confirmation of the theoretical results he arrived at in his paper on the Perseids of 1893; the values of the inclination (i) of the centres of radiation—with the exception of the three first, which are somewhat uncertain—are all below the value of i for the comet of 1866. The average value of i before the epoch (August 10·5) is 60°, while after that time it is only 56°; but this decrease cannot be considered as quite real, on account of the said uncertainty in i for July 24–27. An inspection of the charts shows that a condensation of the radiation is taking place towards the epoch which falls on the night of the 10th to the 11th, as seen from the observations made in Italy by P. Denza. The arithmetical average of the coordinates of the three chief radiants of August 10 are $\alpha = 48° 48'$, and $\delta = 56° 30'$, we have: $l = 63° 32'$, $b = 36° 51'$, $i = 64°·8$, $s = 72°·8$, and $V = +34°·4$. The value of i corresponds to the radiant of the comet of 1866. Considerable variations appear in the elements Ω and π; the perihelium is displaced in the direction of the orbital motion of the meteors. In a subsequent memoir the author proposes to take up the theory of the subject, and to evaluate the secular variations of the generating orbit of the comet, and of some of its derived orbits.—On the best means of representing a surface of revolution on a plane, a mathematical treatment of the subject, in Russian, by A. A. Markoff.—On the limit values of integrals, by the same.—List of the works of P. L. Tschebycheff.—On the methods for correctly determining the absolute inclination by means of the induction inclinator, and the degree of exactitude lately obtained with this instrument at the Pavlovsk Observatory, by H. Wild (in French).—The non-periodical variations in the quantity of precipitation at St. Petersburg, by E. Heintz (in Russian, summary in French).—Ephemeride of the planet (209) Didon, by Mme. Eugénie Maximoff.—Determination of the magnitudes of the stars in the star cluster 20 Vulpeculæ, by Mme. Marie Shilow. The diameters were measured by the micrometer, and the corresponding magnitudes were calculated by means of Charlier's formula.—On one sum, a mathematical note (in Russian), by I. Ivanoff.

THE numbers of the *Journal of Botany* for May to July contain, besides mere technical papers, one on the genu

Argemone, by Dr. D. Prain, a description of a new species of *Bryopsis*, and of a peculiar mode of growth in another species, by Miss E. S. Barton; an account of fossil plant-remains in peat, by Mr. A. Gepp; and a description of a large number of new species of Orchidaceæ, by Mr. A. B. Rendle, from the plants brought by Mr. Scott Elliot from Tropical Africa.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 16.—"The Complete System of the Periods of a Hollow Vortex Ring." By H. C. Pocklington.

May 30.—"The Kinematics of Machines." By Prof. T. A. Hearson.

In this paper it is shown that all machine movements, however complex, are derived from the association together of some of a comparatively limited number of kinds of simple motions, which take place between consecutive directly connected pieces.

Certain geometrical laws are enunciated, from which are derived the conditions necessary for the association of those motions together in one machine. It is shown that those laws preclude the existence of certain combinations of motions. By attaching to each kind of motion a suggestive symbol a method of expressing the constitution of a machine movement by a simple formula is proposed, whereby similarities and differences between machines may be exhibited at a glance.

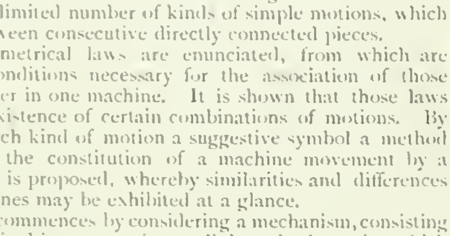
The author commences by considering a mechanism, consisting of four bars united in one continuous linkage by four pins which have parallel axes. By imagining the length of the links to undergo variation from zero to infinity, it is shown that this mechanism is representative of all the simple plane mechanisms, and, by imagining other variations to occur, it is shown to be representative of still further classes of mechanisms, in which the parts do not move in or parallel to one plane. In this the relative motions of consecutive pieces are either turning, when one piece revolves completely around relatively to the other, the representative symbol being the letter O, or swinging, when one piece turns through a limited angle relatively to the adjoining one, represented by the letter U.

O Group.

O Group.

O Group.

U Group



The first law enunciated, which governs the association of the O and U motions, is founded on the geometrical fact that the sum of the four angles of the quadrilateral is constant. After a complete revolution the angle between the bars is considered to have been increased or diminished by 2π.

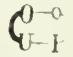
From this it is impossible for only one motion to be turning and the other three swinging, otherwise the sum of the four angles would increase or decrease by 2π each revolution.

The second law, which governs the association of the motions, has to do with the proportions between the length of the links necessary to permit of complete turning. This is founded on the fact that one side of a triangle cannot be greater than the sum of the other two. From these two laws together it is shown that it is impossible to have two Os alternating with two Us.

Next it is pointed out how the U motion may be provided for by constructing a circular slotway in one piece, and shaping the other piece to fit the slotway, so that by imagining the radius of curvature of the slotway to be indefinitely increased a relative movement of reciprocating sliding motion, represented by the symbol of letter I, will be substituted for the swinging motion U. A slide being conceived to be a swing through a zero angle about an infinitely distant centre, the previously mentioned laws will apply to associations containing I motions, and it will follow that a combination of three slides and one swing is precluded by the first law.

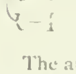
By the application of the governing laws 14 distinct combinations are found to be possible, and only 14. They are exhibited by the following formulæ, in which a large O associated with a small o signifies that in one case adjacent links turn relatively to one another so as to continuously increase the angle between them, and in the other to continuously diminish the angle. The double ◎ signifies that two complete revolutions accompany one complete to-and-fro swing or slide.

Applying Reuleaux's principle of "Inversion" it will be seen that 32, and only 32, distinct machine movements can be derived from the above 14 mechanisms. Those from the same mechanism are distinguished from one another in the formula by using a thick line for the frame link. For example,

 signifies a machine movement like that employed in the crank-and-connecting-rod engine.

 is exemplified in the oscillating engine much used in paddle-wheel steamers.

 is found in Stannah's pendulum pump, and

 quadrupled is the movement adopted by Rigg in the design of his high speed engine.

The author next discusses the relation of cams and spur-wheel mechanisms to the foregoing kinematic chains, showing that they are the result of the suppression of one of the previous four links and the amalgamation of the two adjoining simple motions into one more complex. A comparison is also made with belt gearing, and expressive formulæ suggested.

The author then passes to the consideration of machines the parts of which do not move parallel to one plane.

The first 13 of the previously mentioned mechanisms have their counterpart in mechanisms the parts of which move parallel to the surface of a sphere. Hooke's joint is the best known example. The 14th consisting of 3 slides cannot be adapted to a sphere but it can to a cylinder, and from it are derived 4 possible screw mechanisms.

The remaining mechanisms consist of those in which the axes of the turning and swinging motions neither meet nor are parallel. They include the motion which occurs at a ball-and-socket joint. The method of classification according to the proposed scheme is summarised as follows:—

All simple machine movements may be ranged in four divisions, viz.:—

(1) Consisting of plane mechanisms, in which the pieces move in or parallel to the surface of a plane.

(2) Spherical mechanisms, in which the pieces move in or parallel to the surface of a sphere.

(3) Cylindrical mechanisms, in which the pieces move in or parallel to the surface of a cylinder.

(4) The remainder, to which the name conoidical mechanisms is given, in which the axes of the swinging and turning motions neither meet nor are parallel.

The mechanisms in each of these divisions are classed in two subdivisions.

Subdivision S, with surface contact of consecutive links.

Subdivision P, with point contact of consecutive links.

The mechanisms in each of the eight subdivisions are still further subdivided into combinations. The combinations of 1_s, 2_s, and 3_s, are exhaustively enumerated, and it is suggested that an extension of the methods of applying the geometrical laws would lead to the preparation of an exhaustive list of the possible combinations in the other subdivisions. The combinations are still further subdivided into inversions according to Reuleaux's principle of the inversion of a machine.

Lastly, the author proceeds to show how the foregoing considerations assist in the analysis of compound mechanisms. It is assumed that practically all compound mechanisms contain a continuous mechanism A, of not more than four links, from which definiteness of relative motion of all the other links is derived. Any two links of A in their exact length, or longer or shorter, may be adopted to form with two new links a second mechanism B, and any two of A or B, or one of A and one of B, may be adopted to form with two still further added links a third mechanism C, and so on. In this way a definiteness of relative motion of many links in a compound mechanism is derived. The notation lends itself to a clear exhibition of the

manner in which two or more simple mechanisms are associated together, and the compound mechanism built up.

June 20.—"The Influence of the Cerebral Cortex on the Larynx." By Dr. J. S. Risien Russell.

The author found the condition of the peripheral laryngeal apparatus has practically no effect on the result obtained from the central nervous mechanism, for abduction or adduction of the vocal cords resulted on excitation of the appropriate area of the cerebral cortex, irrespective of whether abduction or adduction was obtained on excitation of the recurrent laryngeal nerves in the same animal. No evidence of unilateral representation of the movements of the vocal cords in the cerebral cortex was obtained, although this point was tested in various ways. Nor was it found possible to inhibit the abductor muscles by excitation of the cortical centre of their antagonists the adductors. It was found that both in the dog and cat there existed a focus, excitation of which resulted in adduction of the vocal cords, and another near to this, stimulation of which resulted in abduction of the cords. While in the cat it was possible to differentiate these movements without any preliminary measures being adopted, it was only after the adductor fibres of one recurrent laryngeal nerve had been divided transversely that it first became possible to evoke abduction of the vocal cords on excitation of the cortex, though in subsequent experiments it was sometimes possible to evoke this movement on excitation of the cortex of the dog without adopting this preliminary measure. The other effect on the cords, which it was as a rule found most difficult to differentiate from that of abduction, was acceleration of their movements. It was further found that on the anterior composite gyrus, below the abductor centre, there existed a focus, excitation of which resulted in a clonic adductor effect on the cords, in which the cords were first brought into a position of moderate adduction, and then there was added rapid short to-and-fro excursions. On passing within the confines of Spencer's area for arrest of respiration, it was found that in the peripheral parts of this area there existed three foci, excitation of which affected the cords in different ways. The most anterior was responsible for arrest of the cords in adduction, *i.e.* in the expiratory stage of their excursions; excitation of the focus behind this, and corresponding, probably, to Horsley and Semon's abductor centre in the cat, was followed by arrest of the cords in abduction, *i.e.* their inspiratory position; while the most posterior focus, which is situated at about the junction of the anterior composite and anterior sylvian convolutions, resulted in intensification combined with acceleration of the movements of the cords when stimulated. Excitation of Spencer's chief focus for arrest of respiration on the olfactory lobe, resulted in arrest of the cords in the position they occupy during expiration in the dog, and in the position they occupy during inspiration in the cat.

Physical Society, June 28. Dr. Gladstone, Vice-President, in the chair.—Mr. Bowden read a note on an electro-magnetic effect. A long glass tube containing mercury, and fitted with a small stand-pipe to indicate hydrostatic pressure, is passed between the poles of an electro-magnet. On passing a current of about 30 amperes through the mercury in this tube, the stand-pipe being turned so as to indicate the pressure either perpendicular or parallel to the lines of force of the field of the electro-magnet, movements of the mercury in the stand-pipe take place. When the stand-pipe is perpendicular to the lines of force of the field, the mercury rises or falls according to the direction of the current. When the stand-pipe, however, is parallel to the lines of force, the mercury always *rises*, whatever the direction of the current. Prof. S. P. Thompson said there appeared to be three unexplained effects, one proportional to the current and the field, and reversible; another, independent of the direction of the current, or of the field; and a third, which only occurred while the current was changing in strength. In addition there may be a fourth effect, which up to now has not been noticed. The motion of the mercury column in Fig. 1 of the paper was in the opposite direction to that of the drag on a conductor carrying the current. An apparent *rise* in pressure might be due to a decrease in the density of the mercury due to the heat developed by the current. Mr. Blakesley asked if the author had noticed any changes in level in the mercury reservoirs at the ends of the tube. The author, in his reply, said the reservoirs at the ends were so large that no changes of level were appreciable. Mr. Rhodes read a paper on the armature reaction in a single phase alternating current machine. In this paper the author gives the investigations that were the subject of a verbal addendum to a paper read before the Society on a previous occasion. He investigates the lag or lead of the E.M.F.s over the current, and applies the results to examine whether the field excitation of the generator or the motor is strengthened or weakened by the reaction of the armature currents. Mr. Tunzelmann expressed a hope that the author would amplify parts of his paper. Mr. Blakesley said the conclusion of the author that "either of two alternate current machines may be driven as a motor by the other, irrespective of their relative E.M.F.s," is not invariably correct. The facts of the case were these: the E.M.F. of the motor may exceed that of the other machine to a certain extent; but that E.M.F., multiplied by the cosine of the angle of electric lag, must yield a product not greater than the E.M.F. of the generator; *i.e.* using Mr. Rhodes' symbols $e \cos \theta$ must not be greater than E. Mr. Blakesley gave a geometrical proof of this, but the same proposition had been given by him some ten years ago in the course of investigating the subject generally. This was at a time when Dr. John Hopkinson was, with less than his usual perspicuity, teaching that synchronous alternate current machines could not be run in series with stability, both doing work. Referring to the author's diagrams, Mr. Blakesley said that in a problem involving so many elements as that under consideration, it was impossible with the limited dimensions of space to represent the results with the complete generality of a formula. Some elements had to be taken as the independent, others as the dependent variables. The author had considered the power transmitted to the motor, the E.M.F. of the generator and the angle of electric lag as independent. The E.M.F. of the motor was dependent. In Mr. Blakesley's original diagrams the E.M.F.s were both considered independent as well as the electric lag, and the powers applied or transmitted as dependent variables. In any case the formulæ properly derived from such diagrams became perfectly general, and it did not appear to him that the change of method indicated could properly be called a new theory on the subject. As a matter of fact, diagrams based on the independence of the E.M.F.s and the electric lag would furnish a better means of discussing the question of the stability of the motion than Mr. Rhodes' plan, and this might account for the entire omission from the paper of this important matter. Prof. S. P. Thompson said it was impossible to discuss the question of stability till the subject of armature reaction had been thoroughly investigated. The terms lag and lead had been used by Mr. Rhodes in a consistent manner; but this was not always done, and he recommended that the phase of the current which was common to both generator and motor be taken as the standard. The author, in his reply, said he agreed with Mr. Blakesley that there was a limit to the extent to which the motor might be excited, and this upper limit could easily be obtained from the figure given in the paper. The question of armature reaction was, however, most important, as it might excite the field two or three times more than the original excitation. Since motors were designed to do a certain amount of work, and not the work to fit the motor, it was most natural to take the output of the motor as fixed. Mr. Shelford Bidwell read a paper on the electrical properties of selenium. The author has continued his investigations on this subject, and has come to the following conclusions: (1) The conductivity of crystalline Se appears to depend principally on the impurities which it contains in the form of metallic selenides. It may be that the selenides conduct electrolytically, and that the influence of light in increasing the conductivity is to be attributed to its property of facilitating the combination of Se with metals in contact with it. (2) A Se cell having platinum electrodes, and made with Se to which about 3 per cent. of cuprous selenide has been added is, even though unannealed, greatly superior both in conductivity and sensitiveness to a similar cell made with ordinary Se and annealed for several hours. (3) Red Se in contact with copper or brass, is quickly darkened by the action of light, owing, it is suggested, to the formation of a selenide. (4) Crystalline Se is porous and absorbs moisture from the air, and it is this moisture that causes the polarisation of Se after the passage of a current. (5) The presence of moisture is not essential to sensitiveness, but appears to be in a slight degree favourable to it. (6) If cuprous selenide is made the kathode in an electrolytic cell, and a strip of platinum the anode in water, red Se mixed with detached particles of the selenide is deposited in the water. (7) The photo-electric currents sometimes set up when light falls upon Se, are dependent upon the

... voltaic origin. So perfectly dry Se is below platinum in the thermo electric series. Prof. Minchin (communicated) suggested that the selenium "cell" should be called a selenium "resistance." A grid having one terminal made of aluminium and the other of copper, might form a true cell, and might generate an E.M.F. when light fell on it. He (Prof. Minchin) would like to know if the author had tried any such cell in which light simply and solely generated an E.M.F. He could not agree that chemical action must necessarily follow the action of light in a cell. For, take the case of the oldest photo-electric cell—the thermopile—what chemical action can we show here for all the energy of the solder heat. Chemical action due to light may, or may not, occur according to the nature of the cell. Mr. Appleyard asked whether the author had submitted these selenium resistances to the action of electric oscillations. Prof. Minchin's "impulsion" cells were greatly influenced by electric oscillations. The great variation in the resistance with time of the author's cells pointed rather to an effect of contact between the selenium and the electrodes, than to an elementary change in the structure or composition. He (Mr. Appleyard) had recently tried to crystallise a supersaturated solution of sodium sulphate by electric oscillations, as well as by direct sparks, and by currents of several amperes, but no crystals could be induced to form. Change of contact, rather than change of structure, appeared to him to be the most promising direction in which to look for an adequate theory of selenium resistances. Prof. Ramsay said the quantity of Se liberated in the electrolytic experiment was much too great to be accounted for by oxygen dissolved in the water. The study of Se was very interesting, for this substance was on the borderland between those bodies in which the electric conduction was metallic, and those in which it was known to be electrolytic. The author, in his reply, said he agreed that the name "selenium cell" was not an appropriate one. He had not tried the effect of electric oscillations. The Society then adjourned till the autumn.

PARIS.

Academy of Sciences, July 1.—M. Marey in the chair. The President announced the decease of Prof. Huxley, Correspondant of the Anatomy and Zoology Section.—On photographs of the moon and new objects discovered by means of them, by MM. Lœwy and Puiseux.—On an extensive class of linear partial differential equations, of which all the integrals are analytical, by M. Emile Picard.—Laws of extinction of a simple wave on the high seas, by M. J. Boussinesq. The coefficient of extinction (with the distance) of a simple wave is inversely proportional to the fifth power of its demiperiod T.—On the estimation of minute quantities of arsenic, by M. Ad. Carnot. The arsenic is separated in the usual manner as sulphide, this is dissolved in hot ammonia and treated with silver nitrate and hydrogen peroxide. The solution is then precipitated by bismuth nitrate, followed by ammonia, the accompanying bismuth hydrate is dissolved out by nitric acid (1 nitric acid of sp. gr. 1·33), and, finally, the bismuth arsenate is dried at 110° and weighed.—Truffes (Terfas) from Morocco and Sardinia, by M. Ad. Chatin.—Comparison of the heating of the muscles in the cases of positive and negative work, by M. A. Chauveau. During negative work, descent or lowering, the temperature of the muscles concerned was raised to a notably less degree than during corresponding positive work, ascent or raising.—Contribution to the study of arable soil. Quantities of air and water contained in soils of earth, by M. P. P. Dehérain.—On the products of sulphonation of benzylidenecamphor and benzylcamphor. Nitrate or nitronitrite of benzylidenecamphor, by M. A. Haller.—A new instrument (tacheograph) serving to survey and trace directly from the earth's surface, by M. Schrader.—On curves traced on a surface, of which the osculating sphere is tangential at each point to the surface, by M. E. Cosserat.—On linear equations to the derived partials, by M. Étienne Delassus.—On the integration of ordinary differential equations, by M. Alf. Guldberg.—On the propagation of sound in a cylindrical tube, by MM. J. Violle and Th. Vautier.—On the apparent attractions and repulsions of electrified conductors in a dielectric fluid, by M. Gouy. The apparent forces exercised between conductors with given charges in a liquid dielectric result: (1) from their mutual attractions and repulsions, the same as in a vacuum; (2) from the hydrostatic pressure produced by the force which attracts the dielectric in the sense where the intensity of the field increases most rapidly.—New method of measurement of electric capacities based on the sensitiveness of the skin, by M. H. Bordier.—On the solubility of superfused liquids, by M. Louis Bruner. The author finds that superfused sodium thiosulphate is much more soluble in alcohol than the corresponding solid compound.—On the specific heat of superfused salts, by M. Louis Bruner. The curve of specific heats at different temperatures for sodium thiosulphate shows a maximum near the point of fusion, 48° C.—On paratungstic acid, by M. L. A. Hallopeau.—On the estimation of alumina in phosphates, by M. Henri Lasne. A method of precipitation of pure aluminium phosphate is described, which avoids the complications introduced by the use of molybdate or citrate in estimating alumina. The precipitation is accomplished by the use of ammonium thiosulphate.—On sodammonium, by M. de Forcrand. A thermo-chemical study.—On the phosphoric esters of allyl alcohol, allylphosphoric acid, by M. J. Cavalier.—Preparation and conductibility of new methyl alkyleyanacetates, by M. J. Guinchant.—Verification of Tschermak's law relative to plagioclases, and a new process of orientation and of diagnosis of felspars in thin plates, by M. A. Michel-Lévy.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Matriculation Directory, No. xviii., June, 1895 (University Correspondence College).—Allen's Naturalist's Library. A Hand-book to the Game Birds, Vol. 1: W. R. Ogilvie-Grant (Allen).—Missouri Botanical Garden, Sixth Annual Report (St. Louis, Mo.).—Iowa Geological Survey, Vol. 3, 2nd Annual Report, 1893 (Des Moines).—An Analysis of Astronomical Motion: Dr. H. Pratt (Norman).—Report of the International Meteorological Congress held at Chicago, Ill., August 21-24, 1893, Part 2 (Washington).—Ice-Bound on Kolguev: A. Trevor-Battye (Constable).—Wild England of To-day: C. J. Cornish (Seeley).—Thirteenth Annual Report of the Fishery Board for Scotland. Part 1. General Report (Edinburgh).

PAMPHLETS.—Report on the Loss of Gold in the Reduction of Auriferous Veinstone in Victoria: H. Rosales (Melbourne).—Royal Gardens, Kew, Hand-list of Herbaceous Plants (Eyre).—Great Eastern Railway Company's Tourist Guide to the Continent (3 Fleet Street).

SERIALS.—Geological Magazine, July (Dulau).—Scribner's Magazine, July (Low).—Jahrbuch der K.K. Geologischen Reichsanstalt, Jahrgang 1894, xliv. Band, 2, 3, and 4 Heft (Wien).—L'Anthropologie, Tome vi. No. 3 (Paris).—Science Progress, July (Scientific Press, Ltd.).—Proceedings of the Bath Natural History and Antiquarian Field Club, Vol. 8, No. 2 (Bath).—Annals of Scottish Natural History, July (Edinburgh: Douglas).—Journal of the Sanitary Institute, July (Stanford).—Himmel und Erde, July (Berlin).—Blackwood's Magazine, July (Blackwood).—Transactions of the Leicester Literary and Philosophical Society, April (Leicester).—Mind, July (Williams).—Botanische Jahrbücher, Zwanzigster Band, 5 Heft (Leipzig).

CONTENTS.

THURSDAY, JULY 18, 1895.

ANALYSIS OF OILS, FATS, AND WAXES.

Chemical Analysis of Oils, Fats, and Waxes, and of the Commercial Products derived therefrom. From the German of Prof. Dr. R. Benedikt. Revised and enlarged by Dr. J. Lewkowitsch, F.I.C., F.C.S. (London: Macmillan and Co., 1895.)

TEN or twelve years ago, the analysis of oils was one of the most neglected branches of analytical chemistry. How the study of it has been taken up and developed since, may be gathered by turning over the 670 pages of this excellent volume, the first English work devoted exclusively to this subject. The information existing in 1882 was comprised within 140 pages (much smaller than these) of Allen's "Commercial Organic Analysis." In the second edition of the same work, published in 1886, the subject-matter had grown to 318 pages. Benedikt's "Analyse der Fette und Wachsarten," second edition, published in 1892, upon which the present work is based, contained 460 pages, and as the literature of the subject has accumulated since then, at an increasingly rapid rate, it is evident that a new volume was demanded, the preparation of which could not have devolved upon any one more capable than Dr. Lewkowitsch, whose practical experience in, and valuable contributions to, our knowledge of this branch of chemistry are well known. To regard this work merely as a translation of the work of Dr. Benedikt would, obviously, be absurd. As the author points out in the preface, every page bears evidence of the alterations and numerous additions which have been made. Obsolete processes have been abridged or entirely omitted, and the new work of the last four years has been sifted, and all that is of value has been incorporated, including a large number of the author's own experiments and observations hitherto unpublished. Benedikt's arrangement of the subject-matter has been generally adhered to, but an improvement has been effected by transferring to the end of the book the chapter on the analysis of soap, candles, glycerine, and other products of the fat industry.

The first two chapters contain a description of the sources and chief properties of the various acids and alcohols obtained, or derived by oxidation, from the fats and waxes, followed by an account of the chemical constitution and the chief chemical and physical characters of the oils, fats, and waxes themselves. Commercial fats and oils are not pure neutral bodies, but always contain more or less free fatty acids which, for some purposes, depreciate their value. The percentage of free acid is liable to increase on keeping, and it was until recently believed that the development of rancidity was connected with this change. But Ballantyne has disproved this by showing that an oil may become rancid without becoming acid, and Heyerdahl has proved that the converse may also be true. The discovery, by Kirchner, of micro-organisms in poppy-seed oil, lent support to the view that rancidity might be the result of a fermentation process; but Ritsert showed that a fat which had been sterilised by heating to 140° C., might subsequently become rancid if exposed to light and air. The latter investigator has also shown that moisture is by no means essential, and he has finally concluded that rancidity must be due to the direct oxidation of the oil or fat by the oxygen of the air acting in presence of light.

Chapter iii. describes the determination of water and other non-fatty admixtures, and the preparation of the pure fat for analysis. Then follow a chapter on the physical properties and methods of examining fats, and four chapters on chemical methods. In the two next chapters the application of the foregoing, and some other methods, to the examination of fats is discussed, and data obtained by submitting the various oils, fats, &c., to examination by each method are collected and arranged in tables. This, however, is hardly shown by the headings of the chapters. Thus, chapter iv., which is headed "Physical Properties of Fats and Waxes," should rather be "Physical Properties and Methods of Examining Fats and Waxes"; and chapters ix. and x., headed "Systematic Examination of Liquid and Solid Fats and Waxes," with the sub-headings "Physical Methods" and "Chemical Methods," would be better entitled "Application of the foregoing Methods to the Systematic Examination," &c., with sub-headings "Application of Physical Methods" and "Application of Chemical Methods." These eight chapters are admirably written, and the value of the information given is greatly enhanced by the able manner in which each method is discussed and criticised. The completeness of the treatment shows how thoroughly the author has ransacked the literature of the subject. Unfortunately the task of reading and sifting papers is rendered heavier than need be by the growing tendency to rush into print with trivial and ill-considered observations. Thus, "the excellent *Reichert-Meissl* process has not escaped the fate of nearly all modern methods used in fat analysis" (there is no need to limit the statement to fat analysis, as the literature of steel analysis would show), "viz., to receive at the hands of numerous analysts a number of supposed improvements, most of which are altogether insignificant and hardly offer any advantage whatever." Again, referring to the Hübl process: "The chemical literature of the last few years contains numerous papers by various authors purporting to give improvements or modifications of the original method. Most of these refer to minor and unimportant points, and some of them even reproduce methods which Hübl in his classical paper has rejected."

For the determination of unsaponifiable matter, the author recommends petroleum spirit in preference to ether, but he very rightly insists upon the necessity of carefully purifying and rectifying the spirit used. If this is not done, some of the lighter mineral oils occasionally used to adulterate rape oil, for instance, may be lost, and for that reason I prefer to use ordinary ether, which can be completely expelled at a very moderate temperature.

For the determination of resin, Twitchell's process is recommended as yielding the best results, but no process yet exists by which resin can be determined with absolute accuracy.

"If a correct method of determining accurately the oxygen absorbed were known, it would be possible to class the determination of the drying power, or, as it might be called, the 'oxygen value' amongst the quantitative reactions." Such an addition to existing methods would be of the greatest value in the examination of the

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various oils from cruciferous and other seeds which now pass under the name of "rape oil."

In the eleventh chapter, which extends over 273 pages, the natural oils, fats, and waxes are systematically arranged and separately described, a very excellent and most valuable feature being a series of tables appended to the description of each oil, fat, and wax, giving the physical and chemical constants (1) of the oil itself, (2) of the mixed fatty acids, and (3) of the wax alcohols. It is a pity these tables were not arranged so as to be readable without having to turn the book half round, which might have been done by cutting each table in half. No less than 106 oils, &c., are thus separately described, and their physical and chemical constants are collected and arranged in about 175 tables. The usefulness of these tables to the analyst cannot be over-rated, though it does not appear to be clear in all cases by what method the melting and solidifying points of the fatty acids were determined. The "saponification values" are expressed per mille, and the iodine and other values per cent., but there is no reason why the simpler plan of expressing all the quantitative values in percentages should not be adopted. The section on butter fat, the analysis of which was the first to be placed upon a scientific basis, occupies twenty-three pages.

In chapter xii. the analysis of the raw materials and products of the fat and oil industries is treated, and in the concluding chapter some examples of the interpretation of results are given; but space does not admit of further reference.

This book is unique: the analyst will find in it practically all the available information upon the subject up to date, with full references to the original papers; and it will increase the author's already high reputation.

L. ARCHBUTT.

TRACES OF A DELUGE.

On Certain Phenomena belonging to the Close of the last Geological Period, and on their bearing upon the Tradition of the Flood. By Joseph Prestwich, D.C.L., F.R.S., &c. (London: Macmillan and Co., 1895.)

HAD the story of the Deluge a foundation in fact; in other words, is it a record of some inundation which affected a considerable area of the earth's surface? This is the question which Prof. Prestwich sets himself to answer in the small volume before us—a volume which combines a paper read to the Victoria Institute with some of the material communicated to the Royal and the Geological Societies.

In the south of England, especially in the neighbourhood of the coast, a drift is often found, varying in thickness from a few inches to a few feet, which consists of angular fragments of rock with loam derived from adjacent higher ground, and lies on the slopes of the hills and at the bottom of the valleys. Frequently it is absent, but where hollows occur in the surface of the underlying rocks, it has accumulated in greater quantities, and occasionally even exceeds eighty feet in thickness. In some localities it rests on an old raised beach, and is banked up against a buried sea cliff; in others it fills up fissures in the rocks. In the last case it frequently contains the bones of mammals, many of them now extinct—at any rate in Britain. These are neither worn nor gnawed, but are commonly broken and split. Its fossils, almost without exception, are of terrestrial origin. Similar deposits occur in the Channel Isles and on the French coast, and in many places around the Mediterranean, not to mention others. What is the origin of this "rubble drift," "head," osseous or fissure breccia?

Prof. Prestwich refers all these deposits to one epoch of very limited duration. He supposes that there was a rather widespread subsidence, amounting, in some places, to a few hundred feet, during which the sea overflowed the lower land. This was sufficiently rapid to make the invading water muddy; then, before the marine molluscs had time to establish themselves in the new territory, the land was upheaved by jerks (with intervening pauses). These sudden disturbances of its bed set up currents in the sea, strong enough to sweep heavy débris, and even largish blocks of rock, from the higher to the lower ground, and to precipitate the material into any open fissures. By this tumultuous action the bones of the terrestrial mammals which had been drowned by the submergence would be dispersed and shattered, and it explains, in his opinion, all the phenomena better than any other hypothesis. As man was living at the time, it gave rise to the tradition of the Flood.

An adequate discussion of Prof. Prestwich's hypothesis is impossible in our limited space; but we may be permitted to remark that it is not free from serious difficulties. Many geologists would dispute the assumption that these deposits all belong to one and the same epoch. Others will doubt whether the sudden upheavals, which he postulates, would be adequate to produce currents, capable of moving the larger débris, or whether the earth movements would suffice, as he supposes, to make gaping fissures. Some will think that he hardly appreciates the effect of "cloud bursts," such as may be seen in many mountain and even lowland districts of Europe, in transporting débris very similar in character to the "head." It is admitted that since this was deposited denudation has wrought some changes in the contours of the country, and this may explain the apparent isolation of some patches of the "head," whether it fill fissures or cap tabular hills. In several cases the ordinary explanation of breccias (admitting as an adjunct the action of snow) seem to us more simple than that proposed by Prof. Prestwich, and his mode of accounting for the abundance of hippopotamus bones at San Ciro, near Palmero (that as the land sank they were embayed between its precipitous face and the advancing sea, and at last were drowned) can hardly be called probable. Lions and hyænas might have perished in that way, but the hippopotamus seems far from helpless in the water, and is likely to have saved itself.

We think, then, that Prof. Prestwich's hypothesis will be received with some scepticism; nevertheless, it demands careful consideration as an attempt to solve a very difficult problem, which is put forward by one who may now be termed the Nestor of British geologists, and who has paid especial attention to questions of this nature.

AN ECLECTIC HISTORY OF SCIENCE.

Progress of Science. By J. Villin Marmery. Pp. 357. (London: Chapman and Hall, Limited, 1895.)

THE custom of inserting laudatory prefaces or introductions, written by well-known men, in works of science by lesser lights, which was commented upon in these columns a few weeks ago, reaches the ridiculous in the case of this book. A letter from Mr. Samuel Laing to the publishers is printed, in the course of which he says: "I have now had time to read Mr. Marmery's book, and find it a work of great learning and research . . . and I can confidently recommend it as alike interesting and instructive." What induced the publishers to print this purely business letter as a testimonial to the book's good qualities, passes our comprehension. A book usually finds its proper level, and the effort to force it into a higher position by means of a letter of introduction from a more or less distinguished individual, must prove futile: for in literature, scientific or otherwise, authors are judged entirely by their own works.

Every one will recognise that to attempt to condense the history of science into a volume of about three hundred and fifty pages, is to court failure. All that can be accomplished in so small a space is to describe the well-defined steps of advancement along the road of natural knowledge, and to exhibit the continuity of scientific developments. Mr. Marmery has done this with a fair amount of success. After briefly noting the knowledge of the Egyptians and Chaldeans so far back as 3000 B.C., he surveys the successive stages in the history of science, and devotes a few lines to men and matters of first-rate importance in each. His statement of the progress made by the Arabians from the ninth to the fifteenth centuries, brings into view the substantial achievements of a people which then stood in advance of the whole world. Our obligations to the Arabs are indubitable; nevertheless, few European historians have expressed them. "Eminence in science is the highest of honours" was a maxim which represented the bearing of Islam towards scientific knowledge at a period when Europe was ruled by monkish philosophy, and when investigators were stamped as heretics.

The review of the science of the Greek, the Arabian, the Mediaeval, and the Revival periods, leads to the science of the Modern period, from the end of the sixteenth century to the present time. And here the author treads upon dangerous ground. He has had, perforce, to create invidious distinctions by selecting from the host of scientific workers those that appear to him to have added most to the store of knowledge. Huxley got over the difficulty in his address on "The Progress of Science," published among his collected essays (vol. i.), by omitting references to all living men, and by dealing only with results. Mr. Marmery might have saved himself from hostile criticism by following the same method; but, in that case, his volume would have wanted the very information which is the chief justification for its existence. His selection of names has, he says, been determined "by what appears *typical originality* in the work, rather than by what is imposing in extent and weight." Here and there we fancy this criterion has not been applied; but in a book covering so wide a scope, such deficiencies may well be excused. Modern investigators are divided into seven groups, viz. (1) biologists, (2) geologists, (3) chemists, (4) mathematicians, (5) astronomers, (6) physicists, (7) eminent practical men. Short accounts of the main achievements of the individual workers in each group are given, and are fairly trustworthy. In an appendix, the names of foremost men of science in all the periods are tabulated, and a copious index makes it easy to find the sketch of the works of any one of them.

Many imperfections the book certainly has, but in spite of them we think it deserves some words of commendation. Those who wish to know something about the evolution of scientific knowledge, and the multitude of readers who like to dip into a book to find what this or that man of science has done, may obtain from this handy volume the information they seek. We could easily enumerate a score of names which ought to find a place in the book, but are wanting. Probably it was because the author was aware of the incompleteness of his record, that he omitted the definite article from the title of his book.

MICROSCOPIC STUDY OF ROCKS.

Petrology for Students: an Introduction to the Study of Rocks under the Microscope. By A. Harker. (Cambridge University Press, 1895.)

THIS latest addition to the Cambridge Science Manuals is intended by the author as a guide to the study of rocks in thin slices under the microscope In scarcely another English text-book on the subject has the treatment of rocks from the purely petrographical point of view of microscopic examination been so strictly adhered to throughout as in the book before us. "Microscope" is almost the first word in the book, and sounds the key-note of the whole.

After a short introduction, containing a few notes on the optical properties of minerals, the author plunges at once into the systematic description of the different rock species. The usual chapters on the characters and methods of separation and determination of the rock-forming minerals are omitted altogether; for all such mineralogical points, the reader is referred to standard works on the subject. The book, therefore, corresponds, though on a much smaller scale, to the second volume of such text-books as those of Rosenbusch and Zirkel.

In the classification of the massive igneous rocks the author divides them into *plutonic*, *intrusive* and *volcanic*, but is careful to point out that the divisions themselves are based upon the *structural* characters resulting from the different conditions of consolidation. This classification resembles that of Rosenbusch, but the author's intrusive groups do not correspond exactly with the Ganggesteine of Rosenbusch, for he extends them to the basic family, whereas even Rosenbusch considered this to be impracticable. In this connection we notice that those much abused terms "diabase" and "porphyrite" receive new definitions. Diabase is in this book used to designate, not pre-Tertiary or altered dolerites, but the group of intrusive basic rocks corresponding to the volcanic basalts, while porphyrite is applied to the intrusive rocks corresponding to the volcanic andesites. The author, of course, follows the British school in admit-

ting no criterion of geological age in the nomenclature of the rocks.

Throughout the book, each rock group is treated for the most part under the three headings: constituent minerals, structure, illustrative examples. Under the last heading, purely petrographical descriptions are given of typical examples, chosen generally from British rocks.

The sedimentary rocks are treated under the divisions, arenaceous, argillaceous, calcareous, and pyroclastic. In perhaps no other English text-book have the microscopic characters of the sedimentary rocks been so minutely described. The subject of metamorphism is treated under the two heads of thermal metamorphism and dynamic metamorphism, and the effects produced on arenaceous, calcareous, argillaceous and igneous rocks are separately described. The book concludes with a short chapter on various crystalline rocks, including gneisses, granulites, &c. It is, perhaps, almost inevitable, owing to the nature of the subject, that the book should give the general impression of consisting of a series of descriptions of rock-sections; but, be this as it may, there can be nothing but praise for the clear and straightforward way in which the author has presented his facts, and for the wealth of new matter which the book contains. The book shows evidence of most careful research into the literature of the subject, and is in fact thoroughly up to date, containing many extracts from papers which have appeared within the present year.

G. T. P.

OUR BOOK SHELF.

Garden Flowers and Plants: a Primer for Amateurs. By J. Wright. With fifty Illustrations. (London: Macmillan and Co., 1895.)

ONE of the great advantages of gardening and of a love of flowers consists in the fact that they may be indulged in by rich and poor alike.

The rich have no monopoly in the beauty of flowers, the poor are not debarred from their enjoyment. The costliest orchid in a ducal garden is not one whit more beautiful than an Iris which may be bought for a few pence. If a slug devour the one it is easily replaced, if such an accident befall the other the loss may be beyond repair. Nor by those who look beneath the surface and seek to penetrate the significance of the diversity of form, and the meaning of the beauty they witness, is costly expenditure needed. The cheapest and commonest afford as copious materials for research and investigation as the dearest plant in the nurseryman's price list. Anything that will lighten the sordid conditions under which so many of the poorer classes live, anything that will brighten their homes and give them an interest in something beyond their daily toil, must be considered as a boon of incalculable value. Such a boon is offered by the pursuit of gardening. In country districts, moreover, where small gardens and allotments can be had, gardening may be made to add considerably to the resources of the family. It may be doubtful whether market gardening on a large scale will always be profitable, but there can be no doubt that the small plot of the labourer may be turned to good account, provided circumstances are even only moderately favourable. To provide for the needs of small gardeners and amateurs, Mr. Wright has published the little manual before us. The author is an accomplished practitioner, and his experience as a County Council lecturer has enabled him to ascertain precisely what is wanted by his auditory. Mr. Wright begins at the beginning by telling his readers how to make a garden, how to lay down gravel walks, what to grow on walls, what on beds, even what may be cultivated in areas. The principal categories of hardy plants are passed in review, such as annuals, perennials, bulbous plants, bedding plants, and so on, and clear directions are given as to their management from beginning to end. In all this there is not much that needs comment from a reviewer, who can only say that the little primer is well done, and excellently suited for its purpose.

An explanation of the real cause of "damping" off would have been of value, as the most "practical" of gardeners is not desirous of cultivating fungus at the expense of cherished seedlings.

The small illustrations are helpful, and a full index adds materially to the value of the book.

The Time Machine. By H. G. Wells. (London: Wm Heinemann, 1895.)

INGENIOUSLY arguing that time may be regarded as the fourth dimension of which our faculties fail to give us any distinct impression, the author of this admirably-told story has conceived the idea of a machine which shall convey the traveller either backwards or forwards in time. Apart from its merits as a clever piece of imagination, the story is well worth the attention of the scientific reader, for the reason that it is based so far as possible on scientific data, and while not taking it too seriously, it helps one to get a connected idea of the possible results of the ever-continuing processes of evolution. Cosmical evolution, it may be remarked, is in some degree subject to mathematical investigations, and the author appears to be well acquainted with the results which have been obtained in this direction. It is naturally in the domain of social and organic evolution that the imagination finds its greatest scope.

Mounted on a "time-machine" the "time-traveller" does not come to a halt until the year eight hundred and two thousand, and we are then favoured with his personal observations in that distant period. In that "golden age," the constellations had put on new forms, and the sun's heat was greater, perhaps in consequence of the fall of a planet into the sun, in accordance with the theory of tidal evolution. "Horses, cattle, sheep, and dogs had followed the ichthyosaurus into extinction"; but, most remarkable of all, "man had not remained one species, but had differentiated into two distinct animals," an upper-world people of "feeble prettiness," and a most repulsive subterranean race reduced to mere mechanical industry. It is with the time-traveller's adventures among these people, and their relations to each other, that the chief interest of the story, as such, belongs.

Continuing his journey to an age millions of years hence, nearly all traces of life had vanished, the sun glowed only with a dull red heat, tidal evolution had brought the earth to present a constant face to the sun, and the sun itself covered a tenth part of the heavens. These and other phenomena are very graphically described, and from first to last the narrative never lapses into dulness.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Teaching University for London.

I HAVE read with surprise your article on the University of London.

Probably by some accident you had not seen my reply to Lord Kelvin's letter when you went to press. I now enclose a copy, and trust to your fairness to insert it:

"*2 St. James's Square, S.W., July 9.*

"MY DEAR RÜCKER,—I am sorry I could not immediately answer the letter which you have forwarded to me on behalf of

Lord Kelvin and other members of the Royal Society, but I only received it this morning, as I was away from home. I observe that most of those who have signed it are (as they themselves say) not members of Convocation, and consequently not constituents of mine. Still, I should welcome any opportunity of co-operation with such high authorities in the promotion of those interests which we all have at heart. I regret, however, that before publishing the letter they did not give me an opportunity of conferring with them, in which case, I think, I could have given good reasons for what I have said in my letter to Prof. Foster. I am glad to observe that the only point objected to is the reference of any new charter to Convocation. In this, however, I am not asking that any privilege which they do not at present possess should be conferred on my constituents, but only supporting what is now their legal right. As the law now stands no change can be made in the charter without the consent of the graduates. This right I know they highly value, and it is surely natural that, as their representative, I should do my best to preserve it. Moreover, in view of the difficulty of passing a Bill strongly opposed, as any Bill would be, which seeks to abrogate the present right of veto possessed by Convocation, I can imagine nothing more likely to wreck any scheme such as you desire than to link it, quite unnecessarily, with an attack on that right. Your objection to the reference to Convocation implies the belief that a Statutory Commission would arrange a wise charter for the University, and that the graduates would reject it. But why should it be assumed that they would do so? It has been my proud boast that I represent a constituency second to none in education and ability, and I am sure you will not, on reflection, be surprised if I have every confidence that when any new charter is submitted to my constituents, they will exercise the rights well and wisely, and with an earnest wish to further the interests of Learning and Education.

"I am, yours very sincerely,
"JOHN LUBBOCK."

I must also ask you to let me say a few words on your own article.

In the first place, I have not "accepted the views" of those who altogether oppose the Reorganisation Scheme. Some, indeed, of the modifications suggested seem to me important improvements, but that is a very different thing.

You say that Convocation is only one of the bodies affected. In the case of the Colleges and Medical Institutions certain privileges are granted, but the University is the only body whose constitution it is proposed to change.

At present, this cannot be done without the consent of Convocation, and you blame me for endeavouring to maintain that right. Your whole article assumes that the Commissioners will make a wise scheme, and then you allege that a reference to Convocation would wreck it. This, however, is an attack on my constituents and not on me. JOHN LUBBOCK.

High Elms, July 15.

The Density of Molten Rock.

IN a review of Lord Kelvin's "Geology," in NATURE, July 26, 1894, vol. l. p. 292, the question of whether solid rock sinks or swims in molten rock was left open for further experimental evidence.

My impression is that this was in accordance with the views of the writer of the book; but if I had had proper acquaintance with the work of Mr. Carl Barus, of the Smithsonian Institution, Washington, I should at least have referred to it. Permit me to do so now, and to give the references:—*Am. Journ. of Science*, 1893, vol. xlv. p. 1; *Phil. Mag.*, 1893, vol. xxxiv. p. 1; vol. xxxv. pp. 173 and 296; also certain *Bulletins* of the U.S. Geological Survey, particularly No. 103, which contain the most complete account. OLIVER J. LODGE.

The Earliest Magnetic Meridians.

IN NATURE of June 6, p. 129, Captain E. W. Creak, F.R.S., questions a statement of mine with regard to this subject, as published in NATURE of May 23, p. 80. I there credited Yeates instead of Duperrey with the first construction of the magnetic meridians for the whole earth. I was careful not to say that Yeates *originated* the idea of magnetic meridians.

Euler, to my knowledge, about the middle of last century, appears to have first appreciated the importance of those lines from a theoretical standpoint. He defines them as those curves on the earth's surface, the tangents to which mark out the actual direction of a compass needle. He did not actually construct them, however, if I remember correctly.[1]

It was my belief then that Yeates first drew these curves, as based upon observations. Captain Creak, however, thinks that John Churchman deserves this honour.

So far as I know, Churchman published but two magnetic charts or atlases, one in 1790, the other in 1794. The chart referred to by Captain Creak is the earlier one, if I mistake not. A text to this chart was also published, called "An Explanation of the Magnetic Atlas," Philadelphia, 1790. It was my belief that this was an isogonic chart a chart giving the lines of equal variation—not a chart of the magnetic meridians. Churchman's later work, "The Magnetic Atlas or Variations Charts," London, 1794, contains charts which, according to Prof. Hellmann, are more theoretical. Prof. Hellmann mentions and briefly describes both of Churchman's charts, and gives the impression that they are isogonic charts.[2]

As I have no means at present of verifying this matter, may I ask Captain Creak to make further examination, and state if Churchman's magnetic meridians are based upon observation? L. A. BAUER.

The University of Chicago, June 29.

Curious Habit of the Spotted Flycatcher.

I HAVE been watching, at intervals during the last week, a pair of Spotted Flycatchers feeding their young in a nest on a ledge of the wall of this house. The nest is embowered by a very free blossoming white rose. I noticed to my surprise the parent birds again and again, after taking food to their offspring, plucking off the petals of the rose near the nest, and transporting them to an acacia tree about ten yards distant, where they let the petals drop upon the ground. The rose blossoms are now quite cleared away from the neighbourhood of the nest, and the lawn beneath the acacia thickly strewn with them.

The rose flowers do not obstruct the approach to the nest, to which the birds have access by running a short distance along the ledge. It is also difficult to suppose that the object of the birds is to admit more air and light to the nest, which is more open to the sunlight than very many nests of this species which I have found. Moreover, the birds take no trouble to remove any of the dead leaves which are near the nest, having an objection, as it appears, only to the blossoms of the rose. I can offer no explanation of this curious conduct of the flycatchers. W. CLEMENT LEY.

Tellack Vicarage, Ross, Herefordshire, July 11.

A Brilliant Meteor.

ON Sunday, July 7, about 10.45 p.m., I observed a meteor of rather peculiar character. Contrary to the general method of appearance of these objects, it came into view very gradually, and its motion was so uniform and slow that its form could be clearly discerned.

The meteor was double, the two components being about $\frac{1}{4}°$ apart, but travelling together, the smaller one being ahead of the larger. The combined magnitude was probably equal to that of Venus as seen earlier on the same evening.

Some trace of trail could faintly be made out, but this was rendered uncertain by the sky being very luminous in consequence of the moon's position near the meridian at the time.

While visible the meteor travelled about 20° in a path approximately parallel to the horizon, and a rough estimate of its position would be:

	R.A.	Decl.
Appearance ...	... 13h. ...	+ 20
Disappearance	... 11h. 30m. ...	+ 35

No explosion of any kind was noticed, nor any accompanying sound. CHARLES P. BUTLER.

Royal College of Science, July 9.

Newton and Huygens.

UPON Newton's conception of the universe, space is considered to be void. A fluid or gas would oppose resistance to the motion of the planets, and however small this resistance might be, it would cause a diminution of the linear velocity of the planets. The central attraction being unchanged, a diminution of the linear velocity of the earth

[1] See Gehler's "Physikalisches Woerterbuch," article "Magnetismus."

[2] "Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus," No. 4, p. 22.

would cause an augmentation of its angular velocity around the sun. The period of revolution would take less time, and the length of the year would gradually decrease. Observation proves that this is not the case, and the necessary conclusion is, that there is no resisting medium in space, which must be, therefore, considered as perfectly void.

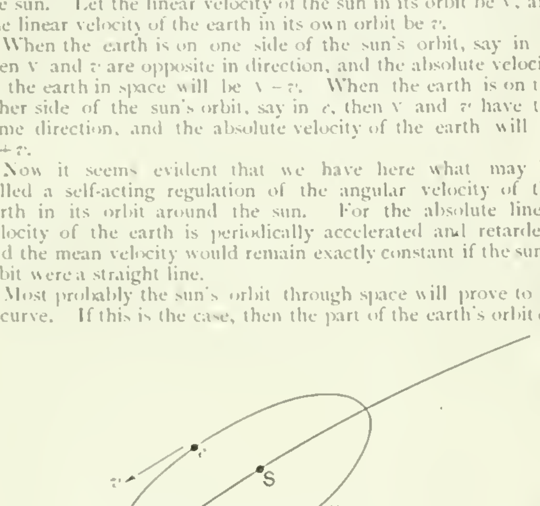
There is no objection to be made to this reasoning so long as we suppose the sun immovable in space, which was the generally accepted belief in Newton's time. But we know at present that the sun, with all the planets, has a motion through space: and this knowledge changes the conditions of the problem, as may be demonstrated by what follows.

In the accompanying figure, S is the sun in a certain point of its orbit in space. E is the earth in a certain point of its orbit around the sun. Let the linear velocity of the sun in its orbit be V, and the linear velocity of the earth in its own orbit be v.

When the earth is on one side of the sun's orbit, say in E, then V and v are opposite in direction, and the absolute velocity of the earth in space will be $V - v$. When the earth is on the other side of the sun's orbit, say in e, then V and v have the same direction, and the absolute velocity of the earth will be $V + v$.

Now it seems evident that we have here what may be called a self-acting regulation of the angular velocity of the earth in its orbit around the sun. For the absolute linear velocity of the earth is periodically accelerated and retarded, and the mean velocity would remain exactly constant if the sun's orbit were a straight line.

Most probably the sun's orbit through space will prove to be a curve. If this is the case, then the part of the earth's orbit on



the concave or outer side of the sun's orbit will be somewhat longer than the part on the convex or inner side.

If this be so, then the acceleration on the outside part will be somewhat greater than the retardation on the inner side of the earth's orbit. Thus the surplus velocity, gained in each revolution around the sun, will compensate the loss of linear velocity which the earth might suffer in its yearly orbit around the sun by the resistance of a supposed medium in space.

It might, however, be asked, Why it is that this compensation is so exact as we find it to be? For Laplace, in his well-known work on the "Système du monde," explains clearly that no change in the period of revolution of the earth around the sun has been observed.

But we may quite as well wonder why the temperature of our blood is nearly constant; and the best answer to such questions is in the well-known words: "Philosophy does not ask what agrees, but what is."

The sun's motion in space is a discovery with far-reaching consequences for science in general; and if space be allowed, a few other corollaries must follow upon it. For the present, it is better to limit research to the single question as to whether we may admit the existence of a resisting medium in space. The answer is that the discovery of the sun's motion in space allows us to settle this much disturbed question in a positive sense.

This result has a particular value, because it takes away the contradiction between two theories which are both generally admitted. The undulatory theory of light, which was first enunciated by Huygens, supposes the existence of an elastic medium in space. When it is demonstrated that the supposition of this medium is not incompatible with Newton's theory of central forces as applied to our planetary system, this must certainly be considered as a step in advance.

A. HUET.

Delft, Holland, July 5

THE INTERNATIONAL CATALOGUE OF SCIENTIFIC PAPERS.

THE following report of the International Catalogue Committee was presented to the President and Council of the Royal Society on July 5, and the recommendations contained in it were approved.

At the first meeting of this Committee (February 8, 1894), the Memorial to the President and Council (July 1893) which led to the appointment of the Committee, and the Minute of Council of December 7, 1893, appointing the Committee, having been read, it was resolved to request the President and Council to authorise the Committee to enter directly into communication with societies, institutions, &c., in this country and abroad, with reference to the preparation, by international co-operation, of complete subject and authors' catalogues of scientific literature.

Subsequently, a draft circular letter was prepared, which, on February 22, 1894, received the approval of the President and Council, who also authorised its issue.

This letter was sent to 207 societies and institutions selected from the exchange list of the Royal Society, and to a few others. It was also sent to the Directors of a number of Observatories and of Government geological surveys, to the Foreign Members of the Royal Society, as well as to those of the following Societies: Chemical, Geological, Physical, Royal Astronomical, Linnean, Royal Microscopical, Entomological, Zoological, Physiological, and Mineralogical, and of the Anthropological Institute. A special letter was addressed to the Smithsonian Institution.

More than a hundred replies to the letter have been received; several of these are reports of Committees specially appointed to consider the suggestions put forward by the Royal Society. A list of answers received up to December 1894, with brief excerpts from the more suggestive, was issued to members of the Committee early in this year. It should, however, be added that from some important institutions no answer has as yet been received.

It may be said at the outset that in no single case is any doubt expressed as to the extreme value of the work contemplated, and that only two or three correspondents question whether it be possible to carry out such a work. It is a great gratification to the Committee that the matter has been taken up in a most cordial manner by the Smithsonian Institution, the Secretary of which, in his reply, refers to the desirability of a catalogue of the kind suggested as being so obvious that the work commends itself at once. The importance of having complete subject catalogues, and not mere transcripts of titles, is also generally recognised.

Some bodies and individuals take the matter up very warmly and urge that steps be taken forthwith to put the scheme into action, this being especially true of the replies received from the United States; others, while giving a general approval, dwell upon the difficulties of carrying out the suggestions put forward; and others, again, ask for more details before committing themselves to any answer which may seem to entail future responsibility, especially of a financial character.

Incidentally it may be pointed out as very noteworthy that over and over again reference is made to the great value of the Royal Society's "Catalogue of Scientific Papers." There is abundant evidence that considerable use is made of this on the continent of Europe. And it is clear that a proposal to carry out a more comprehensive scheme initially under the direction of the Royal Society of London is likely to meet with general approval owing to the fact that the Society is credited with having already carried out the most comprehensive work of the kind yet attempted. Indeed, the Academy of Natural Sciences of Philadelphia, U.S.A., directly advocates the

establishment of a central bureau under the Royal Society; and several others more or less clearly imply that they would favour such a course.

Over and over again, it is stated that the production by international co-operation of a catalogue such as is contemplated is not only desirable, but practicable. The Americans who, as already stated, are the most enthusiastic supporters of the scheme, especially dwell on the importance of early action being taken. Prof. Bowditch, of Harvard University, in particular, points out that if the Royal Society of London wish to guide the enterprise, it ought to announce its views and put forward a comprehensive scheme with the least possible delay. It may be added here that he also urges that in determining the scope of the catalogue a very wide interpretation should be given to the word "Science."

No very precise information as to the best mode of putting the scheme into operation is to be gathered from the replies as a whole.

It is generally agreed that the enterprise should be an international one. Many think that international financial support should and would be accorded to it, but no method of securing this is indicated; others express the view that the cost may be met by subscriptions from societies, libraries, booksellers and individuals without Government aid, and this is, perhaps, on the whole, the prevailing feeling among those who have discussed the matter from a financial point of view. But in no case is any attempt made to form any exact estimate of the cost.

A number of scientific bodies and institutions express themselves prepared to work in such a cause. The Secretary of the Smithsonian Institution suggests that as the Institution receives all the serials and independent works published in America, a branch office might be established there, and that it is not impossible that a sum of money might be given yearly in aid. The Royal Danish Academy is willing to render as much assistance as possible. It would charge an official of one of the Danish chief libraries in receipt of all Danish publications with the task of editing slips, and would defray the cost of this work. The Société des Sciences of Helsingfors would furnish the central office with information as to the scientific work done in Finland. The Kongl. Vetenskaps Akademie of Stockholm would organise a Committee for Sweden.

As regards language, there appears to be more unanimity than could have been expected. Over and over again the opinion is expressed that English should be the language of the subject catalogue. Frequent reference is made to the importance of quoting titles in the original language, although some suggest that this should be done only in the case of those published in English, French, or German, and perhaps Italian.

Some form of card catalogue appears to be generally favoured, especially in America, as the basis of the scheme; the Committee of Harvard University, whose reply is very full, in particular discuss this point in detail.

In an interview with the Committee in March last, Prof. Agassiz spoke very warmly in favour of the scheme, and of the support which it would meet with in the United States, especially from libraries. As others have done, he strongly urged that the co-operation of booksellers and authors should be secured. Prof. Agassiz also expressed the view that the regular issue to libraries and scientific workers from the central office of cards or slips which would afford the material for the construction of card catalogues would form an important source of income, at all events in his country.

From various sides it is urged that an International Congress should be held to discuss plans. This is advocated as a first step in a reply received from the Königl. Gesellschaft der Wissenschaften in Göttingen, a reply to which, not only as regards this point, but also in respect to the whole matter, the Committee attach very great weight, since it embodies in an official form views arrived at by the academies of Vienna and Munich, and by the scientific societies of Leipsic and Göttingen, who have considered the matter in common. Prof. Agassiz strongly urged the calling of a conference, and among others who share this view, Dr. Gill, of the Cape Observatory, in his letter particularly dwells on the great value of such meetings as the means of securing unanimity of action.

Such being the tenour of the correspondence, your Committee are convinced that initial steps of a definite nature in furtherance of the scheme ought now to be taken.

They accordingly request the President and Council to take measures with the view of calling together, in July of next year (1896), an International Conference, at which representatives of the several nations engaged in scientific work should be invited to attend, with the view of discussing and settling a detailed scheme for the production by international co-operation of complete authors' and subject catalogues of scientific literature.

London will probably be found the best place in which to hold such a conference. It may be desirable to summon the representatives of the different countries through their respective Governments, and it will obviously be necessary that a detailed scheme be prepared, to serve as a basis for discussion at the conference. These and other points will require much consideration before any action at all can be taken; meanwhile, it is desirable that a beginning should be made during the autumn, before the winter session of the Society. The Committee therefore recommend that the President and Council should give the Committee (which includes the President and officers) executive powers in order that they may take, in the name of the Society, such steps as they may think desirable with the view of calling together the above-mentioned conference.

SCIENCE SCHOLARSHIPS AT CAMBRIDGE.

WITHIN the past academical year an attempt has been made by the college tutors at Cambridge, in consultation with representatives from Oxford, to come to an understanding as to the times at which examinations for entrance scholarships shall be held. Headmasters have frequently complained of the interruption to school work caused by the present somewhat haphazard arrangements, and have suggested the grouping of colleges and other expedients in mitigation of the difficulty. Some of the colleges, notably Caius, Jesus, Christ's, and Emmanuel, Pembroke with King's, and Clare with Trinity Hall, have agreed to group their examinations, candidates entering for the combined examination being required to indicate the colleges, in the order of their preference, which they desire to join if successful. The larger colleges, Trinity and St. John's, have for various reasons found it impracticable to form such combinations; but they have agreed at least to avoid clashing by fixing their examinations about a month apart. Nine of the colleges offer scholarships and exhibitions for natural science, the rest confining the competition to the old-established subjects of classics and mathematics. In the ensuing academical year, examinations in natural science for these awards will be held as follows: at Trinity, November 5; at Peterhouse (physical sciences only), November 19; at the group Caius, Jesus, Christ's, Emmanuel—November 26; at the group Pembroke, King's—and also at St. John's College, December 3; at Sidney, Sussex, December 12; at the group—Clare, Trinity Hall—January 1; and at Downing, about March 17. The value of the scholarships varies from £80 to £40 a year, of the exhibitions from £50 to £20. They are usually tenable for three or four years, with a condition that by the end of the second year the scholar shall have

approved himself sufficiently in the college examinations. Scholars are practically required to become candidates for honours in the natural sciences tripos, though the new mechanical sciences tripos will no doubt attract some. The new Salamon scholarships at Caius are, indeed, specially intended for students of engineering. It should be added that candidates for scholarships, who are not yet members of the university, must be under nineteen years of age; there is no restriction of age in respect of the science exhibitions. Though only nine colleges specifically offer entrance scholarships in science, an examination of the awards to the first, second, and third year students shows that in many more good work in science, as tested by university or inter-collegiate examinations, does not go unrecognised. The large body of medical students, now approaching five hundred in number, is distributed over all the colleges, and their presence has apparently brought home, even to the most conservative, the fact that intellectual ability, high-minded devotion to study, and social energy are not confined to students of classics and mathematics alone. Thus, though something remains to be done in certain quarters in the direction of placing science on an equal footing with the older subjects as a fit object of college recognition and reward, it must be owned that a great advance has been made within the last ten years. The natural sciences tripos now attracts a larger number of candidates than any other, and this notwithstanding that its standard has steadily been raised. In the majority of the colleges, distinguished eminence in this tripos has been admitted as a qualification for a fellowship, and in not a few instances governing bodies have felt the need of strengthening themselves on the side of science, and have departed from Cambridge custom by selecting scientific members of other colleges for this honour.

The endowments for research, other than scholarships and fellowships, have in late years been substantially increased. In addition to post-graduate studentships at the larger colleges, such as the Hutchinson at St. John's (physical and natural science), the Coutts-Trotter at Trinity (physics and physiology), the Frank Smart at Caius (botany), the university has of late received a number of benefactions for the same purpose. The Balfour studentship in animal morphology, worth £200 a year, the Harkness scholarship in geology about £100, the Clerk Maxwell scholarship in physics about £185, the John Lucas Walker studentship in pathology £200 to £300, the Isaac Newton studentships (three) in astronomy £200, and the Arnold Gerstenberg studentship, for natural science students pursuing philosophical study, about £55, are among these recent foundations. They are expressly intended to foster advanced study and research, and they have already produced excellent results. The university still lacks the means of providing similar encouragements for higher work in chemistry, in anatomy and anthropology, in botany, in mineralogy, in physiology, in pharmacology, and in scientific engineering. It is to be hoped that the line of generous benefactors is not yet extinct, and that some of these important subjects may ere long receive the benefit of their munificence. The new scheme for the promotion of post-graduate study and research, which has received the approval of the senate, and now only awaits the assembling of Parliament for the sanction of the necessary statutes, will render such endowments opportune and fruitful.

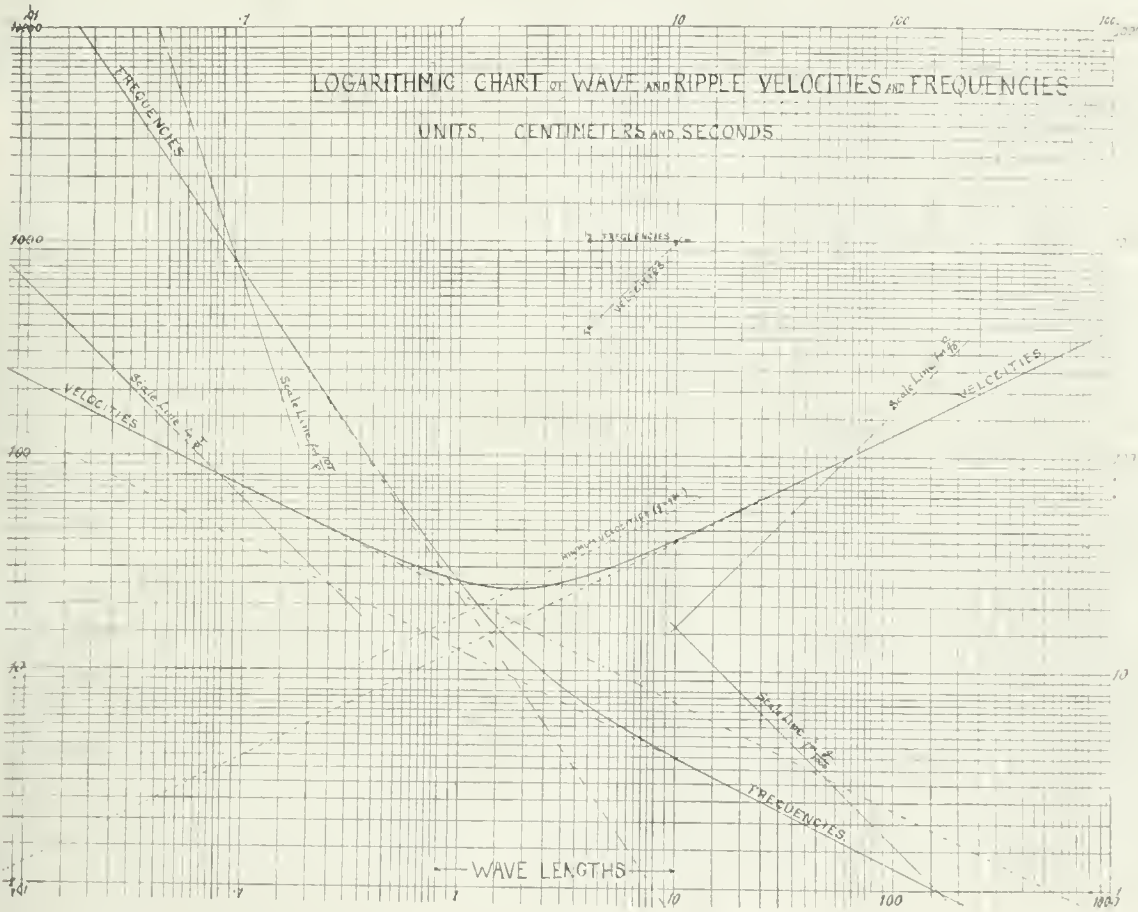
SCALE LINES ON THE LOGARITHMIC CHART.

THE advantages of logarithmic plotting for certain classes of work have for some time been recognised, and now that, thanks to Mr. Human, logarithmically ruled paper can be obtained ready made, the facility of such plotting is greatly increased, so that there is all the more reason on this account why it should become more common than it seems to be at present. It may perhaps be well to point out shortly what the nature and effect of logarithmic plotting is, and to contrast it with the more common method on square-ruled paper. Instead of paper ruled in equal squares, logarithmic paper is ruled first in a series of large equal unit squares representing tenfold changes in the coordinates. Thus two units represent 100, three units 1000, and so on. Similarly the squares are broken up fractionally and unequally into a series of vertical and horizontal lines, whose distance from the left or lower side of the square is equal to the logarithms of the numbers 2, 3, 4, &c., and these are subdivided again logarithmically just in the same way that a slide rule is subdivided. In fact, if logarithmic paper is not available, logarithmic plotting can still be carried out fairly expeditiously by pricking off distances direct from a good slide-rule. The meaning of lines drawn upon logarithmic paper is very different from that upon ordinary square ruled paper. For instance, an inclined straight line ruled in the ordinary way represents the equation $y = a + bx$, whereas when logarithmic paper is employed the corresponding line gives $y = ax^b$. The consequence is that whenever two quantities are related so that one varies as any power, positive, negative, integral, or fractional of another, a straight line drawn in the proper position and inclination represents that relation, the power being equal to the trigonometrical tangent of the angle of slope of the straight line. If the relation that is to be represented is less simple, if the index changes gradually as either of the coordinates changes, so that a curve has to be employed, then the size and shape of the curve represents the law in the abstract, and the position of the curve on the sheet the actual numbers for the particular case and with the particular units; a mere shift of the curve bodily upon the chart, as pointed out by Prof. Osborne Reynolds long ago, being all that is necessary to adopt the same law to new circumstances or new units.

One very important feature of logarithmic plotting is the fact that, not only is it practicable to include an enormous range (in Mr. Human's sheets of four by five squares of 10,000 and 100,000 in the two directions), but the proportionate accuracy is identical in all parts, if it is possible to draw or read to, say, 1 per cent. in one part of a curve, the same figure is true everywhere. On the other hand, in ordinary plotting the proportionate accuracy of quantities near the origin is very small, while at a great distance it becomes enormous. In order to assist in the process of sliding any curve about on a logarithmic chart so as to represent particular cases, special logarithmic scales may be ruled upon the sheet, having a suitable magnitude depending on the index which connects the result with the new variable, or what I have called scale lines may be employed. In illustrating the laws which connect the velocity and frequency of waves and ripples at the Royal Society soirée, I exhibited these lines, and showed how, in order to determine by inspection either the velocity or the frequency of waves and ripples of any wave-length on the surface of any liquid under any acceleration of gravity, a single curve and two scale lines are all that are needed. As by their use the logarithmic chart is made even more comprehensive than it is at present, I feel that no apology is needed for making use of the columns of NATURE to make them more widely known.

As is well known, the velocity of surface waves on a fluid depend both on gravity and on kinematic capillarity or capillarity divided by density. In the case of waves of large size, capillarity is of practically no account, and the velocity depends only on the acceleration of gravity. Since it depends on the square root of this acceleration, the line on the logarithmic chart that represents the velocity of waves of any size travelling under the influence of gravity alone is straight, and slopes up so as to rise one square for every two that it moves to the right, its tangent is

$= \frac{1}{2}$. On the other hand, since the velocity of waves travelling under the influence of capillarity alone is proportional to the square root of the wave shortness or reciprocal of the wave-length, the line that represents their velocity is straight also, but slopes the other way and to the same extent. Actually both causes are in operation, but except over a range of wave-length of about 1 to 100, the one influence so largely predominates that the other is negligible. In the diagram this is made evident on the velocity curve which consists of two straight branches joined by a curve, which runs into them and is rapidly indistinguishable from them. The dotted continuation of the branches shows what would be the velocities under the influence of either cause alone. Where the two lines cross, both gravity and capillarity have equal influence, and the two together



produce a minimum effect. The actual curve may be planted anywhere on the chart; but in the diagram it is so placed as to represent the facts with water for which $T/\rho = 81$ and with $g = 981 \cdot 1$. If other liquids are chosen, then, since the gravitational branch of the curve is unaffected, the curve, as a whole, must be made to slide along the gravity branch on the right until the left branch assumes its proper position. Similarly to represent the effect of changes of g, the curve must be made to slide along its capillary branch on the left until the gravity branch on the right assumes its proper position. The proper position in either case may be indicated by a special logarithmic scale ruled to half the scale adopted for the squares of the chart, and placed upon the chart with its length vertical and so that the branches of the curve cut each scale, one at 81 and the other at $981 \cdot 1$. The *scale line*, however, is much simpler, more convenient, and less confusing. In order to draw it, find a point in either branch of the curve where the velocity reading on the vertical scale of the chart is equal to the value of T/ρ for the left, or of g for the right branch. If within the limits of the paper the branch of the curve does not indicate a velocity, of which the value is T/ρ or g, as the case may be, take some whole power of 10 or $\frac{1}{10}$ as a factor. For instance, though $T/\rho = 81$ is within the limits of the left branch, $g = 981 \cdot 1$ is outside the paper on the right, therefore find on the right branch $g/10 = 98 \cdot 11$. Now, in order to find some other point on the scale line, imagine that each of these quantities is multiplied tenfold. The corresponding branch of each will be raised vertically $\sqrt{10}$, or half a square. The new line so drawn will at some point cut the vertical scale of the chart, in a line of which the value is ten times the reality, or is one square higher up than the first point. Mark this point, and join it to the first. The result is a *scale line* having the property that wherever it is cut by the corresponding branch of the curve the reading on the chart gives at once the value of T/ρ or of g, as the case may be, that is proper to the new position of the curve, and conversely in order to fix the place of the curve for any value of T/ρ or of g, it is merely necessary to find the desired values of these quantities on the scale lines, and then to shift the curve until its two branches or its two branches produced if necessary pass through the points on the scale lines having the values sought. If the effect of a variation of the constant upon the value indicated by the curve line is one of simple proportion, the scale line will be vertical. If its power is less than one, it will be

between the vertical and the slope of the curve; if more than one, it will slope the other way, if it is negative the slope will be less than that of the curve. In order to apply a general rule to all possible cases where both the index connecting x and y and the index connecting the result (y) with the variation of the constant may have any values whatever, it is merely necessary to find a point a upon the inclined straight line representing $y = ax$, at which y is equal to a, or to that part of a which may assume various values. At any distance above it rule a horizontal line. Where the horizontal line cuts the inclined line, write the figure 0; where it cuts the vertical through the point a, write the figure $+1$. Then complete a scale of equal parts on the horizontal line extending to $+\infty$ and $-\infty$. Lines drawn through the original point a and any point q on this scale will be scale lines corresponding to the case where the result depends upon the qth power of the constant as well as upon the bth power of x.

The frequency curve placed upon the same chart has two branches inclined at $\tan^{-1} - \frac{3}{2}$ and $\tan^{-1} - \frac{1}{2}$, joined by a curve such that not only on the straight branches, but at every point, the algebraical difference of the tangent of its inclination and that of the velocity curve for the same value of x the wave-length is equal to unity. The left-hand branch of the frequency curve supplies another example of the rule given above for drawing a scale line: for, while its tangent is $-\frac{3}{2}$, that of the scale line is -3.

It is evident that the curve may be conveniently drawn upon tracing-paper, which may then be moved about, but always keeping the inclinations unchanged until the branches pass through the desired points upon the scale lines. The numerical relations for the new constants may then be read at once.

I have thought it best to explain the method by the use of a concrete example. Of course it is not limited to the case of ripples and waves, but may be applied very widely.

By way of illustrating how to change from one system of units to another, I have drawn a pair of double-ended arrows in the middle of the chart, which show the magnitude and direction of the movement of each of the curved lines with its straight dotted continuations, which will be necessary in order to read the results in inches instead of in centimetres. The one relating to velocities is inclined at 45°, as both the velocity and wave-length are equally changed in the ratio of 2·54 : 1, or 1 : ·3937. On the other hand, the frequency being a mere number is not affected, except in so far as the wave-length will be expressed by a different number. Hence the direction of sliding is here horizontal and the same in amount as either component of the other. The scale lines must then be put in parallel to their former directions, and running through points for which the vertical scale reading has the numerical value of the constant according to the system of units chosen.

C. V. BOYS.

[*Note.* The numerical values represented by the vertical and horizontal lines in each square in the diagram are 1, 1·5, 2, 2·5, 3, 3·5, 4, 4·5, 5, 5·5, 6, 6·5, 7, 7·5, 8, 9, 10. The number of lines in the Human sheets is five times as great, but they are drawn in three degrees of darkness to distinguish them.—C. V. B.]

NOTES.

A REPRESENTATIVE meeting of friends and admirers of the late Mr. Huxley was held on Thursday afternoon, at the rooms of the Royal Society, under the chairmanship of Lord Kelvin, F.R.S., to consider what steps should be taken to initiate a national memorial. It was determined to call a general public meeting after the autumn recess, and, in the meantime, to form a general committee. Sir John Lubbock (15 Lombard-street) has consented to act as treasurer, and Prof. G. B. Howes (Royal College of Science, South Kensington) as secretary to the provisional committee.

WE notice, also, that it is proposed to establish a memorial to commemorate the connection of Huxley with the Charing Cross Hospital Medical School. At a meeting held at the School on Tuesday, the following resolution was passed: "That the memorial shall take the form of a Huxley scholarship and medal to be awarded annually at the Charing Cross Hospital Medical School, and that if funds permit an annual public lecture at the Charing Cross Medical School dealing with recent advances in science, and their bearing upon medicine shall be instituted."

WE understand that a large majority of those Fellows of the Royal Society who have expressed an opinion on the matter, being in favour of retaining the present quarto form of the *Philosophical Transactions*, the President and Council have decided to retain that form. As stated in a circular recently addressed to Fellows, the President and Council, finding that the majority of those expressing their opinion were in favour of a royal octavo form for the *Proceedings*, have decided to adopt that form. The change will probably be made at the beginning of next year.

SEVERAL new instances of generous gifts for the advancement of scientific knowledge are reported in *Science*. Mr. Archibald, President of the Trustees of Syracuse University, has offered to be one of six subscribers for funds to build a hall of science costing about £30,000. The University has also been offered £2000 and £20,000 towards a new medical college. Another American institution which has benefited by the epidemic of generosity which has lately prevailed in the United States is the Johns Hopkins University, which has received from Mrs. Williams a sum of money sufficient to establish a lectureship in geology in memory of the late Prof. George H. Williams. Sir Archibald Geikie has been invited to be first lecturer.

THE sum of £50,000 required for the New York Botanic Garden has been contributed by twenty-two donors. Subscriptions of £5000 were given by each of the following:—Mr. J. P. Morgan, Columbia College, Mr. Andrew Carnegie, Mr. C. Vanderbilt, Mr. J. D. Rockefeller, Mr. D. O. Mills, Judge A. Brown. Mr. Wm. E. Dodge, Mr. J. A. Scrymser, and Mr. Wm. C. Schermerhorn each gave £2000, and there were eight subscribers of £1000 each. The act of incorporation required that this amount be collected for an endowment. The city must now raise £100,000 by bonds for building purposes, and provide 250 acres of land in Bronx Park. This part of the agreement will probably soon be carried out, so New York may look to possessing shortly a botanic garden of the first order. Writing with reference to the prospect in *Science* of July 5, Prof. G. L. Goodall, of Harvard University, remarks: "To Columbia College and the other educational institutions of New York and vicinity, this new appliance for instruction will mean indeed a great deal. To all the citizens who are to take advantage of the opportunities for instruction which the garden will afford, Bronx Park will be a constant delight. But far beyond these limits, wide as they are, the garden will exert a profound and beneficial influence. Other cities will surely be stimulated by this noble movement and enrich their park systems with an educational aid of the greatest value. Formerly botanic gardens, attached even in a remote manner to educational institutions, were largely used for the cultivation of medicinal plants, and for the reception of species from distant lands. Of course, this use, although its importance is now relatively less than ever before, will still long continue to be a factor in the direction of activities. But here and there new phases of plant

relations are being displayed in the greater gardens, and with the most gratifying results. Geographical questions are asked and answered by skilful grouping of species, and in the most attractive way. The bearing of climate on the structure, habit, and possibilities of plants is made prominent in an interesting fashion. The capabilities of useful plants and the extension of their range of usefulness comprise another phase of illustration which always sets visitors to thinking. Beyond and, we may say, above these questions, which are pretty strictly utilitarian, there comes nowadays another class of illustrations which are of the highest educational value in a community, namely, the biological features which are invested with such important relations to all departments of intellectual activity."

We regret to announce the death of Prof. F. Tietjen, for many years past Director of the Recheninstitut of the Berlin Observatory, and editor of the *Berliner Astronomisches Jahrbuch*; also of Prof. G. F. W. Spörer, of the Potsdam Observatory, well known amongst astronomers for his solar observations.

A statue to Boussingault was unveiled at the Paris Conservatoire des Arts et Métiers last week. The French Minister of Agriculture, who presided at the inauguration, pointed out how very largely Boussingault's work had benefited agriculture. The funds for the erection of the monument were raised by public subscription, through a Committee of which M. Schloesing was the president.

A few days ago, the Municipal Council of Paris, and the General Council of the Seine, presented Dr. E. Roux, who has devoted so much attention to the anti-toxic serum treatment of diphtheria, with two gold medals struck in his honour. M. Pasteur was unable to be present on account of ill-health, but he sent a letter in which he expressed his great gratification at the way in which the municipality were publicly expressing their appreciation of the work of his pupil and collaborator.

Sir William H. Flower, K.C.B., has been elected a Correspondant of the Paris Academy of Sciences; and Prof. Cohn has been elected to succeed the late Marquis de Saporta, as Correspondant in the Section of Botany.

The death is announced of Dr. Hermann Knoblauch, President of the Kaiserliche Leopoldinisch-Carolinische Akademie of Halle. He died in the seventy-sixth year of his age on June 30th.

Dr. Fabian Franklin, Professor of Mathematics in the Johns Hopkins University, has resigned his position in order to take up editorial work on the *Baltimore News*.

M. Paul Sintenis has returned from Turkish Armenia with large collections of rare plants.

Sir Edward Lawson will distribute the prizes to the students of the Charing Cross Hospital Medical School this afternoon, at 4 o'clock. Next Thursday evening, the distribution of prizes to the students of the Dental Hospital of London will be made by Sir William MacCormac, at a conversazione to be held in the Royal Institute Galleries, Princes Hall, Piccadilly.

The University of Chicago has decided to add Terrestrial Physics to the subjects taught in the Physical Department under Prof. Michelson, says the *American Meteorological Journal*. Dr. L. A. Bauer has just commenced courses in terrestrial magnetism, thermodynamics of the atmosphere, and dynamic meteorology. This step marks a new era in the development of the study of meteorology in the United States.

Prof. F. Omori, of the Seismological Institute of Tokio, contributes an interesting paper on the velocity of earthquake-waves to the *Bollettino* of the new Italian Seismological Society (vol. i., 1895, pp. 52–60). The chief value of his investigation lies in the fact that the distances traversed are generally short and the times exceedingly accurate, so that we thus obtain some idea of the surface-velocity in the neighbourhood of the epicentre. The mean velocity for twenty-five earthquakes (1891–94) is found to be 2·04 km. per second. Prof. Omori also shows that for earthquakes originating in the same region, the velocity is practically constant, whatever be the intensity of the initial disturbance or the distance of the place of observation from the centre.

The prizes and medals of the Paris Société d'Encouragement have just been awarded. The prize of twelve thousand francs (£480), awarded every six years to the author of the most useful discovery to French industry, has been given to Prof. Lippmann, for his method of photographing colours. Among the other awards we notice the following: Prize of 2000 francs to M. F. Osmond for his works on the microscopic analysis of steel, of which an account is given in the May *Bulletin* of the Society; 500 francs to M. Garçon for his work on "La Pratique du teinturier"; 1000 francs to M. Ch. Tellier, 500 to M. Lacroix, 500 to M. Maignen, and 500 to M. Schlumberger, for the purification of potable waters; 500 francs each to M. Lartigue and M. Roux for their investigations in connection with the electrical installations; 1000 francs to M. Guerrier, 500 francs to M. Allard, and 500 francs to M. Martin for their agricultural studies. The grand gold medal, awarded every six years for works which have exercised the greatest influence upon the progress of French industry during the preceding six years, has been given to the Comité de l'Afrique française for their great services to African colonisation.

The current number of the *Annales de l'Institut Pasteur* contains an official account of the antirabic inoculations carried out at the Pasteur Institute in Paris during the past year. From this it appears that 1387 persons were treated, out of which seven died subsequently. On comparing the statistics for last year with those compiled for 1893, we find that although the total number of admissions fell short last year by 261 of the figure reached in the previous year, yet England's contribution in the shape of patients rose from 23 in 1893 to as many as 128 in 1894. Thus, in spite of the broadcast circulation of a vast amount of sentimental opposition to the carrying out of Pasteur's antirabic treatment in this country, we appear to be developing an increasing desire to avail ourselves of the benefits to be derived from its use across the Channel! In all, 226 foreigners were treated in the Institute last year; Spain and Greece each sending 26; Belgium, 16; Turkey, 7; Russia and Egypt, 1 each; and Holland, 2; whilst under the heading "Indes Anglaises" we find 19 as compared with 14 last year.

In connection with the questions lately raised as to the relation of spectra to molecular structure, it is interesting to recall a paper by Prof. Eder and and Mr. Valenta, communicated to the Vienna Academy a year ago. Mr. J. S. Ames summarises the paper in the May *Astrophysical Journal* as follows:—"The paper deals with the different spectra of mercury. Observations on the arc and spark-spectra and on the ordinary Geissler tube discharge showed that all three were alike, the most prominent lines in one spectrum being also the most prominent in the others. But two entirely new spectra were discovered. If mercury vapour is distilling at a low pressure through a capillary tube, and if a spark be passed through it, spectra are observed which are quite distinct from the ordinary one. If there is a large number of Leyden jars in circuit, the spectrum consists of an *immense* number of fine, sharp lines; but if there are no jars in circuit, the spectrum is entirely changed; it becomes a series of bands

whose edges are towards the red. One spectrum is just as complete as the other, neither one being a development of the other. The band spectrum corresponds to a trifle lower temperature than the new line spectrum; but it is difficult to see how complexity of molecular structure can account for the difference between the two spectra in the case of mercury, whose vapour is monatomic. This has, of course, a most important bearing on the theory of band and line spectra, and seems to decide definitely against some of the present ideas concerning them."

THE current number of *Wiedemann's Annalen* contains a paper by Herr J. E. Myers on the influence of gases dissolved in the electrolyte of a silver voltameter on the weight of deposited silver. The author finds, as has previously been shown by Schuster and Crossley, that if the same current is sent through two voltameters containing neutral solutions of silver nitrate of the same strength and at the same temperature, one voltameter being in a vacuum and the other in air, then the weight of the silver deposited in the vacuum voltameter is, for a solution containing from 20 to 40 per cent. of silver nitrate, about 0·1 per cent. greater than that of the silver deposited in the other voltameter. For a 5 per cent. solution, the difference is somewhat smaller. If the solution is saturated with carbon dioxide the deposit is about 0·055 per cent. lighter than when the solution is saturated with air. With nitrogen, however, the deposit is about 0·05 per cent. heavier than with air. The electrical resistance of a 5 per cent. solution saturated with air is practically the same as that of the same solution in a vacuum. With a current of more than 0·25 ampere, it is found that in vacuum an evolution of gas takes place at the anode. The author has also examined the grey deposit which is formed on the anode, and finds that it consists of pure silver oxide.

THE results of some observations on declination made by M. Ch. Lagrange, which, if unaffected by some unsuspected error, are most unexpected, are given in a recent number of the *Comptes rendus* (June 17, 1895). During the last three years the author has been making observations of declination at the Uccle Observatory at Brussels, using for this purpose magnets having very different magnetic moments. He finds that systematic differences occur in the values obtained, but what is most astonishing is that diminution, within certain limits, of the magnetic moment of the magnet causes an amplification of the observed changes in declination. In one set of observations, lasting for six months, one of the magnets consisted of the almost astatic magnetic system taken from a galvanometer. By comparing the readings obtained with this system of magnets with those obtained on the self-registering magnetometers, it was found that the amplitude of the movements of the galvanometer needle was from fifteen to twenty-five times as great as that of the magnetometer needle. Another set of observations have been made with a large steel magnet, only feebly magnetised, however, so that its magnetic moment was only about [illegible] of that of the magnet of the magnetograph. This magnet was suspended by a fine platinum wire, and here again the amplitude of movement of the feebly magnetised bar was [illegible] than that of the more strongly magnetised one.

WE have received *Bulletins* Nos. 119-124 of the Michigan Agricultural Experiment Station, dealing with a variety of subjects of [illegible] interest. With regard to the troublesome [illegible] rot, which is often the cause of serious loss, it is stated that spraying with Bordeaux mixture is efficacious. [illegible] when the tomatoes had grown to the size of [illegible] the plants were given a thorough spraying, and [illegible] after the application was repeated. Very little rot [illegible] on the sprayed plants, whilst on those which were purposely left unsprayed many diseased fruits were to be seen. In the summary of results of experiments with potatoes, it is said that potatoes deteriorate rapidly under ordinary cultivation, and it is necessary to frequently change seed in order to keep them in their pristine purity and excellence. We need go no farther than Ireland, with its worn-out variety of the Champion potato, for a case in point. As a treatment for apple-scab (*Fusicladium dendriticum*, Fckl.) it is recommended to thoroughly spray the trees, before growth begins in spring, with copper sulphate solution. This should be followed with an application of Bordeaux mixture as soon as the blossoms have fallen. In a wet season two or three more dressings will be necessary to produce the best results. The addition of Paris green to the second and third applications will keep the codlin-moth and the canker-worm in check. A caution is given never to spray with arsenites when fruit-trees are in bloom, or the bees will be killed.

AN attempt at a partial restoration of the geography of the world in Cretaceous times is made by Dr. F. Kossmat, of Vienna, in the May number of the *Records* of the Indian Geological Survey. He recognises the broad distinction of Atlantic and Pacific faunal provinces in Cretaceous times, a distinction very marked in the northern hemisphere, but disappearing to the south of the then existing Indo-African continent. The Cretaceous beds of Southern India form the clearest link between the two; combining in their fauna the typical Pacific forms with others characteristic of Central Europe. Their connection with the latter area was a roundabout one, through Natal, Angola, and the Atlantic, by which they are also linked to the Cenomanian and Danian deposits of Brazil. The fauna of Northern India is quite distinct, and must be regarded as inhabiting the easterly termination of the Mediterranean province, one which was an almost isolated area, though to the westward, in the Gosau beds and those of Southern France, we can see evidence of a connection with the Atlantic. Further west a similar fauna is found in the Antilles, and extends even into the Pacific region in Peru. The fauna of North America shows close affinities with that of Europe, and less marked relations to that of Southern India, while it stands sharply contrasted with that of the Pacific side of the continent. The upper Cretaceous beds of Atlantic facies are found, however, to extend into British Columbia and Queen Charlotte's Islands, and there rest upon lower Cretaceous beds of Pacific facies. The American continent must thus have existed as two great insular masses forming a barrier between the two great marine provinces, broken across by two arms of the sea. The author purposes constructing a chart to embody these conclusions.

THAT quite a considerable number of bacteria exist which will only grow at such high temperatures as lie between 50° and 70° C., was first shown by Globig; but his investigations only succeeded in demonstrating them in the superficial layers of soil. Now, however, we know that such bacteria are to be found in river water and mud, in fæces, and at considerable depths in the soil. Quite recently Dr. Lydia Rabinowitsch has made extensive researches in Dr. R. Koch's laboratory on these so-called thermophilic bacteria, and their distribution appears to be much wider than was at first supposed. Thus Dr. Rabinowitsch has found them abundantly present in surface soil collected from various parts of Berlin and other places in Germany; they were also discovered in freshly-fallen snow, indicating their probable presence in the air, and large numbers were obtained from river Spree water, although they were not found in the Berlin water supply; they were also isolated from excrementitious matter derived from horses, cows, goats, dogs, rabbits, ducks, parrots, some fish and other cold-blooded animals, such as the frog and python. These bacteria are also

present in large numbers in the mouth and all along the intestinal tract of man. Cow's milk contains them, and they are not destroyed even when the latter is vigorously boiled. The most favourable temperature for the growth of these thermophilic bacilli lies between 60° and 70° C., but they may be induced to grow also between 34° and 44° C. It would be interesting to learn what part is played by these bacteria in nature, and it is to be hoped that Dr. Rabinowitsch will continue these investigations, and instruct us as to these functions of thermophilic bacteria.

DR. J. HANN has sent us a copy of his paper on the conditions of atmospheric electricity on the summit of the Sonnblick mountain, deduced from the records of an improved registering hair hygrometer by Richard, which had been adjusted and tested at the Central Meteorological Office in Vienna. The discussion is one of much importance, and the subject is treated by Dr. Hann in a very thorough manner; but the space at our disposal will only allow us to notice briefly some of the general results. The yearly range of relative humidity on the mountain is the reverse of what it is over the plains; the minimum, or greatest dryness, occurs in winter, and the maximum in spring and summer. This much was known from observations at Alpine stations, but at these the uncertainty of the behaviour of the hygrometers in low temperatures made the results doubtful. Temperature and vapour pressure on the Sonnblick run in nearly parallel curves, each degree of difference of temperature corresponds to a change of tension of vapour in the same direction. With regard to the daily range, it is found that in all, except the three winter months, there is low relative humidity in the morning and a great humidity during the evening and night. In winter, however, the case is very different; from about 6h. p.m. to 7h. a.m. the relative humidity remains below the mean, and from 9h. a.m. to 5h. p.m. it is above the mean. The daily range of absolute humidity (vapour tension) is nearly the same in all seasons of the year; the minimum occurs early in the morning, and the maximum in the afternoon. The most remarkable feature in the daily range of relative humidity is that on very clear and warm days, long before the rise of the sun has any effect, the humidity falls below the mean value on the Sonnblick, and by about 6h. in the morning, it has fallen nearly 7 per cent. below the daily mean. This important fact seems to show that the relative dryness of the forenoon on mountains is due to a descending movement of the atmosphere, caused by the winds blowing from the mountains to the valleys during night-time, and thus cooling the sides of the mountains.

THE July *Journal* of the Chemical Society contains the paper on "Helium, a Constituent of certain Minerals," by Prof. W. Ramsay, Dr. J. Norman Collie, and Mr. M. Travers, read before the Society at the last meeting. There are also fifteen other papers read before the Society, and 138 pages of abstracts of chemical papers published in other journals.

WITH the current number, the *Medical Magazine* enters upon its fourth year of issue. The magazine is always readable, not only by members of the medical profession, but by the laity, and the papers which it publishes on medical history and literature are invariably of general, as well as technical, interest. We notice among the articles in the number before us, one on "Mountain Sickness," by Dr. H. Kronecker; and another on "Immunity," by Dr. J. G. Sinclair Coghill.

UNDER the title *Beiträge zur wissenschaftlichen Botanik* a new contribution to general botanical literature is announced, to be edited by Dr. M. Fünfstück, and published by Nägele, of Stuttgart. The first number, which is already published, contains papers on the physiology of woody plants, by Lutz; on the action of "Bordeaux-brühe" and its constituents on *Spirogyra longata* and on the uredespores of *Puccinia coronata*; and on the oily excretions of calcareous lichens, by the editor.

THE report for 1894 of the American Museum of Natural History shows that a number of valuable specimens were added to the collections last year. The new wing, for the building and equipment of which 550,000 dollars (£110.000) were voted in 1893 and 1894, is approaching completion, and is expected to be opened to the public in the autumn. Since the preparation of the report, the Legislature has given power to the authorities of New York City to appropriate £100,000 for a further enlargement of the museum, and for an increased grant of £4000 annually, for maintenance. The erection and equipment of another wing to the museum will provide the facilities for carrying out the plans of the Trustees for the establishment of a great department of Anthropology.

THE report of the Trustees of the South African Museum, for the year 1894, has been received. As the staff of the museum does not include collectors, it is gratifying to learn that nearly seven thousand specimens were presented by private collectors during last year. That the museum is appreciated is evidenced by the fact that the number of visitors in 1894 was nearly twenty-six thousand. The Curator, Mr. R. Trimen, has completed the manuscript of descriptions of new Lepidoptera from Mashonaland, which will be published at the beginning of the year. He has also begun the incorporation of the tropical African insects of this order in the South African collection, adopting the 16° of latitude S. as the South African limit. The staff has been increased by the appointment of Dr. G. S. Corstorphine as assistant in the department of geology and mineralogy. A report by him, on the existing collection of that department as at present exhibited, is appended to the report of the Trustees.

THE additions to the Zoological Society's Gardens during the past week include a Campbelli Monkey (*Cercopithecus campbelli*) from West Africa, presented by Miss C. Thompson; a Yellow-billed Sheathbill (*Chionis alba*), captured at sea, presented by Captain Plunket; four Common Chameleons (*Chamæleon vulgaris*) from Egypt, presented by Mr. J. C. Mitchell; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Columbia, presented by Mr. James G. Green; a Royal Python (*Python regius*) from West Africa, presented by Colonel Frederick Cardew; an Alexandra Parrakeet (*Polytelis alexandræ*) from Australia, six Grey Francolins (*Francolinus ponticerianus*) from Mombassa, a Black Tortoise (*Testudo carbonaria*) from South America, deposited; five Fennec Foxes (*Canis cerdo*), two Variegated Jackals (*Canis variegatus*), two Libyan Zorillas (*Ictonyx lybica*), two Egyptian Cats (*Felis chaus*), three Dorcas Gazelles (*Gazella Dorcas*), four White Pelicans (*Pelecanus onocrotalus*), a Grey Monitor (*Varanus griseus*), from Cairo, received in exchange; a Wapiti Deer (*Cervus canadensis*), two Short-headed Phalangers (*Belideus breviceps*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE NEW MADRAS OBSERVATORY.—Prof. Michie Smith, the successor of Mr. Pogson at Madras, has lately made known a few particulars relating to the new Solar Physics Observatory which is to be erected in India. The funds have been voted by the Indian Government, and the site selected is in the Palani Hills at Kodaikanal, 300 miles south of Madras. The daily work of photographing the sun, which is now carried on for the Solar Physics Committee at Dehra Dûn by the officers of the Indian Trigonometrical Survey, will form part of the routine work of the new observatory. It is also proposed to undertake a systematic spectroscopic examination of the sun, but the details of this portion of the programme have not yet been finally determined upon. The climate of Kodaikanal seems to be almost all that can be desired for astronomical purposes. The mean daily temperature varies from 54°·1 C. in December to 62°·2 C. in May, while the rainfall is about 47½ inches. From March to December in the year in which observations were

specially made, the bright sunshine amounted to 1634 hours; the morning is usually bright until about eleven o'clock, then clouds come up and continue until about four o'clock; by six o'clock the sky is generally cloudless. Except during the north-east monsoon, a night which is wholly cloudy is almost unknown. Under these highly advantageous conditions, there is every prospect that the establishment of this observatory will result in a great gain to astronomy, especially in the department of solar physics.

Star Catalogues.—An admirable *résumé* of the history of star cataloguing, from the pen of Mdlle. Klumpke, the gifted directress of the *Bureau des Mésures* of the Paris Observatory, appears in the current number of the *Bulletin* of the Astronomical Society of France. From an instrumental point of view three great epochs may be recognised, each marked by some important discovery. The first epoch is that in which the line of vision is defined by hollow cylinders or by an alidade, and extends from the time of Hipparchus to that of Hevelius; it comprises the catalogues of Hipparchus, Ptolemy, Ulugh-Beigh, and Tycho Brahe. The catalogue of Hevelius, though drawn up from observations with the naked eye, marks a transition period, as he took advantage of the application of the pendulum to the regulation of clocks.

The second epoch is marked by the application of the telescope for accurate sighting of the heavenly bodies, and the employment of the sidereal clock. This period commenced with Flamsteed, and extends even to the present time. In the third epoch the photographic plate replaces the eye. Enthusiasm for this method of cataloguing the stars commenced with the fine results obtained by the Henrys, but it should not be forgotten that as far back as 1865, Rutherford obtained photographs of stars down to the ninth magnitude, and that he clearly foresaw the advantages to be derived from the photographic method. All the world knows now that a great photographic chart of the heavens, initiated by the late Admiral Mouchez, is in course of construction, eighteen observatories participating in the gigantic undertaking. Mdlle. Klumpke estimates that this international catalogue will contain upwards of three millions of stars.

The photographic method, however, does not yet appear to be without imperfections, as the impressions on the negatives are not certainly permanent. In a communication to the editor of the *Observatory*, Dr. Isaac Roberts gives some figures relating to the disappearance of the smaller images in the course of years; in one negative no less than 130 out of 364 star images had disappeared in nine and a quarter years. Hence it is important that as short a time as possible should elapse between the taking of a photograph and its reduction, or, better still, its manifolding by some carbon process.

THE PLACE OF ARGON AMONG THE ELEMENTS.

The position of argon in a classification of the elements depending on atomic weights has been recently defined by C. J. Reed (*Journal of the Franklin Institute*, July). The elements are assigned positions on a plane determined by abscissæ proportional to their atomic weights and ordinates proportional to their valency. Oxygen is assumed to have an electronegative valency 2, and the valency of other elements is referred to this as standard; electro-positive valency is measured upwards, electro-negative downwards from the zero-axis. Under these conditions most of the elements fall on a peculiar series of double, equi-distant, parallel straight lines, connecting elements in order of their atomic weights and separated alternately by distances corresponding to one and sixteen units of atomic weight respectively.

If the plane be now folded into a cylinder with axis parallel to the abscissæ and a circumference of eight units of valency, it is found that the upper and lower parts of the connecting lines coincide; the whole of these lines then form a parallel pair of spirals on the surface of the cylinder, and valency in angular measure becomes directly proportional to atomic weight.

The regularity with which the elements of lower atomic weight fall alternately on each of the parallel spirals is very striking, but this regularity is not maintained among elements of high atomic weight, notable deviation occurring with most of the elements of which the atomic weight ranges from 100 to 130. The axis of atomic weights represents the valency + 0 or + 8 and is cut by the double spiral in fifteen points. There should then be a group of fifteen elements having a valency of zero or eight, and their atomic weights should be, respectively, 4, 20, 36, 52, 68, 84, 100, 116, 132, 148, 164, 180, 196, 212, and 228. All the known elements appear to be grouped together on certain regions of the surface of the cylinder, other parts remaining comparatively bare. The only members of this family to be expected to occur in terrestrial matter will be those in the inhabited regions of the cylinder surface. The hypothetical elements having atomic weights 20, 36, 84, and 132 are the most necessary from this point of view.

It seems reasonable to suppose from the peculiar position of these elements on the border-line between electro-negative and electro-positive valencies, that they should be more strongly electro-negative than the corresponding members of the sulphur group, and should nevertheless be without valency (or octads). They should, in general, be more volatile than the corresponding members of the sulphur group. As electro-negative valency diminishes in any group with increase of atomic weight, the element 196, if it exists, cannot be expected to be electro-negative. This element should be a volatile metal, heavier and scarcer than gold, and capable of easier reduction to the metallic state; it should be capable of forming an oxide RO_4 or a salt K_2RO_5. The volatile metal osmium agrees with the requirements of this element very closely. Similarly, ruthenium may possibly be the element 100.

Finally, argon falls naturally into the place of element 20, and possesses, so far as is known, the properties to be expected of this element in position 20 in the new group. Argon and element 36 should be comparatively abundant in nature, while 84 and 132 should be scarce, but not more rare than selenium and tellurium.

On Mr. Reed's system, argon should be element 36 if it be monatomic as now believed, and not 20 as he assumes; the actual atomic weight found, 39·9, would then indicate the possibility of the presence in argon of some small quantity of element 84 or element 132. It is remarkable also that, if helium has the atomic weight 4, it falls naturally in this group, and that its atomic weight deduced from the observed density is somewhat greater than this number. If this difference should be due to the presence of some small quantity of element 84, then the spectroscopic evidence leading to the conclusion that argon and helium contain a common constituent would be explained.

POCKET GOPHERS OF THE UNITED STATES.

In *Bulletin* No. 5 of the U.S. Department of Agriculture, Mr. Vernon Bailey gives an account of the habits and life-history of the Pocket Gophers of the United States, which contains a number of interesting facts and observations derived from various sources. These curious little rodents live underground in burrows which they tunnel in the soil. When working their way through the earth, they use the upper incisors as a pick to loosen the ground, while the fore-feet are armed with strong curved claws for digging. When a sufficient quantity of soil has accumulated behind an animal, he turns in the burrow and pushes it out in front until an opening in the tunnel is reached; the earth is here discharged, and forms a hillock similar to the hills thrown up by moles. Gopher burrows are extended and added to year by year, and the course is marked by the hills of soil brought up to the surface. Gophers do not hibernate, as has been commonly supposed, but work steadily throughout the winter. They do a great deal of good in mixing the soil, and in this way are probably most useful on poor or uncultivated ground. But, on the other hand, in agricultural districts the animals are highly injurious: they devour potatoes and other tubers and roots in large quantities, as well as corn, wheat, and other farm crops; and they destroy great numbers of fruit trees by gnawing off the roots. Gopher burrows also often do a great deal of damage in meadows or on the banks of artificial water-courses. So great is the harm done by Gophers, that in many districts bounties have been offered for their capture. One of the most striking features of Pocket Gophers is their possession of cheek pouches opening outside the mouth. It is commonly supposed that these pouches are used for carrying earth out of the burrows; but Mr. Bailey's investigations lead him unhesitatingly to the conclusion that this view is erroneous; they are used only for carrying food pieces of

potato and roots, leaves, &c.—to be eaten at ease in the seclusion of the animals' burrows, or to be stored up for use in the winter. The food is passed into the pouches by the fore-feet; and the animals empty their pockets by pressing the sides of the head with the fore-feet from behind forwards, so that the contents fall out in front of them. In disposition Gophers are very fierce; and on the rare occasions on which they wander from their holes, frequently attack passers-by without any provocation. They are not very prolific animals, as is commonly stated, for only one litter of two or three young is produced in a year; but, although their rate of increase is slow, their mode of life protects them from many enemies which attack squirrels, mice, and many other rodents. The Pocket Gophers of the United States belong to three genera, *Geomys*, *Cratogeomys*, and *Thomomys*: Mr. Bailey gives two charts illustrating the distribution of these different genera and their constituent species.

COLOUR PHOTOGRAPHY.

An important paper on the theory of colour photography is contributed to No. 6 of *Wiedemann's Annalen*, by Herr Otto Wiener. The paper deals with the methods of attacking this problem which are based, not upon the photography of the different constituents of coloured light and their subsequent recognition—like Mr. Ives's heliochromy and similar processes—but upon the direct production of colour by the influence of light upon certain chemical substances. The most recent, and in a way the most successful of these methods is that due to Lippmann, and the question raised by Herr Wiener is whether the old processes invented by Becquerel, Seebeck, and Poitevin are based upon interference colours like Lippmann's, or upon "body colours," *i.e.* colours produced by partial absorption of the incident light. That Lippmann's colours are due to interference may be very simply proved by breathing upon a plate with a photograph of the spectrum, when the colours quickly wander towards the violet end, this result being due to an increase in the distance between the nodal layers. This experiment cannot be applied to a spectrum photographed by Becquerel's method. But Herr Wiener succeeded, by a simple and ingenious contrivance, altering the path of the rays through the coloured film by placing a rectangular prism on the plate, with its hypothenuse surface in contact with the spectrum. This experiment had the startling result that that part of the spectrum covered by the prism appeared strongly displaced towards the red. Hence Zenker's theory of Becquerel's process, enunciated in 1868, which ascribed the colours to interference, is substantiated. Instead of Becquerel's homogeneous sheet of silver chloride containing subchloride, Seebeck used the powder, and Poitevin mounted the salt on paper. In these two processes the effect described is not observed. Hence these colours are body colours in these two cases. The production of these body colours is a very mysterious process, but the author hopes that here will eventually be found a satisfactory solution of the problem. To account for the production of these colours he advances a remarkable theory which has a well-known analogy in comparative physiology. Given a collection of compounds of silver chloride and subchloride of indefinite proportions, such as those which Mr. Carey Lea calls by the collective name of "photochloride," we must suppose according to the modern kinetic theories that they are undergoing a rapid series of successive modifications. When the red combination happens to be exposed to red light, it reflects it without absorption, and will therefore no longer be affected or changed by it. Similarly for the other cases. This is another process of "adaptation." The author describes some experiments which prove that this is the true explanation, and points out the importance of this view, not only for colour photography, but for the production of colours in the animal world.

THE SLATE MINES OF MERIONETHSHIRE.[1]

An official Blue Book drawn up by a Departmental Committee appointed by Mr. Asquith, and referring to the dangers of slate quarrying in Merionethshire, has recently appeared. After a brief account of the mode of occurrence, the method of getting the slate by true mining operations is described, and the principal causes of accidents are enumerated and explained. Judging by the statistics of the last nineteen years, the underground worker in Merionethshire is exposed to greater risks than the average collier; some 40 per cent. of the deaths are caused by falls of rock, a fact which causes no surprise when one considers the conditions under which the slate-getters carry on their daily work in huge chambers, the roofs and sides of which cannot be examined without rigging up lofty ladders.

An interesting table of death-rates shows that the Merionethshire slate quarrymen are better off as regards the safety of their occupation than many other classes of workmen, such as navvies, railway servants, and sailors.

The medical evidence, especially that of Dr. Richard Jones, is very complete, and we learn that some of the ills of the Merionethshire quarrymen are practically of their own making. Judging by the report and the evidence upon which it is based, the men are not cleanly in their ways, and if their sober habits lead them to ruin their digestions by stewed tea, it becomes a question whether their so-called, but incomplete, temperance is an unmixed benefit.

For preventing accidents, the Committee make several useful suggestions; one of the most important is their advocacy of "channelling machines" or "groove cutters," for assisting in getting the slate, instead of violently wrenching off the blocks by blasting.

The value of the report is enhanced by some useful appendices, a copious index of the evidence, and several woodcuts and plates. The plates are noteworthy as being the first instances of reproductions of photographs in a Blue Book by the half-tone process. Five of the eight photographs were taken underground by magnesium light; the two best, which represent ladders set up in underground chambers, are the work of Mr. Burrow, of Camborne, already well known by his successful pictures of Cornish mines.

The report is signed by Mr. Le Neve Foster, the Inspector of Mines of the district, Mr. J. E. Greaves, the owner of one of the largest slate mines, Mr. E. P. Jones and Mr. J. J. Evans, both quarry managers of wide experience, and Mr. J. Jenkins, President of the Quarrymen's Union. The opinions of a practical Committee of this kind are entitled to consideration, and it will be interesting to note how far their suggestions are carried out, and how far they attain their object, viz. the increased safety and general well-being of the Merionethshire quarrymen.

[1] Report of the Departmental Committee upon Merionethshire Slate Mines, with Appendices. Presented to both Houses of Parliament by command of Her Majesty, 1895.

THE RELATION OF BIOLOGY TO GEOLOGICAL INVESTIGATION.[1]

II.

THE RELATIVE CHRONOLOGICAL VALUE OF FOSSIL REMAINS.

Rejecting the idea of special endowment held by early geologists, we must consider the relative chronological value of fossil remains with reference to the natural laws which have produced their characteristics and governed the various conditions of their origin. Much may profitably be said concerning the comparative chronological value of the different genera, families, &c., belonging to one and the same class of any branch of either the animal or vegetable kingdom, or to different classes; but I propose to discuss only the broader relations to one another of the more general kinds of fossil remains. These discussions will relate to the time-range of each of those general kinds, the various conditions under which they have been preserved, the various conditions of habitat of the animals and plants which they represent, the relative rate of evolutional development of the different kinds and their differences of reciprocal relation to one another.

No fact in historical geology is more conspicuous than that of the great differences in time range of the various kinds of organic forms, some of them having ranged through the whole of the time represented by the geological scale, while others, and among them some of the biologically most important kinds, ranged through only a comparatively small part of it.

A special grouping of the different kinds of fossil remains is more appropriate for these discussions than is a strictly systematic one, and I have therefore adopted the following: (*a*) marine invertebrates, (*b*) non-marine and land invertebrates, (*c*) fishes, (*d*) batrachians and reptiles, (*e*) birds, (*f*) mammals, and

[1] By Charles A. White. Abstract of a series of eight essays published in the Report of the United States National Museum. (Continued from p. 261.)

(G) land plants. For convenience of reference, our present knowledge of the time-range of these kinds may be presented in tabular form. The accompanying illustration, representing the whole of geological time by its height, indicates in a general way by perpendicular lines the time-range of the kinds just mentioned, and remarks in following paragraphs further explain the known range of some of the subordinate, as well as that of the principal kinds.

The horizontal spaces of the table represent the systems or stages of the geological scale. The proportionate width of the spaces which contain the names of those systems or stages is not intended to indicate the actual ratio of geological time for each, but it may be stated as the general opinion of competent investigators that the portion of the scale from the Cambrian to the Carboniferous inclusive represents a much greater length of time than does the portion from the Trias to the Tertiary inclusive. In other words, it is generally believed that the Palæozoic portion of the geological scale was of much longer duration than was that of the Mesozoic and Cenozoic portions together.

The perpendicular lines in the table, which are placed singly or in pairs or groups under letters of the alphabet from A to G

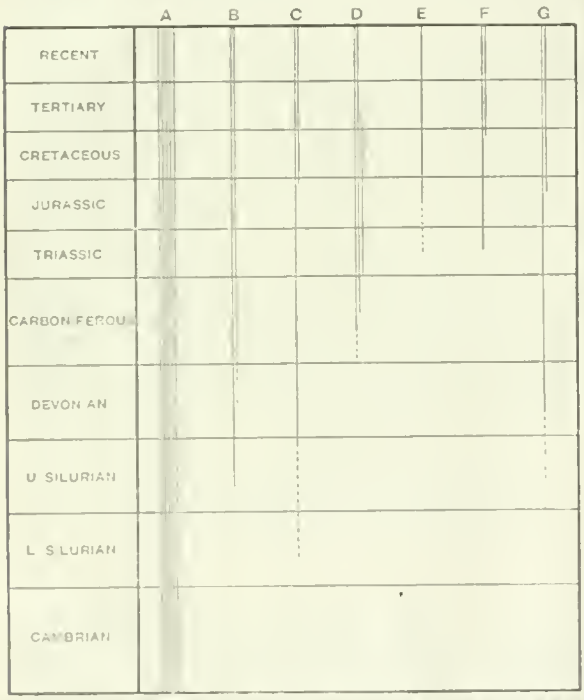


TABLE showing [illegible] (A) Marine invertebrates, (B) non-marine and land invertebrates, (C) fishes, (D) batrachians and reptiles, (E) birds, (F) mammals, and (G) land plants.

indicate, approximately, the time-range of the kinds of animals and plants which have already been mentioned, and which for convenience of reference are again recorded with their corresponding letters at the foot of the table. This method of grouping the different kinds of animals and plants, as already referred to, is adopted only for present convenience in making comparisons of chronological values. All the principal kinds of animals recognised in the usual systematic classification are, however, included in these special groups, the few that are omitted being regarded as of little or no importance in this connection. The dotted portion of certain of the lines indicates approximately the real extent of the time-range which is shown by them, because of imperfect or doubtful representation of those kinds by discovered fossil remains.

Of all the animals which have existed upon the earth, and of which remains have been discovered, only those of marine invertebrates have been found to range through the whole geological scale. The occurrence of these important portions of the animal kingdom is represented by the group of five perpendicular lines under the letter A. The marine invertebrate life thus represented includes the Protozoa, Cœlenterata, Annuloida, Annulosa, and Mollusca, the latter including the Molluscoida. That is, it includes five of the six sub-kingdoms or branches of the animal kingdom.

The non-marine and land invertebrates, the time-range of which is intended to be represented in the table by the two perpendicular lines under the letter B, are only insects and fresh-water, brackish-water, and land molluscs. The discovered fossil remains of all other non-marine and land invertebrates are regarded as either too rare or too unimportant to be profitably considered in the comparisons which are to follow. The longer of the two lines may be taken as representing the known time-range of insects, and the shorter that of land and non-marine mollusca.

The pair of perpendicular lines in the table under the letter C shows the approximate time-range of all the various kinds of animal remains which have been referred to the fishes. The shorter of the two lines indicates the known range of the teleostean fishes, and the longer that of the other kinds, the latter including certain forms that differ materially from any living fishes.

The time-range of batrachians and reptiles, so far as it is known, is shown by the three perpendicular lines in the table under the letter D, that of the dinosaurs alone being represented by the shortest line of the three.

The known time-range of birds is represented by the single line under the letter E. It is here assumed that most, if not all, the fossil tracks found in Triassic strata, and formerly referred to birds, are those of dinosaurs.

The two lines in the table under the letter F represent the known time-range of mammals, the longer line representing that of the non-placental, and the shorter that of the placental mammals.

The known time-range of land plants is represented by the two lines under the letter G. The shorter line represents the range of the dicotyledons and palms, and the longer one that of all other kinds. The algæ and diatoms are omitted from the table, as being of little or no importance in the comparisons and discussions which are to follow.

The earlier portion of the time-range for each of the kinds of animals and plants, as shown by the perpendicular lines in the table, is naturally more incompletely and indefinitely represented by fossil remains than is the later portion, because of the smaller variety and greater rarity of those earlier remains, and also in most cases because of the increasing difference in character from living forms which is observable from later to earlier formations. In some cases, however, the early portion of the time-range as it is now known begins so suddenly, and with forms of such high biological rank, as to make it evident that its real beginning was much earlier than it has yet been proved to be by actual discovery of fossil remains. The last-mentioned fact is of great importance in many respects, but it does not necessarily affect the question under consideration, because all estimates of the relative chronological value of fossil remains must be confined to the kinds already known, and the application of such estimates must refer only to those portions of the geological scale in the strata pertaining to which the remains are known to occur.

It has been shown that it is the general advancement in biological rank for all organic forms and for the whole of geological time that constitutes the ideal ultimate standard of measure for that time. It does not necessarily follow, however, that the geological scale is actually based upon the combined average rate of advancement of all those forms, because this is a factor which cannot be definitely ascertained. Still, in all cases it is necessary to apply that idea so far as is practicable.

In view of the facts recorded in the preceding paragraphs, the highest estimate of chronological value must necessarily be placed upon the fossil remains of those kinds which have existed under the most nearly uniform conditions through the whole of geological time, and which give evidence of the most nearly uniform advancement in biological rank. Accordingly, the remains of marine invertebrates possess legitimate claims to a higher estimate of chronological value than do those of any other kinds of animals or of plants.

It is true that the rate of development in biological rank of marine invertebrates does not embrace the entire advance for the whole animal kingdom, because it begins in the scale as it is now known with many highly organised forms, and ends without including the vertebrates; but this fact does not affect any of the

necessary elements of their superior chronological value, which have just been mentioned. The following summary of facts relating to the marine invertebrates show their principal claims to the highest estimate of value in characterising the divisions of the geological scale, and in determining the geological age of the strata in which their remains are found.

The marine invertebrates embrace five of the six sub-kingdoms or branches of the animal kingdom.

They have coexisted in every stage of geological time, while the known time-range of other animals, as well as of land plants, has been very much less.

The preservation of their remains having been a natural consequence of the character of their habitat, they are faunally more complete than are those of any land animals, and for the same reason they are florally more complete than are remains of land plants.

They all lived under the same or closely similar conditions, and those conditions were more nearly uniform throughout all geological time than were those under which any other forms of life existed. Their remains have, therefore, produced a more nearly uniform chronological record.

Their relations to one another were wholly congruous, while the relations of all of them to all non marine faunas and land floras was more or less incongruous, and in many cases extremely so.

The formations containing their remains are for the whole world and the whole of the geological scale far in excess of those which contain the remains of any other forms of life, especially the remains of land plants and land animals.

CORRELATIVE GEOLOGY AND ITS CRITERIA.

The term "correlative geology" is not in common use, but it is adopted as a present convenience in discussing the correlation of assemblages of strata as divisions or subdivisions of the geological scale as it is developed in separate regions, and the identification of formations within one and the same district or region. As here used, the term correlation refers to geological systems or other comprehensive series of stratified rocks which occur in different and more or less widely separated parts of the world, between which parts there is no physical continuity of strata, or none that it is possible to discover. Correlation applies to general geology, identification to local or regional investigations.

The latter may be discussed under two heads, direct and relative. Direct identification applies to formations the characteristics of which at one or more localities have been ascertained, and as these are naturally of limited geographical extent, the application is similarly restricted.

Although fossils in all cases constitute not only much the most, but usually the only, trustworthy criteria for such indentification of formations as is indispensable in the study of structural geology, the various kinds differ materially as to their relative value. This value, however, has no necessary relation to that which they may possess as indicators of geological time, or of the correlation of the strata containing them with those of other parts of the world. The two values are distinct, although one kind of fossil remains may often possess both.

While fossil remains unquestionably afford the most trustworthy and often the only means of either direct or indirect identification of formations, in the absence of these means the geologist often reaches conclusions in this respect by methods of reasoning that it would be difficult even for himself to formulate, and these conclusions are valuable in proportion to his acquirements and experience. Among these less clearly definable methods is that which takes cognisance of homogeny; that is, of a method in connection with which certain inherent lithological and stratigraphical characteristics, which are possessed by a formation or series of strata in one part of a given region under investigation, are accepted as evidence that it had a common origin with a formation or series presenting similar characteristics in another part of the same region. Such a conclusion necessarily implies that originally there was physical continuity of similar strata between such localities, and that it has either been destroyed or obscured.

This method of identifying formations is one of minor importance as compared with that which is based upon fossil remains, but unfortunately it has, especially within the last few years, been adopted by certain geologists in charge of important works, almost to the entire exclusion of palæontological considerations. Although it cannot be denied that in the hands of an experienced and broad-minded investigator this method of identifying formations is of great value, the fact remains that some of the most grievous mistakes that have ever thrown discredit upon geological investigation have occurred by its adoption to the exclusion of palæontological evidence.

It has been the custom of a large proportion of geologists to regard the geological scale as it has been established in Europe as the absolute standard for the whole earth. A necessary consequence of this view is their assumption that the systems which physically constitute that scale, and at least most of the divisions of those systems, may not only be recognised, but as clearly defined in all parts of the earth as they are in Europe, if in those parts contemporaneous deposits were made and still remain intact.

In view of known facts and principles, the idea held by the early geologists, as well as by some of those now living, that identity of fossil types proves synchronism or exact contemporaneity of origin of any two or more series of strata containing them, is quite untenable. The facts which have been presented also suggest that the term "homotaxy" must be used with some degree of latitude as to its application to the subdivisions of systems, because the order of sequence in the occurrence of the types which characterise them, respectively, in one part of the world is in another part sometimes partially reversed or partially interchanged. That is, the taxonomy of those divisions, as biologically indicated, is not the same for all parts of the world.

The presence in widely separated parts of the world of all the systems of the geological scale, as well as of some of their larger divisions, has been demonstrated by the labours of a multitude of geologists, so the fact of correlation is not called in question. The principal questions which are here raised concern the scope of correlation, or the limitation of the assemblages of strata, the relation of which to respective divisions of the scale is more or less obvious. These questions are of practical application in the study of the structural geology of any part of the world other than that in which the geological scale was established; but they are of such a character that they must be conventionally rather than arbitrarily determined.

For example, in discussing the questions which have arisen concerning the earlier and later limits of the systems of the geological scale in North America, the difference of opinion as to those limits have been wider and more various with regard to the later systems than to the earlier. This is because of the greater number and variety of the kinds of fossil remains to be considered in such discussions of the later systems. It is therefore evident that in reaching a conclusion as to the limitation of any of these systems, or of any of their subdivisions, it is necessary to take into consideration all available facts, physical as well as biological. It is equally evident that it is the duty of every American geologist to hold in abeyance any final decision as to the correlation of the groups of strata which he may study with the divisions of the European scale until all such facts have been duly and justly considered. In short, the idea of absoluteness in such cases is as much out of place as is the assertion or recognition of personal authority.

Although these remarks refer directly to North American geology and geologists, they are equally applicable to other parts of the world when reference is made to the scale as represented by the European rocks.

Notwithstanding the great excellence of the scale now in general use, and the fact that so little change has been made in it since it was first devised by the early geologists, the future progress of geological science will demand modifications the necessity for which will be especially urgent when the true character of correlation for all the principal parts of the earth has been ascertained. Hitherto correlation has been investigated with the single purpose of adjusting the series of formations which occur in each of the various parts of the world to the scale now in use; but although its general applicability to that purpose is not to be questioned, the ultimate result of the study of correlation will be to modify this scale and adjust it to the systematic geology of the whole earth. That is, the scheme of stratigraphic classification, which has been the main factor in adjusting the elements of systematic geology, must in turn be itself adjusted to the great system which it will have been the principal agent in producing.

CRITERIA OF PAST AQUEOUS CONDITIONS.

Among the more conspicuous facts in geology are some of those which relate to the manner of origin as well as to the original and present condition of the sedimentary formations. These subjects have already been discussed, and among those discussions are

some references to the character of the water in which each formation was deposited. Studies of the sedimentary formations, especially those made from a biological standpoint, have demonstrated that the bodies of water in which they were deposited were of the various kinds that are now known: that is, some were marine, some fresh, and some brackish.

Upon physical evidence alone, it is not practicable to satisfactorily classify the sedimentary formations of the earth in such a manner as to serve the purpose of thorough geological investigation. Therefore such data are in this, as in most other cases, chiefly valuable as being accessory to the evidence afforded by biological data.

The biological criteria which are relied upon by geologists to distinguish from one another the sedimentary formations which have been produced in marine waters, or in those of inland seas, lakes, rivers, or estuaries, relate to the characteristics of faunas which now inhabit those waters respectively, and to the differences from one another of such faunas. That is, the conclusions which geologists reach concerning the questions just indicated are based upon now-existing physical conditions, upon the known character, structure, and habits of animals with relation to those conditions, and upon the assumption that in past geological epochs animals of a given character and structure had similar habits, and lived under conditions similar to those which are congenial to their living congeners.

The various bodies of water which existed during geological time, and which constituted the habitat of aquatic animals, were of the same kinds that now exist, namely, marine and fresh, together with those of the various intervening grades of saltness. Although it is probable that the marine waters of early geological time were not so salt as those of the present oceans, it is believed that this difference in saltness has not been so great as to make any appreciable difference as to legitimate conclusions of the kind that have been indicated. It seems to be especially evident that this difference has been thus inappreciable since the close of palæozoic time, since which time the greater part of the known unmistakably non-marine formations were deposited.

If all the known now living members of a given family are confined to marine, or to fresh waters, as the case may be, it is assumed that the habitat of the extinct members of such families were similarly restricted, and that the presence of fossil remains of such animals in a given formation, is, in the absence of conflicting facts, sufficient evidence of its marine origin on the one hand, or of its fresh-water origin on the other. Again, if a given family is known to have representatives now living in marine, brackish, and fresh waters, respectively, it is assumed that it had a similar range of habitat during past geological epochs. Therefore, the discovery in a given formation of fossil remains of a single representative of a family having such a varied range of habitat is not of itself sufficient to enable one to decide whether it was of marine, brackish, or fresh-water origin, and other evidence must be sought.

The criteria of past aqueous conditions here discussed are, of course, only such as may be derived from sedimentary formations and their contents. It cannot be said that there are any fully trustworthy physical criteria because a non-marine formation rarely presents any condition of stratification, or any lithological character, which is not observable in some marine formations. Still, there are many more or less valuable indications which may be observed and to some degree relied upon in the absence of fossil remains.

For example, although considerable accumulations of calcareous strata are sometimes found among the generally arenaceous strata of fresh-water formations, they have never been found to contain any important accumulations of regularly bedded limestones. Furthermore, estuarine deposits are often still more of a detrital character than are fresh-water formations, and also they more rarely contain calcareous layers. Therefore, if one should encounter a series of regularly bedded limestones, either magnesian or fully calcareous, he will rarely, if ever, be at fault in regarding them as of marine origin even without biological evidence.

In a large proportion of the non-marine formations, the stratification is less regular than is usually the case with marine formations. Still, this is by no means a certain criterion, and in some cases non-marine formations are found to rest so conformably upon the marine and to be so conformably overlain by them as to give little indication of the great difference in the condition of their origin.

These examples serve to show how indefinite is the character of physical evidence as to the past aqueous conditions under which the various sedimentary formations have been produced, but they serve to emphasise a statement of the fact that almost entire reliance must be placed upon the evidence furnished by fossil remains.

With reference to general indications of difference between marine and non-marine formations which are furnished by their fossil remains, we observe that a conspicuous difference lies in the comparative abundance and variety of forms of life which the fossil faunas of the formations respectively represent. Marine waters have always teemed with life in a wonderful variety of forms, and their fossil remains are proportionally abundant. The variety is less in brackish waters, and least of all in lacustrine waters. It is true that ichthyic life is abundant in some fresh waters, but never so generally abundant or so various as in marine waters. It is also true that molluscan life is often locally abundant in shallow fresh waters, but, as already several times mentioned, the variety is extremely meagre. All these peculiarities are distinctly observable among the fossil faunas of the non-marine formations.

Other general indications of difference between marine and non-marine formations are furnished by remains of land plants and animals. Open-sea formations are naturally free from any vegetable remains derived from the land, although coal and other materials of vegetal origin are not unfrequently found alternating with layers containing marine fossil remains. These, however, are regarded as cases of emergence of the bottom of shallow sea waters and the subsequent subsidence of the same as plant-laden marshy land. It is a matter of fact, the reason for which has been suggested in preceding sections, that plant remains of any kind, especially such as are in a classifiable condition, have so rarely been found associated with remains of denizens of marine waters, that the discovery of fossil plants in any formation is of itself presumptive evidence of its non-marine origin.

It has already been shown that the remains of land animals have so seldom reached marine waters, or, having reached them, they were probably so generally destroyed by the triturating action of coast waves, that the discovery of any of this kind of fossil remains in any formation may also be regarded as presumptive evidence of its non-marine origin.

The foregoing statements have been made with reference to indications which are either of a general character or without direct relation to the quality of the waters in which sedimentary formations have been deposited. All the direct evidence, as has been already fully stated, is derivable from the fossil remains of the denizens, especially the gill-bearing kinds, of the waters in which were deposited the formations under investigation.

Referring to the previous review of the animal kingdom, it will be seen that a large number of families of both fishes and invertebrates are confined to a marine habitat, and that every member of even some of the higher divisions is similarly restricted. For example, every known member of the classes Cephalopoda and Brachiopoda is confined to a marine habitat. It will also be seen that a certain small number of families, especially of the mollusca, are equally restricted to fresh waters. The significance of such cases as these has already been pointed out, but it is desirable to refer to them again.

Fossil remains representing any one of these kind of animals may be taken as positive evidence of the quality of the water in which the formation containing them was deposited, provided there shall be no room for reasonable doubt that the animals were really denizens of that water. That is, caution is necessary even in these more positive cases, especially when the amount of discovered fossil material is meagre.

Not only caution but the exercise of careful judgment is necessary in other cases. For example, it will also be seen by referring to the foregoing review that certain families, while most of its members are confined to one kind of water, may have one or more representatives in other kinds; and again, that certain families may have representatives in all the known kinds of habitable waters. In such cases as these it is plain that all evidence afforded by fossil remains, to be of any value, must be corroborated by other evidence.

Still, the cases are very few in which serious doubt need be entertained as to the true character of the water in which a given formation was deposited. This is especially true if the fossil remains are sufficient in quantity and perfection to approximately represent the whole fauna that lived in those waters. Indeed, if the facts which are recorded in this review are borne in

mind, there need be no more doubt as to what was the quality of the water in which any given formation was deposited, than might arise concerning any other geological observation.

THE CLAIMS OF GEOLOGICAL SCIENCE UPON INVESTIGATORS, MUSEUMS, &C.

With reference to the ordinary pursuits of life it can hardly be said that, apart from a natural demand for respectable emulation, one's occupation has any claims upon him other than those which are either conventionally or legally imposed by society upon every one of its members. The geological investigator, however, is not only amenable to all such claims, but to others of a different nature which, although not enforceable by legal, and unfortunately not yet by conventional, penalties, are not less imperative in their character.

Much might be said in favour of the demands which may be made in the name of science upon the individual on the ground of justice and of moral and social ethics; but all considerations of this kind will be omitted, reference only being made to those claims which are supported by the urgent necessities of science itself. Claims of the kind referred to might be made in favour of all the various divisions of science; but on the present occasion the discussions will be confined to those which pertain to biological geology, including both its structural and systematic branches. With reference to the manner in which the subject is presented, it is proper to say that thehomiletic form has not been adopted merely from personal preference, but because it appears to be in the present case a proper and effective, if an indirect, method of calling attention to prevalent errors, and of suggesting necessary improvements in certain prevalent methods.

These claims of science will be considered not only with reference to the individual investigator, but to associations, museums, and geological organisations. Those which may be made upon the individual investigator relate to the manner of prosecuting his work and of publishing its results, and also to his final disposition of the evidence upon which his conclusions are based. Claims upon associations or societies relate to the character and methods of publication; those upon museums, to the conservation and installation of fossil remains, and of the records pertaining to them; and those upon organisations, to the preservation of the integrity of geological science.

In considering the claims of science upon the individual, it is desirable to make some reference to the amateur as well as to the special investigator. This recognition of non-professional work is desirable because the general subject of geology has acquired such a hold upon the popular mind, and the opportunities for making observations with relation to it are everywhere so common, that in every civilised country there is a multitude of persons who are in the habit of making more or less critical observations. Notwithstanding the usual limited and desultory character of such observations, they have often contributed materially to the general fund of geological knowledge, especially when accompanied by a faithful record and preservation of evidence. Indeed, some of the most valuable facts in geology have been brought out by amateur observers, who themselves were hardly conscious that they had made their way alone to the frontier of acquired knowledge; and from the ranks of such observers have arisen many of the leaders in geological investigation.

It has been shown that systematic geology could have no existence without the use of fossil remains, and also that without their use structural geology would be reduced to mere local and disconnected studies. It has also been shown that to arrive at a just estimate of the value of fossil remains in these branches of geology they must be thoroughly and systematically studied as representatives of faunas and floras, as well as tokens of the formations in which they are found. The proper collection and preservation of fossil remains is therefore a subject of the greatest importance. In view of these facts it is the plain duty of every geologist, upon beginning a piece of field-work in structural geology, to accompany every step of his examination of the strata by as full a collection as possible of the contained fossils, and to preserve them, together with notes recording the results of his observations and a statement of all the facts relevant thereto.

Fossils thus collected, and the facts concerning them recorded, become invested with a value which differs materially from that which is possessed by ordinary property, and the claims of science upon them and upon the investigator with relation to them at once begin. These claims, as just intimated, require that a careful descriptive record be made of the stratigraphical conditions under which the fossils are found, including a directive record of the locality and designation of the stratum from which they were obtained. They also require that these records should be inviolably preserved and made inseparable from every specimen by indices that shall be as intelligible to other investigators as to the original observer.

Apart from the claims of science such precaution is necessary, because reliance upon memory alone is always unsafe in the most favourable cases, and it can at best give rise only to such oral traditions as are out of place in scientific work. The immediate preparation of the records and indices just mentioned is also necessary, because, while every specimen is at all times competent to impart to an investigator all obtainable knowledge of its own character, it can of itself convey no information as to its original locality and stratigraphic position. With this information secured for a collection of fossils they may be made at all times available as aids to scientific research, not only by the collector, but by all other investigators.

The claims of science also require that immediately upon the completion of the original study of fossils thus collected and recorded, they shall be placed where they will be freely accessible to the scientific public, and that reference to their place of deposit shall be made in connection with their publication. It is needless to say that the only suitable places for such deposit are public museums. It is only when this indispensable evidence is thus made accessible that the public can exercise that arbitration over the accumulated results of the labours of investigators which has been shown to be imperative.

The preparation and publication of complete records concerning the locality and strata from which fossil remains are obtained are necessary even from a biological point of view alone, especially when those remains are studied with reference to the range of organic forms in time, and without such records fossil remains are comparatively worthless as aids in geological investigation. It is unfortunately true that a not unimportant proportion of the palæontological material contained in our best museums is without these essential records, and that many of the publications containing descriptions and illustrations of fossil remains give no satisfactory information as to the localities and strata from which they were obtained, or of the final disposition of the specimens. In such cases those authors and collectors have evidently assumed to decide for themselves and for science the exact taxonomic position in the geological scale of the strata from which their fossils came. In omitting such records as have been referred to, they seem to have considered any information unnecessary that would enable the scientific public to repeat their observations upon their specimens, or those which they may have made in the field, or to learn the biological characteristics of the formations from which their collections were obtained other than those which may be suggested by their own partial collections and their necessarily imperfect descriptions. It is doubtless true that such omissions have been largely due to an honest lack of appreciation on the part of authors and collectors of the importance of preserving such records, but it is to be feared that in some important cases the omissions or suppressions have been intentional. In the former class of cases the fact can only be deplored, but in the latter every geologist is justified in feeling that a crime has been committed against science.

The claims of geological science upon associations and societies are so generally and justly recognised, that only the one which relates to the manner of publishing the results of investigation need be referred to in this connection, and this reference will be confined to the necessity of enforcing the claims upon individual investigators which have already been discussed. This claim may be sufficiently indicated by reference to those last mentioned, and by the remark that if it is the duty of individuals to publish records of their observations in the manner that has been stated, it is plainly the duty of those persons who may be in charge of the means of publication to refuse to publish the writings of those authors who do not conform to that requirement.

The facts and principles which have been stated fully warrant the statements that individual authority can have no existence with relation to geological science, that the public must be the final arbiter of all questions concerning the value of proposed contributions to its advancement, and that a public exposition should be made of the evidence upon which any contribution to biological geology is based. In accordance with the last-named requirement it is necessary to consider the claims of this branch

of science upon museums, the force of which is apparent when it is remembered that the material pertaining to it therein stored constitutes the vital evidence of the value of all contributions to its advancement, and that without such evidence this branch of science would be reduced to a mass of personal testimony.

In view of the great scientific value of fossil remains the following remarks are offered concerning the precautions which are necessary in their preservation. It is true that most, if not all, these precautions are observed in a large part of the principal scientific museums of the world, but it is also true that much remissness in this respect has occurred in others. Besides the propriety of referring to the latter fact, these remarks are necessary to complete my statement of the claims of science which constitute the subject of this essay.

Three general classes of specimens of fossil remains should be recognised in museum collections, namely, typical, authenticated, and unauthenticated. Under the head of typical or type specimens are included not only those which have been described and figured in any publication, whether original or otherwise, but those which have in any public manner been so used or referred to. While all such specimens as these should at all times be accessible to any competent investigator, the risk of loss or injury is so great that they should in no case be allowed to be taken from the museum building in which they are installed. Such specimens are in a peculiar sense unique, and there can be no substitution and no equivalent in value. Their loss greatly reduces the value of every publication any part of which is based upon them, and to that extent retards the advancement of science. It is not enough that other, and even better, specimens of presumably the same species may be discovered; the former constitute the original, the latter only suppositious evidence. Besides the risk of loss or injury to type specimens by removal from the place of their instalment, their absence is a disadvantage to science. That is, no one investigator should be allowed their use to the exclusion of any other.

The term "authenticated specimens" is here applied to such as have been studied and annotated by competent investigators and properly installed. Such material constitutes the bulk of every important museum collection, and next to the type specimens already mentioned, they are most valuable. Their increased value is due to the scientific labour that has been bestowed upon them, and it needs only the additional labour of publication to constitute them type specimens and to make them of like value. Authenticated specimens when installed are ready aids to all investigators of such value, that even the temporary removal of any of them from a public museum is, to say the least, of doubtful expediency.

Unauthenticated specimens are, of course, those which have not been studied and installed, and they constitute the great mass of material from which authenticated and type specimens are drawn. Among them are those which constitute the material evidence upon which original observations in biological geology are based. If these are accompanied by the records and descriptive notes which are essential to their value, they constitute proper material for acceptance by museum authorities; but if not, their instalment should be refused, whatever their character may be. That is, to apply a statement made in another connection, no specimen of fossil remains should be admitted to permanent installation in any public museum which is not accompanied by such a record of the locality and stratum from which it was obtained, as will enable any investigator to revisit the same. In every case of instalment such records should be so connected with every specimen as to be readily accessible, and so arranged that the danger of loss or disconnection shall be reduced to a minimum.

The foregoing discussion of the claims of science upon museums is intended to embrace reference only to those which are devoted to the preservation of material pertaining to biological geology, but they are of more or less general applicability. These partial [illegible] demonstrate the important relation that museums hold to science and to civilisation as centres of learning and [illegible] of the evidence concerning acquired knowledge. Museums should not only be made safe treasure houses of science, but they should be what their name implies—temples of study, [illegible] open to all investigators.

The claims of science upon geological organisations cannot be [illegible] here, but because the ratio of power for the advancement or retardation of science possessed by such organisations is much greater than that of individuals working [illegible], it is desirable to make this brief reference to them. That power increases also with the ratio of the extent of the organisation, and it is largely centred in the director. His responsibility, especially if his organisation is a large one, is peculiar, and, to himself, of an unfortunate character. That is, while all, or nearly all, the advancement of science that may be accomplished by the organisation is the work of his subordinates, retardation, if it should occur, is mainly due to his failure to require that each branch of investigation should be prosecuted in accord with all others, and the case would be little less than disastrous should he himself favour *ex parte* methods, or fail to require a symmetrical development of the work in his charge. The claims of science upon geological organisations are therefore really claims upon their directors, and they are more responsible than any other class of persons for the preservation of the integrity of geological science.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

AT a meeting of the Council of University College, Dundee, last week, it was announced that the trustees of the late Miss Margaret Harris had allocated a number of securities, valued at nearly £14,000, to establish a chair of Physics in the College, as recommended by the University Commissioners. The Council resolved to institute immediately a chair of Natural Philosophy; and an appointment will be made before the beginning of next session. Hitherto the classes of Mathematics and Physics have been combined. The salary will be £400 with share of the fees.

THE invaluable *Record* of technical and secondary education continues, in the quarterly number just issued, the review of the work done by the Technical Education Committees of the English County Councils, commenced in the preceding issue. A summary is also given of the work of the Scotch County Councils, from which it appears that, out of a total of thirty-three County Councils, twenty-four are devoting the whole, and seven a part, of their grants to educational purposes, while two counties are applying the whole of the fund to the relief of the rates. Out of a total sum of £25,157 distributed among the County Councils of Scotland, £22,491 was devoted to education in the year 1893-94. Mr. P. J. Hartog contributes to the *Record* an illustrated description of the Owens College, Manchester.

THE Town Trustees of Sheffield have (says the *Athenæum*) voted a sum of £10,000 towards the endowment of Firth College, with a view to enabling the authorities to affiliate it to Victoria University. The actual endowment of the College is £23,000, in addition to its income of £1200 from the State and £800 from the Corporation. It is understood that a total of £50,000 would be sufficient, but no more than sufficient, for the purpose of affiliation. A further sum of £5000 has been conditionally promised by Sir Henry Stephenson, and a public appeal is contemplated for the remaining £12,000.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for March 1895 contains:—On the variation of the tentaculocysts of *Aurelia aurita*, by Edward T. Browne. (Plate 25.) Of 359 Ephyræ collected in 1893, 22·6 per cent. were abnormal in possessing more or less than eight tentaculocysts; and of 1156 collected in 1894, nearly the same percentage, 20·9 was obtained. Of 383 adult Aurelia collected in 1894, 22·8 per cent. were abnormal.—On the structure of *Vermiculus pilosus*, by E. S. Goodrich, gives a detailed account of this interesting Oligochæte, found near Weymouth in 1892. (Plates 26-28.)—On the mouth parts of the Cypris stage of Balanus, by Theo. T. Groom. (Plate 29.) "It may be regarded as tolerably certain that: (1) The antennæ of the Nauplius become definitely lost with the moult resulting in the production of the Cypris stage. (2) The biramous mandibles of the Nauplius become reduced at the same time to the small mandibles, the ramus being probably preserved in the form of the small palp. (3) The first pair of maxillæ arise behind the mandibles, and at a later date, as a small pair of foliaceous appendages. (4) The second pair of maxillæ arise still later, just in front of the first pair of thoracic legs (cirri)."—A study of Coccidia met with in mice, by J. Jackson Clarke. (Plate 30.)—Observations on various Sporozoa, by the same. (Plates 31-33.)—Revision of the genera and species of the

Branchiostomidæ, by J. W. Kirkaldy (Plates 34 and 35), enumerates two genera, Branchiostoma (as sub-genera, Amphioxus, Heteropleuron) and Asymmetron. A new species of Heteropleuron, *H. cingalense*, is described.—On Sedgwick's theory of the embryonic phase of ontogeny as an aid to phylogenitic theory, by E. W. MacBride.

June.—On the anatomy of *Alcyonium digitatum*, by Prof. Sydney J. Hickson (Plates 36-39), gives a brief account of our knowledge of the anatomy of Alcyonium, the general morphology, the English species, their geographical and bathymetrical distribution, then the general anatomy, followed by the minute anatomy of the ectoderm, nematocysts, stomodæum, mesenterial filaments, mesoglœa, spicules, endoderm, ovaries and testes, the buds, concluding with a note on the circulation of the fluids in the colony and on the digestion. In the history of investigations, Pallas' name is not alluded to, and yet he deserves to be quoted as having even before Savgny assigned correct characters to Alcyonium ("Hist. nat. des Coralliaires," Milne-Edwards, tome 1, p. 114), and the "Contribution a l'anatomie des Alcyonaires," by Pouchet and Myevre, dates, if we mistake not, before Vogt and Jung's account in their "Lehrbuch," and while it may be of little use to the student, it is not without interest, as it figures, after a fashion, the nematocysts in *A. digitatum*, and this possibly for the first time (1870). Prof. Hickson, however, leaves all previous writers far behind in his modern treatment of this subject, and if he keeps his promise of publishing an account of the maturation and fertilisation of the ovum and its development, he will leave us under still further obligations, for except Kowalevsky and Marion's important papers on the developmental history of *Clavularia crassa* and *Sympodium coralloides*, we have but little light on Alcyonarian development.—Note on the chemical constitution of the mesoglœa of *Alcyonium digitatum*, by W. Langdon Brown. It is chiefly composed of a "hyalogen" prior to the conversion of the hyalogen into hyalin the mesoglœa will yield a mucin; it also contains a small amount of an insoluble albuminoid body, whose nature was not determined; it does not contain gelatine or nucleo-albumen. A study of metamerism, by T. H. Morgan. (Plates 40-43). The author in a long memoir, that does not admit of being briefly abstracted, thinks that the cases he cites show very positively that the variations appearing in a radiate animal must have come simultaneously and all together into the antimeres; he thinks few will doubt that the relation existing between repeated organs in a radiate animal is at bottom the same relation existing between the right and left sides of the body of a bilateral animal. Mivart and Brooks have emphasised the further fact that the relation between the right and left sides of the body is the same relation that exists between the serially repeated parts of a metameric animal; and he concludes that if this line of argument be admitted, it puts the problem of metamerism into a large category of well-established facts.—On the Cœlom, genital ducts, and Nephridia, by Edwin S. Goodrich. (Plates 44-45). The chief object of this paper is to call attention to the theory, "that the cavity which is known as the cœlom in the higher Cœlomata is represented by that of the genital follicles in the lower types of that grade."

American Journal of Science, July.—On the pitch lake of Trinidad, by S. F. Peckham. This pitch lake is situated near the village of La Brea, on the Gulf of Paria. At first sight it appears to be an expanse of still water, frequently interrupted by clumps of trees and shrubs, but on a nearer approach it is found to consist of mineral pitch with frequent crevices filled with water. The consistence of the surface is such as to bear any weight, and it is not slippery nor adhesive. It is about 100 acres in extent. It occupies a bowl-like depression in a truncated cone on the side of a hill covered with tropical jungles. The cone consists of both asphalt and earth. A heavy stream of asphalt has overflowed to the sea, forming a barrier reef for a considerable distance. Asphalt has also overflowed to the south, and the general appearance of the escarpment seems to indicate that at some remote period the basin now occupied by the lake had been filled some three feet higher than the present level. It occupies what appears to be the crater of an old volcano. Some diggings have been pushed to forty feet without reaching the bottom. There is a steady outflow towards the sea through the side of the cone. The Trinidad Bituminous Asphalt Company have lately run a tramway from the pier through the lake and back, so as to facilitate the removal of the material. This tramway in crossing the lake is supported on palm-leaves, some of which are 25 feet long, and this plan has answered every purpose.—On some reptilian remains from the Triassic of Northern California, by John C. Merriam. The author gives a description of some of the few Californian Mesozoic reptiles. One of these resembles *Ichthyosaurus*, while the other is described as *Shastasaurus Pacificus*.—A further contribution to our knowledge of the Laurentian, by Frank D. Adams. This paper is accompanied by a map of a portion of the edge of the Archean protaxis north of the island of Montreal, Quebec. There are in the district considered at least two distinct sets of foliated rocks. One of these represents highly altered and extremely ancient sediments, while the other is of igneous origin.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 16.—"On Measurements of Small Strains in the Testing of Materials and Structures." By Prof. J. A. Ewing, F.R.S.

The paper describes a new form of "extensometer," or apparatus for measuring the elastic stretching of bars subjected to pull in the testing machine or otherwise. At the two extremities of the length under test, which is usually eight or ten inches, two cross-pieces are attached to the rod by means of a pair of diametrically opposed set-screws. Each piece is separately free to oscillate about the line joining its screw points, since it touches the rod under test at no other place, but the two pieces are caused to engage with each other in such a way that when the rod extends the end of one of the pieces becomes displaced through a distance which is proportional to the extension. The amount of this displacement is measured by means of a microscope attached to the other piece. The whole apparatus is self-contained, and the parts are arranged to have no unnecessary constraint. Its indications show the mean extension taken over the whole section of the rod, and are independent of any small amount of bending or twisting which the rod may undergo as it is stretched. The microscope is furnished with an eye-piece micrometer which reads the extension to $\frac{1}{50000}$ inch, and a calibrating screw is provided for testing and setting the micrometer scale. Two forms of the instrument are described, one suitable for laboratory use when the specimen under test stands vertically, and the other applicable to rods in any position, such as the members of bridge or roof frames *in situ*. In the laboratory use of the instrument the elastic properties of the material are examined by observing the strains under known loads; in the application to structures the object is to determine experimentally what the stress on any member is, from observation of the strain, the modulus of elasticity being assumed.

The author describes a number of observations made with the new extensometer, chiefly on rods of iron and steel. The following readings refer to successive loadings of a bar of steel, which conforms closely to Hooke's Law, the loads being well within the primitive elastic limit. They serve to illustrate the sensibility of the instrument. The zero of the extensometer was set at 400, and the unit of its scale was $\frac{1}{50000}$ inch. The bar was $1\frac{1}{4}$ inch in diameter, and the length under test was 8 inches.

Load in tons.	Extensometer readings.			Differences.		
	First loading.	Second loading.	Third loading.	First loading.	Second loading.	Third loading.
0	400	400	400			
$2\frac{1}{2}$	461	461	461	61	61	61
5	522	522	522	61	61	61
$7\frac{1}{2}$	583	583	583	61	61	61
10	645	645	645	62	62	62
$12\frac{1}{2}$	707	706	707	62	61	62
15	769	768	768	62	62	61
$17\frac{1}{2}$	830	829	830	61	61	62
20	892	891	891	62	62	61
0	400	400	400	492	491	491

In other experiments the rod under examination was allowed to become overstrained, that is to say the load was increased until the elastic limit was passed and permanent set was produced. In this condition the elastic properties of the rod are materially

different from its properties in the primitive state. On reloading the overstrained rod it is found that the proportionality of strain to stress no longer holds good, even under very light loads, and further that there is "creeping," or continued extension with the lapse of time, when any load is kept on for a few minutes. Again, on removing load the bar continues to retract for some time. These features of the overstrained state are most conspicuous in tests made directly after the overstrain has taken place. They tend to disappear if the bar is allowed to rest for some days or weeks. This elastic recovery with the lapse of time, some features of which have been already noted by Bauschinger and others, is less rapid in moderately hard steel than in iron or mild steel, apparently because the condition of overstrain requires a greater load to produce it. Thus a rod of common iron, overstrained so much that the yield-point was reached, was found to have made a practically complete recovery of its elasticity in five days. On the other hand, in a rod of rather hard steel, overstrained by applying a load of 11 tons and subsequently tested with loads of 8 tons only, the recovery was still imperfect after three weeks. The following table shows the progress of the recovery by giving the observed extensions of this rod after three intervals, namely ten minutes, one day, and three weeks, after the overstrain took place.

Load in tons.	Ten minutes after overstrain.		One day after overstrain.		Twenty-one days after overstrain.	
	Extensometer readings.	Differences.	Extensometer readings.	Differences.	Extensometer readings.	Differences.
0	200	—	200	—	200	—
1	287	87	286	86	285	85
2	377	90	373	87	371	86
3	469	92	463	90	458	87
4	565	96	559	96	545	87
5	662	97	658	99	632	87
6	760	98	758	100	720	88
7	866	106	860	102	810	90
8	976	110	963	103	900	90

The molecular settlement which is shown by these experiments to be going on for some time after overstrain has taken place, is known to be associated with a rise in the yield-point. Instances of this were given by the author in a previous paper (*Proc. Roy. Soc.*, No. 205, 1880).

May 30.—"On the Motions of and within Molecules; and on the Significance of the Ratio of the Two Specific Heats in Gases." By Dr. G. Johnstone Stoney, F.R.S.

In treating of molecular physics it is found to be convenient to widen the meaning of the word motion, so that it may be employed in regard to any change or event in which energy is stored, whether as kinetic energy, or as potential, electrical, chemical, or any other. It is in this generalised sense that the term is to be understood throughout this paper.

The aim of the paper is to demonstrate the existence of events going on within the molecules of matter which are so sluggish in affecting its pressure when in the gaseous state, or its temperature as measured by the thermometer, that it is only after millions of encounters that any manifestation of their having thus lost energy by conduction becomes appreciable; while these same events are prompt and active agents in other operations of nature through chemical reactions or by radiation.

Molecular events may be distinguished into A or external events, and B or internal. The external events are the movements of the centres of inertia of the molecules relatively to one another. They present themselves most conspicuously in those comparatively protracted journeys which the molecules of gases make between their much briefer encounters. By B motions are to be understood all events in which energy can be stored that go on within individual molecules, including rotation of the molecule if there be any movement of this kind, which, however, is not probable; along with every other relative motion of the parts of the molecule, movements within its ponderable matter, or of its electrons, changes in the configuration of its parts, and every other event within the molecule which can absorb and yield energy. The electrons are those remarkable charges of electricity, all of the same amount, which are associated in every chemical atom with each capacity that it possesses of entering into combination with other atoms.

It is convenient to distinguish the B or internal events, into Ba events between which and the A or translational motions of the molecules there is ready interchange of energy whenever encounters take place: Bc events which are so isolated that no such interchange takes place: and Bb events which lie between these extremes. In the struggle which takes place during an encounter, or in any one of the much longer intervals between two encounters, a Bb event will part with but very little of any excess of energy it may possess by conduction, *i.e.* by transferring energy over to A or Ba events. Nevertheless it may sustain an appreciable loss of energy in this way when the molecule has been buffeted in a sufficient number of encounters. This may easily occur in a time which seems short to us, since, if the gas be at atmospheric temperature and pressure, each molecule meets with some thousands of millions of encounters every second. Meanwhile, during this process, which is slow from the molecular standpoint, the Bb events, if they have electrons associated with them, may be engaged in a prompt and active exchange of energy with the æther by radiation.

In substances that are appreciably phosphorescent, it is easy to detect the presence of these Bb events; and, accordingly, a proof that they exist in this class of bodies is given in the paper. Moreover, by comparing the behaviour of different phosphorescent bodies, we learn that the degree of isolation in which Bb motions stand varies much from substance to substance. Motions of this type, which are so conspicuous in the bodies that can be perceived to be phosphorescent, are, of course, not confined to that class of bodies. In fact, they appear to be an important part of what is going on in every molecule of matter that can emit a spectrum, a description which probably embraces every molecule.

Since Bb motions are in various degrees isolated from the other events that are simultaneously going on in the molecules, it follows that in some gases the specific heat as determined by experiment will not be a definite quantity, but will partly depend on the duration of the experiment by which it is determined—*i.e.* upon whether or not there has been time for an interchange of energy between the Bb motions and the A and Ba events. This is likely in some gases to make an appreciable difference between determinations of γ—the ratio of the two specific heats—deduced from the observed velocity of sound in the gas (where the real experiment lasts only during one semi-vibration of the musical note employed), and determinations made by other experiments which require seconds, perhaps minutes, to carry them through.

There is reason to believe that it is with these Bb motions that the electrons within chemical atoms are chiefly associated, and that in most cases it is they which are concerned in luminous effects, whether in flames or when the gas is under the influence of electricity. Accordingly in both cases the luminous effects may have their origin in events that are in a considerable degree isolated from those that directly affect the thermometer; and wherever this is the case, the luminous effects will be in excess of what belongs to the temperature of the gas as determined by its power of communicating heat by conduction to bodies upon which its molecules impinge. This seems to have been proved by Prof. Lewes of flames (*Proceedings* of the Royal Society, vol. lvii. p. 404 and p. 467), and many phenomena indicate that it is also true of all gases which exhibit spectra of bright lines when in that state which has been miscalled incandescent.

It is specially to be noted that the interpretation usually put upon the value of γ in a gas has to be profoundly modified in consequence of the presence of Bb motions within the molecules, and of the degree in which the corresponding Bb motions of swarms of molecules are more or less linked together by the interaction that goes on between their associated electrons and the æther. (See Fitzgerald, in the *Proceedings* of the Royal Society, vol. lvii. p. 312.)

These examples may serve to show how a knowledge of the presence and activity of Bb motions supplies a clue to interpreting some of the phenomena of nature; and the extent of its applications may be judged by reflecting that it is electrons for the most part associated with Bb motions which appear to be primarily concerned in every chemical reaction and in all phenomena of radiation.

"On the Velocities of the Ions." By W. C. Dampier Whetham. A continuation of a former paper (*Phil. Trans.* 184, 1893 A, p. 337). The velocities of certain ions

during electrolysis are observed by tracing the formation of the precipitates which they give with a trace of a suitable indicator. Thus solid agar jelly solutions of barium chloride and of sodium chloride containing a little sodium sulphate were set up in contact, and a current passed across the junction. The barium ions form a little insoluble barium sulphate as they travel, and so their velocity can be measured. The specific ionic velocity under a potential gradient of one volt per centimetre can then be calculated, the area of cross section of the tube, in which the solutions are placed, the mean specific resistance of the solutions, and the strength of current being known. The following table gives a comparison between the results thus obtained and the numbers theoretically deduced by Kohlrausch from the migration constants and the conductivities of the corresponding aqueous solutions:—

	Calculated velocity in c.m. per sec.	Observed velocity in c.m. per sec.
Barium	0·00037	0·00039
Calcium	0·00029	0·00035
Silver	0·00046	0·00049
Sulphate group (SO_4)	0·00049	0·00045

June 20.—"On the Occlusion of Oxygen and Hydrogen by Platinum Black." Part I. By Dr. Ludwig Mond, F.R.S., Prof. W. Ramsay, F.R.S., and Dr. John Shields.

The authors describe some preliminary experiments on the occlusion of oxygen and hydrogen by platinum sponge and foil, which in general confirm the results obtained by Graham. At most only a few volumes of these gases are occluded by the more coherent forms of platinum.

After giving details of what they consider the best method of preparation of platinum black, they next describe some experiments which had for their object the determination of the total quantity of water retained by platinum black, dried at 100° C., and the amount of water which can be removed from platinum black at various temperatures in vacuo. As the result of these experiments they find that platinum black dried at 100° retains in general 0·5 per cent. of water, and this can only be removed in vacuo at a temperature (about 400°) at which the black no longer exists as such, but is converted at least partially into sponge. At any given temperature the water retained by platinum black seems to be constant. The density of platinum black dried at 100° C. is 19·4, or allowing for the water retained by it at this temperature, 21·5.

The amount of oxygen given off by platinum black at various temperatures was determined. Altogether it contains about 100 volumes of oxygen; the oxygen begins to come off in quantity at about 300° C. in vacuo, and the bulk of it can be extracted at 400° C., but a red heat is necessary for its complete removal. Small quantities of carbon dioxide were also extracted, chiefly between 100-200° C.

In determining the quantity of hydrogen occluded by platinum black the authors have carefully distinguished between the hydrogen which goes to form water with the oxygen always contained in platinum black, and that which is really absorbed by the platinum *per se*. Altogether about 310 volumes of hydrogen are absorbed per unit volume of platinum black, but of this 200 volumes are converted into water, or only 110 volumes are really occluded by the platinum. Part of it can be again removed at the ordinary temperature in vacuo; by far the larger portion can be extracted at about 250-300° C., but a red heat is necessary for its complete removal. The amount of hydrogen absorbed by platinum is very largely influenced by slight traces of impurity, probably grease or other matter which forms a skin over the platinum.

Platinum black in vacuo absorbs a certain quantity of hydrogen. On increasing the pressure of the hydrogen up to about 200-300 mm. a further quantity is absorbed, but after this pressure is almost without effect. By increasing the pressure from one atmosphere up to four and a half atmospheres, only one additional volume of hydrogen was absorbed. On placing platinum black charged with oxygen in an atmosphere of oxygen, and increasing the pressure to the same extent, eight and a half additional volumes were however absorbed.

Platinum black charged with hydrogen and placed in an atmosphere of hydrogen kept approximately at atmospheric pressure, and platinum black charged with oxygen and confined in an atmosphere of oxygen, behave quite differently when heated. In the former case hydrogen is immediately expelled on raising the temperature, whilst in the latter case oxygen is steadily absorbed until a temperature of about 360 C. (the temperature of maximum absorption) is reached, when on further heating oxygen begins to come off again.

Incidentally it was noticed that mercury begins to combine with oxygen at 237° C., and that a mixture of platinum black and phosphorus pentoxide absorbs oxygen at a high temperature, probably with the formation of a phosphate or pyrophosphate.

In the discussion of the results special reference is made to the work of Berliner and Berthelot, and it is pointed out that there is not sufficient evidence for the existence of such chemical compounds as $Pt_{30}H_3$ and $Pt_{30}H_2$. Moreover, the authors are of opinion that the heats of combination of hydrogen and platinum as determined by Berthelot and Favre are valueless, and that the heat which they measured is due for the most part if not entirely to the formation of water by the oxygen always contained in platinum black. It has yet to be *proved* that the absorption of hydrogen by pure platinum black is attended by the evolution of heat, and as regards the formation of supposed true chemical compounds, solid solutions, or alloys, the authors prefer to wait until sufficient data have been accumulated for an adequate inquiry before coming to any definite conclusion.

Royal Microscopical Society, May 15.—E. M. Nelson, Vice-President, in the chair.—Messrs. Watson and Sons exhibited a simple centring underfitting for use with any ordinary student's microscope.—The Chairman exhibited a new low-lower lens by Zeiss, and a new photographic lens.—Mr. W. C. Bosanquet read a paper on the anatomy of *Nyctotherus ovalis*.—Mr. G. C. Karop read a paper, by Dr. A. Bruce, describing a new microtome for cutting sections.—The Chairman announced that the library would be closed from August 12 to September 9, and that the next meeting would be on October 16.

Mineralogical Society, June 18.—Lewisite and Zirkelite, two new Brazilian minerals, by Dr. E. Hussak, of the Geological Survey of São Paulo, and Mr. G. T. Prior. Lewisite is a new titano-antimonate of calcium and iron, which was found with xenotime, monazite, cinnabar and other minerals in the heavy sand obtained by washing the gravel from a hill slope at the cinnabar mine of Tripuhy, Minas Geraes, Brazil. It is cubic, occurs in small brown translucent octahedra, and has the composition $5RO.3Sb_2O_5.2TiO_2$. Zirkelite is a new titano-zirconate of calcium and iron found in association with the new zirconia mineral baddeleyite in the magnetite-pyroxenite from Jacupiranga, São Paulo, Brazil. It is cubic, occurs in black octahedra, and contains about 80 per cent. of ZrO_2 and TiO_2. The authors describe the physical and chemical characters of the two minerals, and also give an account of the minerals associated with the Lewisite at Tripuhy; amongst these occurs sparingly a new titano-antimonate of iron, the description of which will be completed when more material is obtained.

PARIS.

Academy of Sciences, July 8.—M. Marey in the chair.—On the physical characteristics of the moon and the interpretation of certain surface details revealed by photographs, by MM. Lœwy and P. Puiseux. A general discussion of surface characters of the moon and their origin, and comparison with certain terrestrial features of possibly similar origin.—On the manner in which any confused but periodic wave-agitation becomes regular in the distance, reducing to a simple wave, by M. J. Boussinesq.

Action of zinc chloride on resorcinol, by M. E. Grimaux.—Comparison of the work done by muscles in the case of positive work with that developed in the corresponding case of negative work, by M. A. Chauveau.—Law of the distribution of mean magnetism at the surface of the globe, by General Alexis de Tillo.—Volumes of salts in their aqueous solutions, by M. Lecoq de Boisbaudran. The author considers all soluble substances to belong to a continuous series of which the members at the one end may show dilatation on solution, whereas the members at the other end may exhibit contraction under similar circumstances. He illustrates his theory by examples demonstrating that the former at low temperatures give contraction also on solution, whereas the bodies usually showing contraction on solution exhibit dilatation on solution in sufficiently concentrated

solutions.—On diphenylanthrone, by MM. A. Haller and A. Guyot. The researches detailed prove that the substance $C_{26}H_{18}O$ is diphenylanthrone, $C_6H_4 \begin{matrix} C(Ph)_2 \\ CO \end{matrix} \diagup C_6H_4$. From this established constitution, the phthalyl tetrachloride melting at 88° C. must have the dissymmetrical formula, $C_6H_4 \begin{matrix} \diagup CCl_3 \\ \diagdown COCl \end{matrix}$.—A new lymphatic gland in the European scorpion, by M. A. Kowalewsky. The gland described has already been made known by J. Muller, who, in 1828, termed it a salivary gland.—On the laws of friction in sliding, by M. Paul Painleve. The conclusion is deduced, from the singularities developed in the paper, that the empirical laws of friction are logically inadmissible (even for ordinary pressures and velocities) so soon as the friction becomes at all noticeable.—On the mirage effects and differences of density observed in Natterer's tubes, by M. P. Villard.—On explosive statical and dynamical potentials, by M. R. Swyngedauw.—On direct spectroscopical analysis of minerals and of some fused salts, by M. A. de Gramont. Determinations of the solubility, at very low temperatures, of some organic compounds in carbon disulphide, by M. Arctowski. Etard found the solubility of substances to be represented for other solvents than water by curves practically of hyperbolic form of which the branches respectively directed themselves towards the points of fusion of the solvent and of the dissolved substance; he even admitted that the solubility would be zero at the point of congelation of the solvent, and infinite at the point of fusion or ebullition of the dissolved substance. The author finds, with carbon disulphide, that the point of fusion of the solvent appears not to be an essential point on the curve of solubilities; and it is otherwise known that the property of dissolving is not an exclusive property of the liquid state of matter.—On some oxidising properties of ozonised oxygen and of oxygen in sunlight, by M. A. Besson.—Action of nitric oxide on some metallic chlorides: ferrous, bismuth, and aluminium chlorides, by M. V. Thomas. A fine red ferrous compound has been obtained of the formula $5Fe_2Cl_4.NO$. By decomposition of this, or by suitably heating anhydrous Fe_2Cl_4 in a current of nitric oxide, yellowish brown $Fe_2Cl_4.NO$ is obtained. A fine yellow bismuth compound and a pale yellow aluminium compound have also been obtained. They are very hygroscopic substances, and have the composition $BiCl_3.NO$ and $Al_2Cl_6.NO$ respectively.—Action of halogens on methyl alcohol, by M. A. Brochet.—On a physical theory of the perception of colours, by M. Georges Darzens.—On the presence and the *rôle* of starch in the embryonic sac of Cacti and Mesembryanthema, by M. E. d'Hubert. The observations favour the view that starch serves to preserve the embryonic sac in these plants in that state which characterises the ripe and readily fertilised sac.—On the tectonic characters of the north-west part of the Alpes-Maritimes department, by M. Leon Bertrand.—An inferior maxillary human bone found in a grotto in the Pyrenees, by MM. Louis Roule and Felix Regnault. From the characters of the bone described and other similar remains it is concluded that: In the time of the great Cave-bears, France was inhabited by a human race of normal height with a flat and powerful lower jaw.

NEW SOUTH WALES.

Linnean Society, May 29.—Mr. P. N. Trebeck in the chair. Oological notes (continued), by A. J. North. Note on the correct habitat of *Patella* (*Scutellastra*) *kermadecensis*, Pilsbry, by T. F. Cheeseman.—On two new genera and species of fishes from Australia, by J. Douglas Ogilby.—Descriptions of new species of Australian Coleoptera, Part II., by Arthur M. Lea. This paper comprises descriptions of over one hundred species, for the most part referable to the families *Malacodermidæ*, *Mordellidæ*, *Anthicidæ*, and *Corylophidæ*.—Life-histories of Australian Coleoptera, Part III., by W. W. Froggatt.—Description of a giant *Acacia* from the Brunswick River, New South Wales, by J. H. Maiden. This Acacia was collected by Mr. W. Bauerlen on Tergoggin Mountain and on Mullumbimby Creek, Brunswick River, N.S.W. As far as known, it is confined to brushes, as distinguished from open forest. It attains a height of 120 feet and a diameter of 5 feet; it is therefore one of the largest of the genus. Its closest affinity is with *A. binervata*, from which it differs in the structure of the flowers, seeds, and pod, and in other less important particulars. The inflorescence is in loose, elongated panicles or racemes, with peduncles in clusters. The flowers are few—never more than twenty—with villous petals and sepals, which are spathulate and tetramerous. The pod is nearly six lines broad, thin and straight. The author proposes the name of *Acacia Bakeri* for the species, in honour of his colleague, Mr. R. T. Baker.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Open-Air Studies: Prof. G. A. Cole (Griffin).—A Garden of Pleasure (E. Stock).—Dr. Schlich's Manual of Forestry, Vol. 4 (Bradbury).—The Alps from End to End: Sir W. M. Conway (Constable).—Nature *versus* Natural Selection: C. C. Coe (Sonnenschein).—Microbes and Disease Demons: C. Berdoe (Sonnenschein).—The Climates of the Geological Past: E. Dubois (Sonnenschein).—Physikalisch-Chemische Propaedentik Erste Halfte: Prof. H. Griesbach (Leipzig, Engelmann).—Die Physiologie der Geruchs: Dr. A. Zwaardemaker (Engelmann).—Experimental Plant Physiology: D. T. Macdougal (Holt and Co., New York).

PAMPHLETS.—Static and Dynamic Sociology: L. F. Ward (Boston, Ginn and Co.).—On Kaloxylon Hookeri and Lyginodendron Oldhamium: T. Hick.—On the Structure of the Leaves of Calamites (Manchester).—Report of the Trustees of the South African Museum for 1894 (Cape Town).—Returns of Agricultural Statistics of British India, &c., 1893-4 (Calcutta).—Studies on the Dissemination and Leaf Reflexion of Yucca Aloifolia: H. J. Webber (Missouri Botanic Garden).—On the Osteology of Agriochœus: J. L. Wortman (New York).—Fossil Mammals of the Uinta Basin Expedition of 1894: H. F. Osborn (New York).

SERIALS.—Journal of the Royal Statistical Society, June (Stanford).—Record of Technical and Secondary Education, July (Macmillan and Co.).—American Journal of Science, July (New Haven).—Psychological Review, July (Macmillan and Co.).—Engineering Magazine, July (Tucker).—Medical Magazine, July.—Natural History of Plants, Part 14 (Blackie).—Tokyo Sugaku Butsurigakaukwai Kizi Maki, No. vi. Dai 1 and 2 (Syuppan).—Journal of the Franklin Institute, July (Philadelphia).—Bulletin of the American Mathematical Society, June (Macmillan and Co., New York).—Bulletin of the Johns Hopkins Hospital (Baltimore).

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THURSDAY, JULY 25, 1895.

THE DISTRIBUTION OF ANIMALS.

A Text-Book of Zoogeography. By F. E. Beddard, M.A., F.R.S. Cambridge Natural Science Manuals. Pp. viii. and 246. Cambridge: University Press, 1895.)

WITHIN the small limits of 246 duodecimo pages of fairly large type, it is scarcely possible to do justice to such an extensive subject as the geographical distribution of animals; and, bearing in mind the difficulties thus imposed upon him, we think the author of the volume before us is, on the whole, to be congratulated on the manner in which he has completed a very difficult task. He has given the student a large amount of very valuable information, and this in a pleasantly-written and easily-understood form. A writer who was not thoroughly at home in his subject might have contented himself with merely giving us abstracts of Mr. Wallace's works, with such corrections as are necessary in order to bring them up to date. Not so Mr. Beddard, who has introduced into his text-book a very large number of facts, chiefly relating to the lower vertebrates and invertebrates, which are not to be found in more pretentious works, and his volume will thus be of value to all students. As being one of the author's specialities, attention is strongly directed to the distribution of earth-worms; and the remarks concerning the curious relationship between the worms of Patagonia and those of Australia and New Zealand will be found specially interesting.

The general plan of the work is as follows. After defining locality and station, and pointing out the variability in the distributional areas of animals, the author takes a number of selected instances, drawn from very varied classes, of the distribution of particular groups. We have, for example, the range contrasted of such different animals as rheas, ibexes, gallinaceous birds, edentates, tortoises, batrachians, scorpions, planarians, and earth-worms. Having contrasted the differences presented by these groups, Mr. Beddard comes to the consideration of zoological regions; and here he concludes on the whole to adopt those of Messrs. Sclater and Wallace. "As a mere matter of convenience," he remarks, "it is immaterial whether we join Europe, Asia, and North America into one Holarctic region, or use the current terms of Nearctic and Palæarctic for the Old and New World divisions of this extensive tract." With all due deference, we submit that convenience has nothing whatever to do with the matter; and it is to be regretted that the author has not been bolder, and made a clean sweep of what is obsolete in our present system of zoological geography. He admits that mammals are, on the whole, the most satisfactory group on which to lay the foundations of the scheme; and yet he deliberately throws away Mr. Blanford's very excellent classification, in order to adopt one which obviously does not accord with the facts.

A want of boldness is, indeed, in our opinion, one of the most serious defects in the work, and we should have much liked to hear the author express, without reservation, his real opinions both as regards the so-called Antarctica, and also in respect to Dr. Baur's view that the Galapagos Islands are part of a sunken continent. We gather that, on the whole, Mr. Beddard appears to be indisposed to admit Antarctica in its entirety, but as to how much he believes in a southern land connection of more limited extent, it is almost impossible to discover. In this section of the work, moreover, the author has made two glaringly contradictory statements. Thus whereas on page 116, in treating of the limits of the Australian region, he remarks that "the boundary between it and the Oriental is sharply marked," we find him on page 106 hesitating whether Celebes should not be transferred from the former to the latter region. So much for sharp boundaries.

The third chapter deals with the causes influencing distribution; and here it may be noted that the author differs from Dr. C. H. Merriam,[1] in that he attributes a very minor part to the influence of temperature. Not improbably, however, the difference of opinion is largely due to the different environment of the two workers, the effects of this factor being apparently more noticeable in the New World than in the Old. Very many interesting instances bearing on the problem of dispersal will be found in this chapter. In the fourth chapter, the faunas of islands are discussed; while the fifth closes the work with a few theoretical considerations. In this chapter we find the remarkable suggestion that Marsupials have taken their origin in Australia; a conclusion which, in our opinion, has no shadow of justification from the facts of their past history, and which is absolutely contradicted by the author himself. After stating on page 226, that their "number in Europe may have been small," he speaks of these animals on page 227 as "once existing in great variety in Europe and North America," and later on in the same page that the "survivors have been pushed in to the furthest corner of the world—the Australian continent, and some of the islands to the north." More hopelessly contradictory statements it would be difficult to find. As to the author's conclusions that there has been a general migration of the older forms from north to south, we are in full accord.

It is much to be regretted, especially from the point of view of elementary students, that the work should be disfigured by several glaring inaccuracies which ought to have been corrected in proof. We find, for instance, the genus *Anurosorex* given as exclusively Palæarctic, whereas one of the two known species is from Assam. On the same page, again, the genus *Capra* is likewise given as confined to the Palæarctic region, whereas, on p. 22, the South Indian *C. hylocrius* is included in the same genus. Should Mr. O. Thomas ever read the work, he will be surprised to learn (p. 90) that he has identified the African pouched rats of the genus *Cricetomys* with the American *Hesperomys*. On p. 97 we have "musk-deer" in place of "musk-ox"; while on p. 100 we find the Siberian hippopotamus figuring as *Chœropotamus* (the name of an Eocene genus of pigs) instead of *Chœropsis*. Again, on p. 103, we have the langurs alluded to under the name of *Presbytes*, while on p. 106 they appear as *Semnopithecus*. By what confusion of ideas the name *Hyracodon* (which belongs to an extinct genus of rhinoceros-like animals) is made to do duty for *Didelphys*, we are at a loss to understand. Carelessness is likewise

[1] See *Nat. Geogr. Mag.*, vol. vi. pp. 229-238 (1894).

exhibited by the statement, on p. 111, that *Rhea* is exclusively confined to the Chilian sub-region of South America, especially after the author has stated on p. 20 that *Rhea macrorhyncha* occurs in Pernambuco and Bahia.

As likely to mislead the student, we must also call attention to the so-called genera *Aquias* and *Phyllotis* being placed among those characteristic of the Oriental region, whereas Dr. Dobson,[1] whose views are endorsed by Mr. Blanford, states that there is no justification for the separation of the forms thus named from the ordinary *Rhinolophus*. If the author has reason to doubt the correctness of such generally accepted views, he should have appended a note to that effect. Many other points of this nature might be alluded to; but we cannot help regretting that the author has once more resuscitated the myth of the fossil Australian elephant.

While the book would have been much better had more care been exercised on its composition and correction, it will serve a useful purpose as a general guide to the principles of the geographical distribution of animals, and may accordingly be recommended to the student, provided he have sufficient knowledge to steer clear of the pitfalls.

R. LYDEKKER.

ALKALI MANUFACTURE.

A Theoretical and Practical Treatise on the Manufacture of Sulphuric Acid and Alkali, with the Collateral Branches. By George Lunge, Ph.D., Professor of Technical Chemistry at the Federal Polytechnic School, Zurich. Second edition, vol. ii. Pp. xi. 929. (London: Gurney and Jackson, 1895.)

TO criticise, in the ordinary sense of the term, such a book as this, demands an experience as wide as that of the author—not only in the laboratory investigation and the exposition of the problems of chemical technology, but in the exigencies of daily life in a chemical works. This dual experience is possessed by few, and the present writer can lay no claim to it. But the wide acceptance of the first edition of Dr. Lunge's book as *the* work of reference on alkali manufacture, makes the expression of a judgment on its value superfluous, and the reviewer need do little more than make a general comparison between the present volume and its predecessor of fifteen years ago.

It may at once be said that the book has been thoroughly brought up to date. It is bulkier than the former edition to the extent of over 200 pages, though many processes described in detail in the earlier work, being now obsolete, or nearly so, are here merely referred to; but though some of this increased bulk arises from lengthy detailed accounts of new processes, yet most of it is due to the small additions interpolated on almost every page of the book. No published work on alkali manufacture appears to have escaped Dr. Lunge, whether in journal or patent literature; and he has not only furnished an admirable digest of the progress made in technological thought and practice since 1880, but has throughout given references to original sources.

One change in arrangement commends itself at once: the modes of occurrence and properties of raw materials and products are collected in the first chapter, while analytical methods are similarly gathered together in the second. A striking feature in the first chapter is the amount of space devoted to native soda. Recent explorations have greatly extended our knowledge of the occurrence of this substance, and with sources of supply like Owen's Lake in California, it seems not at all unlikely that in a few years native soda may compete on a large scale with that manufactured by the Leblanc and the ammonia processes. The chapter on analytical methods is very complete, the chief new feature in it being the description and illustration of Lunge and Marchlewski's gas analysis apparatus on p. 113. It seems a pity that those who buy and sell alkali should not by this time have reformed the chaotic condition of "trade customs" which makes it necessary still to devote five pages of a work like this to the question of alkalimetric "degrees."

In the chapter on the salt-cake process the changes consist chiefly in the greater prominence given to plus-pressure furnaces, of which two forms are figured, and to mechanical furnaces. At the date of the first edition, plus-pressure furnaces were in little more than an experimental stage; but the advantages they present have gradually made themselves felt, and their use has become correspondingly more frequent. The early type of the Jones mechanical furnace has been omitted from this edition, and mechanical furnaces are represented by the later form of the Jones furnace, with fixed stirrers and movable bottom, by the Mactear furnace, and by Larkin's mechanical roaster. These furnaces are all fully described and figured, and the discussion of their merits and demerits is eminently fair. The account of the Hargreaves process has been completely rewritten and greatly improved, entirely new drawings of the arrangement of the cylinders having been introduced. That this beautiful process should not have further extended, is matter for regret; but, as Dr. Lunge justly says, it came too late—it has had to succumb to the competition of the ammonia soda process, and the consequent necessary subordination of other considerations to the production, in the Leblanc process, of strong hydrochloric acid.

The condensation of hydrochloric acid had reached such a stage at the date of publication of the first edition, that we find but few changes in this one, and but two noticeable additions: an account and discussion of Dr. Hurter's mathematical treatment of condensation, and a description of the Lunge-Rohrmann plate-columns. The gist of Dr. Hurter's papers is, on the whole, very faithfully reproduced; but there are two errors which are likely to cause confusion to the reader unacquainted with the originals: on p. 308, lines 6 to 10, where the source of the figure 43·3 is not obvious, the fact being that it is quoted from a third example of Hurter's, in which the gas dealt with contains 43·3 per cent. of hydrochloric acid; and on p. 313, where, in converting Dr. Hurter's English measures into metric units, 20 cubic feet per second is taken as 20 feet per second, and the resulting contact figure is worked out to 324 instead of 3474. The Lunge towers are described in the body of the work, and details of their structure, as well as a summary of results obtained in their actual working at Duisburg, are given in the addenda. These figures are certainly remarkable testi-

[1] Cat. Chiroptera Brit. Mus., p. [illegible].

mony to the efficiency of the plate-columns; whether in all respects they will achieve the results their inventor claims for them, it is, perhaps, yet premature to say.

The chapters on the black ash process, on the manufacture of finished soda, and on caustic, are examples of what has been said above as to Dr. Lunge's care and industry; exhibiting no striking changes, they are yet charged with additional matter, of which no satisfactory account can be given, but which will become continually evident to those using the book.

The recovery of sulphur from tank waste is, of course, treated at length. The multitude of attempts to solve this problem, the repeated failures—chemical or economic—of these attempts, the apparent hopelessness of further discovery in so well-explored a field, and the tenacity with which the attack has been continued, form one of the most interesting chapters in the history of manufacturing chemistry, and the account given here is full and accurate. Though the detailed description of Schaffner and Helbig's process has been omitted from this edition, yet the bulk has swelled by some forty pages, an increase due, of course, chiefly to the Chance-Claus process, the account of which, with its modifications and variations, is one of the best written portions of the book. How far this beautiful process affords a satisfactory solution of the problem of sulphur-recovery, may be gleaned from the fact that in 1893 the produce of Chance sulphur in Britain was estimated at 35,000 tons.

An indication of the tendency of chemical manufacture to become more scientific, to be guided by principles rather than by rule-of-thumb, is found in the increased amount of "theory" in the book. Not only have we accounts of investigations into the reactions involved in the various processes, but also accounts of the thermochemistry of the Hargreaves process and the black ash process, and of Dr. Hurter's application of mathematics to technology, mentioned above. No one will dispute Dr. Lunge's statement that manufacturing conditions are complex, and difficult to imitate in laboratory experiments, still more to state in a form definite enough for mathematical expression: no one will question the justness of his warning against proceeding too rashly on lines suggested by theory alone, or indicated by mathematical reasonings on insufficient bases; but the fact that thermochemistry and mathematics find a place at all in such a work as this, shows that our manufactures are being conducted with a closer knowledge than formerly of the principles—chemical, physical, and mechanical—which underlie them, and that we may look forward to a time when we shall have as full control over the conditions of our operations in the manufactory as we now have in the laboratory.

The Leblanc soda process is regarded, by those who are in any way connected with it, with feelings akin to those with which they look on the British Constitution. It inspires a certain affectionate respect, from its combined familiarity and antiquity; and the contemplation of its decay or extinction gives rise to feelings of regret, apart altogether from the pecuniary interests which are involved in it. The statistics given by Dr. Lunge, which show a steady increase in the salt used for the ammonia process, from 27,000 tons in 1880, to 350,000 in 1895, while that used for the Leblanc process has decreased in the same period from 650,000 to 470,000, are not reassuring; but if the older process be doomed to ultimate extinction it will at least have a worthy monument and history in the successive editions of Dr. Lunge's book.

Misprints and slips in such a work are inevitable; there are several, but nearly all such as betray themselves at once, and carry their corrections on their faces. A copious index to the volume adds greatly to its value for reference.

J. T. Dunn.

PHYSICAL ANALOGUES OF PROTOPLASMIC MOVEMENT.

Microscopic Foam and Protoplasm. By Otto Bütschli. Translated by E. A. Minchin. (London: Black, 1894.)

PROF. BÜTSCHLI'S work on Microscopic Foams has been already discussed in these columns; and therefore, in noticing the English translation, a very short account of the book itself will suffice. From his long series of observations, especially upon the structure of the protozoa, the author was led to regard protoplasm as a substance arranged always in the manner of an exceedingly minute honeycomb, containing a second substance in its cells. Taking this view of the structure of protoplasm, and probably stimulated by the experiments upon capillarity and surface-tension made by his colleague Prof. Quincke, he next endeavoured to find a substance having an analogous physical structure, and to produce in it some of the simpler phenomena of protoplasmic movement. The result was the manufacture of the remarkable foams, now so well known in zoological laboratories, in which the walls of the protoplasmic honeycomb are represented by thin laminæ of olive oil, the chambers containing a solution of potassium carbonate and soap. The remarkable resemblance between the histological structure exhibited by drops of this substance, and that of an amœba, is probably familiar by this time to most biologists, as is the resemblance between the streaming movements of the two structures, and the protrusion and retraction of pseudopodia by each.

In the work before us, the final investigations upon oil-foams are first described. The first eighty pages contain a minute description of the manner in which the foams are best prepared, and of their behaviour under the influence of various agencies. Especially interesting is the effect of induction shocks, by which convulsive movements are obtained, and the streaming is frequently slowed down or depressed. It is difficult to avoid comparing the manner in which such a foam-drop flows towards a solution of certain substances, such as soap, with the simpler phenomena of "chemiotaxic" attraction.

After a detailed description of the preparation and behaviour of oil-foams follows a summary of investigations on the structure of protoplasm, as seen in the living condition and after various methods of preparation. This account deals with the structure of various protozoa, and with the cells of many metazoon tissues, especially with nerve-cells and fibres; the object of the whole account being to demonstrate the "alveolar" structure of the protoplasm in all these cases. This account is illustrated by several plates, which have been admirably re-drawn for the English edition of the work, and in addition to these a collection of photographs has been prepared

illustrating the minute structure of oil-drops, and of many of the animal cells described. The evidence of this series of photographs is perhaps even more striking than that of the plates; and it is well here to draw special attention to them, because the only information given to the English reader as to the means of obtaining them is in a note on p. 341, where it may be easily overlooked.

The second part of the book contains a short history of the views which have been held concerning the structure of protoplasm, from the time of Remak's early observations on nerve-fibres until the year 1892; this is followed by a full exposition of the view that all protoplasm has the foamy structure exhibited by the oil-foams already described, and by a discussion of the difficulties which attend the explanation of all protoplasmic movement by reference to changes in the surface tension of a foamy substance.

Such is the arrangement of a work containing the most remarkable attempt to express protoplasmic movement in terms of inorganic phenomena which has yet been made. That the attempt is not yet successful in a number of special cases, Prof. Butschli himself is careful to point out; and the difficulty of explaining in this way the formation of fine thread-like pseudopodia is, as he admits, very great. A more serious difficulty, even in cases of simple lobose motion, is the difficulty of demonstrating those currents in the water outside an amœba in motion, which should, on the diffusion-theory, exist. These and other points are clearly stated by Prof. Butschli, so as to inspire the hope that the final section of his book will lead to the prosecution by himself and his pupils, and by others, of further work on the lines he has here laid down. Without such investigation, any detailed criticism of the difficulties would be simply impertinent.

Mr. Minchin is to be congratulated on his translation. The original German, while always lucid, is often difficult to translate, because the author has throughout been influenced on the one hand by a desire to be as brief as possible, and on the other by a spirit of scientific caution; so that he qualifies statement after statement with epithets which make his sentences easy enough to understand, but hard to render into such English as Mr. Minchin has generally achieved.

By incorporating the appendix of the original edition in the body of the work, a distinct advantage has been gained; and a useful feature, wanting in the German edition, is a very excellent index.

OUR BOOK SHELF.

Æsthetic Principles. By Henry Rutgers Marshall, M.A. (New York and London: Macmillan, 1895.)

MR. MARSHALL has done such good work in the field of æsthetics that we are glad to welcome this short and simplified exposition of the principles which he regards as fundamental. As we said on reviewing his more technical treatise, there is good stuff in his work, and it is based on right lines. We have only space to deal very briefly with one or two points on which we are still constrained to assume a somewhat critical attitude.

Although the view that pleasure is the accompaniment of the using up of surplus stored energy, and that pain arises when the stimulus calls for an overdraught of energy, may well hold good in certain fields of activity, it does not appear to touch some of the pleasures and pains of special sense. That certain groups of sensory stimuli are pleasurable, and others painful, seems just as primary and inexplicable (and therefore to be at present treated merely descriptively) as that certain light-vibrations give rise to the sensation blue, and others to the sensation red. They are primary data of "algedonics," as the colour-sensations are primary data of colour-vision.

In the helpful classification of "Instinct-feelings," so-called, we think more stress is laid on heredity than the facts at present justify. That there is an innate inherited potentiality of fear, for example, is unquestionable; and that it is connected with a tendency to flee from a disadvantageous object, may be admitted. But the disadvantageous nature of the object would seem to be a matter of individual experience, aided by the effects of what Mr. Hudson terms tradition through parents or others. It is at least questionable whether the advantageous or disadvantageous nature of the object is "determined by the experience of untold generations of ancestry."

The third, and last, point on which we would touch is the delimitation of the æsthetic field. That what is judged to be æsthetic appears to be permanently pleasant in revival may be, and in the main is, true enough. But that the relative permanence of the pleasure-field can be regarded as a sufficient æsthetic differentia, we are not prepared to admit. We cannot here discuss the question; we hold, however, that just as the pleasures and pains of sense on the algedonic accompaniments of sense-experience, so are the distinctively æsthetic pleasures and pains the algedonic accompaniments of the perception of relations. Mr. Marshall's criticisms of the intellectualist position (if this view of the purely algedonic accompaniment of activities, which in their cognitive aspect are intellectual, may be included under this head) is insufficient to carry conviction.

We have selected one or two points on which Mr. Marshall's views do not appear to us to be convincing; but it is partly because he is really worth differing from, that we can recommend his work for careful and serious consideration.

An Analysis of Astronomical Motion. By Henry Pratt, M.D. (London: G. Norman and Son, 1895.)

THE present small volume is a contribution to the ever-increasing mass of pseudo-scientific literature, in dealing with which a scientific reviewer must always find a difficulty. His first impulse is to ignore such a book altogether, but there are objections to such a course. To preserve strict silence might, in the first place, lead the author, and those who blindly trust his guidance to claim that his work was of real scientific value, since it had been tacitly accepted by the scientific world, or, at least, that his theory could not be confronted by any fatal *à priori* objections. Further, a book of this kind is liable to lead astray the untrained minds of chance readers, and one's duty to the public requires that some effort should be made to prevent the waste of time and money over an ignorant and worthless book.

Dr. Pratt's object in publishing the book is to give a simpler expression to the views developed in his earlier work, "Principia Nova Astronomica" (see NATURE, May 17, 1894). He may have found that students needed additional explanations, or that another advertisement was necessary to assist the sale of the earlier work. If the course were prompted by the first suggestion, one cannot say that the author has been altogether successful, for his theory remains quite as obscure and unsatisfactory as when first presented. The distinguishing feature of this theory requires our own sun to revolve round an "equatorial" sun, which in turn revolves round a "polar"

sun, which finally has its centre of motion in a "central" sun. "The evidence of the existence of the central, polar, and equatorial suns is found in certain observed phenomena, hitherto attributed to other causes, but which are in reality due to their presence and influence." Besides the simple enumeration of these phenomena, it is in vain to look for any direct proof of this statement. The author's method of removing objections to his theory, one of the principal objects of this book, is, however complicated in detail, extremely simple in principle. It practically consists in calling a motion, or an absence of motion, when it does not fit in and support his theory, *apparent*, and when such motion can be explained, or Dr. Pratt considers is explained, *real*. Such juggling with phenomena resulting from a combination of revolution and rotation, naturally presents no difficulty to a man who cannot see that a body revolving in an orbit, and always presenting the same face to the centre of the orbit, rotates once in the period of revolution. But others, taught in a different and more rigorous school, have great difficulty in apprehending the author's meaning, and fail altogether to appreciate the arguments by which he seeks to support the successive parts of his theoretical system.

Neither does Dr. Pratt understand the arguments, nor, as far as we can see, admit the facts, by which the gravitational theory is supported. In the third chapter, the author, in criticising our current ideas of planetary motion, discloses the awkward fact, that he has not the slightest acquaintance with Kepler's laws. He has not taken the trouble to master the first principles of the system he would overthrow, but seems to think himself qualified by inspiration to offer another. His inspiration, we fear, is due to a disordered and ill-regulated imagination.

LETTERS TO THE EDITOR.

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*]

The Physical Properties of Argon.

THE following measurements may be of interest in connection with the chemical position of argon. The gas was prepared from atmospheric air with the aid of oxygen and alkali only.

Weighings at 0° C. upon a large scale (two litres), and with the apparatus formerly employed for other gases, give as the density of argon ($O_2 = 16$)

$$19{\cdot}940,$$

a number in almost exact agreement with that obtained by Prof. Ramsay, working upon a relatively small scale and with gas derived by magnesium (Rayleigh and Ramsay, *Phil. Trans.*, 1895).

In spite of its greater density, the refraction ($\mu - 1$) of argon is only ·961 of that of air; so that if we take for air under standard conditions $\mu = 1{\cdot}0002923$, then for argon

$$\mu = 1{\cdot}000281.$$

Terling Place, July 20. RAYLEIGH.

The Teaching University for London.

I WAS absent from the country during the University of London Election; but I may be permitted to make a few remarks on Sir John Lubbock's letter in the last number of NATURE.

I am afraid he has hardly weighed the very serious consequences of the action he has taken. They will have to be met as best we may. What I now desire to consider is some of the grounds on which he has attempted to defend it. These themselves afford matter for sufficiently grave reflection.

(i.) Sir John states in his letter to Prof. Rucker: "I am not asking that any privilege which they do not at present possess should be conferred on my constituents, but only supporting what is now their legal right. . . . This right I know they highly value." This is a most extraordinary statement. What Convocation undoubtedly possesses is the right of veto on any fundamental change in the constitution of the University. It has been exercised in the past to some effect when Convocation summarily rejected the recommendations of the first of the recent Commissions. It might have been exercised when Convocation assented to the admission of women to the University. But it has never hitherto been exercised except by the personal vote of members attending Convocation who have had the opportunity of hearing in adequate debate the arguments for and against the proposal at issue. What Sir John proposes now is something widely different: a *referendum*, in fact, in which the decision of Convocation is to be signified "as at a Senatorial election," *i.e.* by voting papers. In my judgment such a precedent, if once established, would utterly destroy the prestige and authority of the meetings of Convocation as at present constituted. To this point I will return presently. But at any rate I think it will be admitted by all who know anything of the practical working of this body that Sir John's proposal is a pretty revolutionary change. How then are we to reconcile it with his language which I have quoted above.

(ii.) But Sir John's action becomes still more extraordinary in the light of the actual recent proceedings of Convocation itself. To read his letter it might be thought that we were smarting under a sense of injury and injustice, and that Sir John, as in duty bound, had come chivalrously to the rescue of our oppressed body. Far from this being the fact, I think, that in plain language Sir John has given Convocation the severest slap in the face it has ever received.

After the report of the first Commission was dead and buried, the second came up in due course for consideration by Convocation, and for the past two years its mind has been occupied with little else. The report might have succumbed to the veto like its predecessor, but it did not. I need not recapitulate all that has happened. It is enough to say that though Convocation approached the conclusions of the Commission with a certain timidity or, at any rate, reserve, their substantial acceptance after each successive debate steadily gained ground.

Finally at the meeting on January 22 of the present year the following resolution was carried:—

"That Convocation, while desiring to express generally its approval of the proposals contained in the Report of the Royal Commission, is of opinion that power ought to be given to the Statutory Commission to vary the details of the scheme, and that it ought to be made an instruction to the Commissioners, before framing the statutes and regulations, to confer with duly accredited representatives of the Senate and of Convocation, as to the modifications which may be desirable."

Now whatever be the opinion of different sections of Convocation on the general merits of the question, I think that we are all agreed as to the latter part of the resolution. Convocation regards the Report as a possible basis for reconstruction, but declines to pledge itself to all the details. But it is most important to observe, and it was most clearly pointed out in the debate, that in adopting this resolution Convocation *waived its right of veto*. In other words it dropped its possible *non possumus* and looked to negotiation to attain what it wanted.

This resolution was followed by a further one, which was its necessary executive corollary. I may be permitted to extract the whole from the minutes, as it is significant to observe that it was moved and seconded by a representative of either side.

"On the motion of S. P. Thompson, D.Sc., B.A., seconded by T. B. Napier, LL.D. Resolved:—

"(1) That a Special Committee of nine members, including the Chairman of Convocation, be nominated to prepare for presentation to the Statutory Commission, when appointed, a memorandum of points in the Scheme of the Royal Commission in which modification is desirable, and with power to confer with such said Statutory Commission, and with any Committee of the Senate.

"(2) That this Special Committee consist of the following Members: The Chairman of Convocation, Dr. Allchin, Mr. Bompas, Mr. Stanley Boyd, Dr. Cave, Mr. Cozens-Hardy, Mr. Thiselton-Dyer, Dr. Napier, Dr. S. P. Thompson."

Now I put it to Sir John, who, though I am glad to say not "an old," is certainly an experienced "parliamentary hand," whether the action he has taken is exactly courteous to Convocation in general or to its formally constituted Committee in particular.

What Sir John practically says to us is this: "You may do as you like, but I am taking the management of this business into my own hands." Now, we are undoubtedly proud of having a representative in Parliament; but I am very doubtful whether Convocation is prepared to accept that representative as its master.

The resolution of January 22, as it happened, owing to the prolongation of the debate, was not carried by a large majority. The question was therefore brought up again on May 14, and reaffirmed by more than two to one.

The present position then is this: Convocation has accepted the Report of the Commission in principle; awaits the appointment of the Statutory Commission; and has delegated to a Committee of men representative of various views the duty of conferring with it. This Committee, which has already held a preliminary meeting, can be in touch at any time with Convocation, and it is difficult to see what better machinery Convocation could provide to bring about the result which all reasonable men desire. And all this, Sir John, who is not a member of Convocation, and who has not apparently taken the trouble to acquaint himself with its proceedings, calmly sets aside for a new-fangled and unheard-of plan of his own.

(iii.) Sir John, in what I suppose I may call his defence, says "the University is the only body whose constitution it is proposed to change." I do not know, I am sure, how he arrives at this. But we, who have had to consider the point, have been advised very differently. It has been pointed out to us by very high legal authority, that some at least of the bodies which it is desirable to bring into closer co-operation with the University may be impeded by disabling enactments. And one of the strongest arguments brought before us in favour of a Statutory Commission was the fact that it is a legislative solvent, and could, subject of course to the approval of Parliament, remove any legislative impediment which stood in the way of its ordinances.

(iv.) What I have stated above is sufficient, I hope, to show that Sir John's interference really amounts to a grave invasion of the privileges of Convocation, and I am utterly at a loss to see by what considerations it can be justified. The principle of a *referendum* which it is proposed to force upon us, is one which can only be accepted after the most serious examination.

Let us consider what it involves. At present, on any question of moment, Convocation only proceeds to a decision after a prolonged debate. And I venture to say that in ability, and certainly in earnestness, the debates in Burlington Gardens will compare not unfavourably with those at Westminster. The divisions, it may be inferred, are the outcome of reasoned conviction. A *referendum* is a very different matter. It is only theoretically applicable when the issue is of the sharpest, and can be stated on the most explicit terms. For anything short of this it would be necessary to organise for and against any proposal a costly machinery in order to put before each voter a reasoned statement on one side or the other. But the Statutory Commission, from the nature of things, will have to deal with matters of the most delicate compromise, affecting, as I have shown, other institutions besides the University. To subject these to the accidents of a *referendum*, is, I venture to say, one of the maddest political expedients ever proposed.

I cannot refrain from adding one more remark. I deeply regret that Sir John, in addressing the President and other Fellows of the Royal Society, thought it worth while to point out to them that some of them were not his constituents. There are many students of practical politics who find it difficult to justify the existence of University Members at all. I take it that the only defence that can be made for them is that they are something more than the mandatories of merely local interests, such as may exist, say, in a borough. They stand in Parliament, if they have any claim to be there at all, as the representatives of those interests remote from party which ennoble and dignify the life of a nation. Universities may select and return such Members. But that duty performed, theirs begins. If Sir John really seriously thinks that it is inappropriate that a body of Fellows of the Royal Society should address the Member for the University of London on a matter of supreme public interest, then I can only say with the deepest regret that I hope that the day is not distant when our choice may fall on a man of larger sympathies with the interests of the higher education and learning.

W. T. Thiselton-Dyer.

Kew, July 20.

P.S.—I think it important to add from the Bill a portion of Clause 3.—"(1) The Commissioners shall make statutes and ordinances for the University of London in general accordance with the scheme of the report hereinbefore referred to, but subject to any modifications which may appear to them expedient after considering any representations made to them by the Senate or Convocation of the University of London, or by any other body or persons affected." It will be seen (i.) that it practically accepts the procedure of Convocation and (ii.) gives a *locus standi* to other bodies beside the University which may be affected.—W. T. T. D.

Sir John Lubbock seems to have a mistaken conception of the nature of the right of veto possessed by the Convocation of the University of London. The Charter of that University provides that Convocation shall have "the power of accepting any new or supplemental Charter for the University or consenting to the surrender of this our Charter." But such provisions cannot limit the action of Parliament. The provision is similar to the reference to Convocation at both Oxford and Cambridge of new statutes and of all alterations in old statutes proposed by the Council of the University. Our statutes take the place of the Charter of the University of London in many respects.

When Parliament has overhauled the Universities of Oxford and Cambridge by means of a Royal Commission, it has never occurred to any one that it would be proper to refer the statutes proposed by such Commission to the Convocation of Oxford or Cambridge. Sir John Lubbock's proposal to do what is parallel to this in the case of the University of London is a new departure. Whether he is aware of the customary procedure in dealing with universities, and thinks it objectionable, or whether he supposes that the plan he suggests is according to precedent, or, again, whether he is merely anxious to claim for his constituents an exceptional privilege by demanding which he will be giving effect to their wishes and justifying their selection of him as Parliamentary representative, does not appear.

For my own part, though not a graduate of the University of London, I have been most closely associated with its work and organisation—as professor in University College and as examiner in the University during twenty years. My conviction is that there is a large body of graduates, members of Convocation, who do not at all approve of Sir John's too flattering claim on their behalf; they do not desire that the Convocation of London should be given exceptional powers possessed by no other body of University graduates in this or any country. They are deeply concerned for the progress and development of the University of London in its true character of the University in the greatest city in the greatest empire of the world. And they are prepared to forego the gratification of personal vanity offered by Sir John Lubbock, in order that an executive Commission may carry out without delay the important development of the University proposed by the Gresham Commission. These proposals have been already approved of by a majority of voters in meetings of Convocation at which they were considered and discussed; the plan of again submitting the matter to Convocation after a Statutory Commission has embodied the Gresham Commissioners' proposals in detailed enactments, is one which can have no other object than that of defeating or, at any rate, delaying the whole scheme.

Sir John Lubbock has adopted, and made himself the leader of this extraordinary and fantastic policy. It seems to me that he has by his action shown an unfavourable estimate of the intelligence of his constituents, and that the time may come when the Convocation of the University of London will require from its representative active co-operation in the task of organising the University, and single-minded devotion to the interests of science, learning, and education, together with attention to those interests in Parliament, in place of the empty flattery of an impossible proposal to confer on Convocation powers rendering the customary Parliamentary control of the University impossible.

E. Ray Lankester.

July 20.

Without entering into the vexed question of the Gresham scheme, will you allow me to explain, in a few words, the grounds on which so many of Sir John Lubbock's old friends and supporters join issue with him entirely on the attitude he has taken up in his letter to Dr. Foster.

We object to the proposed *referendum* to the graduates, and to the mode in which he suggests that it should be exercised.

First, as to the mode. If Sir John Lubbock insists on the maintenance of the right of veto according to the Charter, this should clearly be exercised in the only method provided by the

Charter, that is, by Convocation assembled in a regular way. The constituency may be, as Sir John states, an exceptionally educated and intelligent one; but a very large proportion of the graduates have never studied the question of reorganisation, and are ignorant of its complications and difficulties. We have already had painful experience of how the votes of these graduates may be influenced by inaccurate or misleading statements in circulars issued through the post on the eve of an election by the party who are hostile to the Gresham scheme. If made in debate in Convocation, these statements could at once be corrected.

But, secondly, we object to the *referendum* in itself. Convocation has already, twice, deliberately, knowing what it was about, waived the right of final veto by agreeing to the appointment of a *Statutory* Commission. It maintains its full right of presenting its views to this Commission, when appointed, and of protesting against any provision that may interfere with its rights and privileges; and, furthermore, of influencing Parliament against it through its Member, or through any graduate who may have a seat in the House of Commons, or through its Chancellor, who sits in the House of Lords, should any such provision still be retained when the Bill is presented to Parliament. Any further right than this Convocation does not claim.

For my own part, should the position assumed by Sir John Lubbock be maintained by Parliament, it seems to me that we must abandon all hope of bringing our University into a line with the requirements of the age. ALFRED W. BENNETT.

The Earliest Magnetic Meridians.

IN reply to Prof. L. A. Bauer's letter in NATURE of July 18, p. 269, I may remark that I possess two of Churchman's Magnetic Atlases. The first of these I now believe was published in 1790, and to be that described in his tract, "An Explanation of the Magnetic Atlas, Philadelphia, 1790." The lines on this chart are magnetic meridians only, as fully defined in Churchman's text, and largely based upon Cook's observations of the variation.

It is evident that Churchman depended largely on observation, as he discussed the question of the effects of a ship's iron in altering the value of the variation when observed on board ship.

The second atlas, which is dated July 1, 1800, has isogonic lines for each degree of variation with magnetic meridians superposed, similar to Yeates' Chart of 1819, which I also possess.

Lastly, I would observe, that Yeates mentions the charts of Halley, Bellin, and Mountaine, and Dodson in 1794, but makes no reference to Churchman, who presented a copy of his work to the Royal Society in January 1791. It is possible, therefore, that Yeates constructed his chart in ignorance of Churchman's work, but the latter certainly was the first of the two to construct magnetic meridians. ETTRICK W. CREAK.

London, July 20.

Variegation in Flowers and Fruits.

REFERRING to a letter by Mr. Newnham Browne, in NATURE of July 11, describing a parti-coloured rose, it may be of interest to state that a somewhat similar occurrence in the case of an apple is recorded by Mr. Darwin in his "Animals and Plants under Domestication" (vol. i. pp. 392-3). The reference is to a specimen which I brought from Canada, and of which I sent him a careful drawing. In this specimen it appeared as if a smooth-skinned bright green apple had been cut in half and joined to a rough brown *pomme-gris*. The line of junction was perfectly sharp, but not quite symmetrical, the brown portion extending over the whole of the bud, while the green just included the stalk. I was told that similar instances sometimes were found on the tree from which it was gathered.

J. D. LA TOUCHE.

Stokesay Vicarage, Craven Arms, July 12.

Science Scholarships at Cambridge.

THOUGH the arrangements for the competitions for Science Scholarships at Cambridge, as described in NATURE of July 18, are in many respects eminently satisfactory, yet from the point of view of the candidates they leave something to be desired.

In the first place, they are unduly favourable to those whose nineteenth birthdays will fall early in 1896, and correspondingly unfavourable to those who are six or eight months younger. They will compel these younger candidates not only to compete at a marked disadvantage in the matter of age, but also after a shorter period of reading in science; unless, indeed, they have sacrificed an important part of their general education by commencing specialised study at an undesirably early age. Secondly, they are calculated to throw out altogether any candidates who may, through illness or other causes, be unable to compete during the very limited period covered by the examinations as at present arranged.

Similar difficulties are avoided in the case of the Army examinations by holding them twice yearly, at intervals of about six months. In the present case, sufficient equality could be secured by a fairly strong group of colleges holding their examinations a little later—for example, in April or May.

If it be feared that only the inferior candidates would be left to compete at this later examination, we would point out that, on the contrary, there would be less chance of this happening if our suggestion be adopted than under the present scheme. In April or May the older of the previously unsuccessful candidates would be excluded, and only the younger and, presumably, better candidates would remain. On the other hand, the later examination would have attractions for the ablest of those still younger candidates, who will not, under the present system, come into the field until the autumn of 1896. W. A. SHENSTONE.

Clifton College, July 23. D. RINTOUL.

SIR JOHN LUBBOCK AND THE TEACHING UNIVERSITY FOR LONDON.

THE feelings of "surprise and regret" which we said had been aroused by Sir John Lubbock's election address, will not be diminished by the perusal of the reply to which, at his request, we gave publicity in our last issue. Rather the surprise will turn to amazement, that he should deem that to be a reply which evades every material issue, and appears to be written in ignorance or forgetfulness of all that has taken place. And the regret will be enhanced when it is observed that his language now makes plain what could only be inferred from his address, namely, that he has never grasped the distinction between a Charter granted by the prerogative of the Crown, and a scheme framed under the authority of the Legislature.

Yet Sir John Lubbock has for many years taken an active, and even a prominent, part in public affairs; has for many years occupied a seat in Parliament; has in the course of his lifetime seen almost every university in the three kingdoms reformed by the machinery of Statutory Commissions; and has, if we are not mistaken, himself sat on a Commission entrusted by the Legislature with the duty of remodelling the constitution of the great public schools, which, next to the universities, are the most important educational institutions of the country. That he should be unaware of the distinction, or have forgotten it, seems incredible; but his language and his reasoning seem to leave no doubt on the point. "I am glad," he says, "to observe that the only point objected to is the reference of any new *Charter* to Convocation. In this, however, I am not asking that any privilege which they do not at present possess should be conferred on my constituents, but only supporting what is now their legal right."

What then, we are forced to ask, is Sir John's idea of a Statutory Commission? Does it need an Act of Parliament to authorise a body of persons to formulate proposals affecting a public corporation or institution, which, when framed, may be accepted or rejected at the pleasure of those whom they affect? Or does he suppose that it needs an Act of Parliament to enable the Crown to concur with bodies which the Charter of the Crown has called into existence, in effecting a modification of the franchise which they enjoy? An Act of Parliament, we had thought, was an Act of the Sovereign Legislature, which changed the "legal rights" as they previously existed; and we had never heard that Parliament added to its necessary labours the superfluous

task of passing Statutes to enable people to do what they had already the "legal right" of doing.

If this is Sir John Lubbock's view of a Statutory Commission, it was not the view taken by the late Royal Commission, to whose Report he indeed refers, but whose Report, we are compelled to believe, he has never read. For, in words too clear for misunderstanding, they have expressly recommended that the proposed change should be effected, "*not by Charter*, but by legislative authority."[1] Is it possible to suppose that in the discussions which have taken place in the Senate on the subject of the Report, the distinction so clearly pointed out has never been noticed or commented on in that august assembly, though presided over by the highest legal authority in the realm? Or if (as we must needs assume) the distinction did not pass unnoticed, was Sir John slumbering in his chair; and when he concurred in voting the resolution, by which the Senate accepted generally the recommendations of the Commissioners, including this vital one, was he not aware of the meaning of his act? Every assumption we make seems incredible; yet it looks as if, notwithstanding, some or one of them must be true.

The authors of the protest addressed to Sir John Lubbock say truly that it would be "without precedent" to confer on Convocation the right to "supervise the Acts of a Commission entrusted with the reorganisation of the University of which Convocation itself is a part." But when we ask ourselves how this right is to be exercised, the matter becomes not only unprecedented, but almost inconceivable. Is the ratification or veto of Convocation to be exercised directly on the Acts of the Statutory Commission, so as to be interposed between such Acts and the "approval of Parliament in the usual way," and so as to exclude Parliament from the power of considering any proposals of its own Commissioners not so ratified? Or is it to stand in lieu of the ratification of Parliament, so as to transfer the power of Parliament to the individual graduates? Or is the ratification of Parliament to be given only subject to the power of the graduates to disallow the Act of the Legislature? Or is, perhaps, the ratification and veto to be exercised by the more compendious method of entrusting the Member for the University with a power to overrule the decision of Parliament and its Commissioners? We shall look with interest at the particular form given to the clause which Sir John Lubbock proposes to introduce into the Bill.

But yet, for one so careful of the "legal right," one or two strange things are to be observed as to his proposal. Convocation, as we all know, has already, like the Senate, accepted the recommendations of the Commission, and, like the Senate, claims to represent its views before the Statutory Commission, when appointed. Convocation has passed this resolution in the exercise of its "legal right," and in the legal mode, that is, in the mode prescribed by the Charter on which alone its rights depend.[2]

[1] "In view of the failure of previous attempts to settle this question, and of the [illegible] and delay which must inevitably attend an alteration of the constitution of the University through the action of the University itself, we are of opinion that in accordance with the precedents followed in other cases of University reform, the change we recommend should be effected not by [illegible], but by legislative authority, and by the appointment of a Commission with statutory powers to settle, in the first instance, arrangements and [illegible] in general conformity with the recommendations which we are [illegible] to Your Majesty." (Report, p. [illegible].)

[2] The twenty-first clause of the Charter provides "That the Convocation of the University shall have the power following (that is to say):—The power [illegible] three persons for every Fellow to be appointed in the manner [illegible] [illegible] the power of [illegible] any matter [illegible] the opinion of Convocation [illegible] the power of accepting any new or supplemental Charter [illegible] ... (The italics are ours.)

Is it not a little strange, then, that this new power of ratification or veto, which is not an "existing legal right" at all, is to be exercised, not in the manner in which the acceptance of a new Charter is by the express language of the existing Charter to be exercised, but in a mode in which that very right, on the analogy of which the claim is based, cannot be exercised. But truly the argument is all of a piece; and the result is, that the individual graduate is to have a larger, and a more irresponsible, power in controlling the Acts of the Legislature, than he has in controlling the Acts of the Crown alone, acting on the instance of the Senate.

For, and this is the other strange thing, what in the view of this champion of "legal rights" is to become of the legal rights of the Senate? The Senate is the sole administrative governing body of the University. It is the Senate which must necessarily have the most intimate knowledge of the working of the system which it administers, and of the needs of the University for the conduct and reputation of which it is responsible. It is the Senate which would alone apply to the Crown for that new Charter which Convocation has the power of accepting or rejecting, and without whose application no such Charter would ever come under discussion. Surely it would be logical, or at least consistent in its illogicality, to require that the acts of the Statutory Commission should also be submitted to the approval of the Senate, and (let it be added) that the individual members of the Senate should record their opinion by means of voting papers. Or is it indeed only the "legal rights" of "constituents" that are to be, not indeed preserved, but extended by the creation of a new and exorbitant precedent?

POST-GRADUATE STUDY AND RESEARCH AT CAMBRIDGE.

THE Senate of the University of Cambridge have now approved new statutes for submission to Her Majesty in Council, conferring on the University the power of admitting to the degree of Bachelor of Arts, or Bachelor of Law, "advanced students" who have resided six terms, and have fulfilled certain requirements to be prescribed by ordinance from time to time.

The regulations which will become ordinances when the statutes are confirmed have been published, and run as follows. A few notes are added in square brackets by way of explanation.

Regulations for Courses of Advanced Study and Research.

(A) *Admission as Advanced Students of Persons who are not already Members of the University.*

(1) Applications for admission as advanced students shall be made to the Registrary.

No person shall be admitted as an advanced student who has not attained the age of twenty-one years.

(2) Each application shall be accompanied by

(i.) a diploma or other certificate of graduation at a University [British or foreign]:

(ii.) a statement as to the course or courses of (*a*) advanced study or (*b*) research which the applicant desires to pursue, together with such evidence of qualification, attainments, and previous study as he may be able to submit:

(iii.) a certificate or declaration that the applicant has attained the age of twenty-one years.

(3) In exceptional cases persons who do not present a diploma or certificate of graduation [at another University] may be admitted as advanced students, provided they give such evidence of special qualification as may be approved by the Degree Committee of the Special Board of Studies with which the proposed course of advanced study or research is most nearly connected.

(4) Applications shall, in general, be submitted not later than the first day of October in the academic year in which the applicant proposes to begin his course. But the authorities

specified in Regulation 5 shall have power to consider applications submitted at other times.

(5) The Registrary shall forthwith communicate each application to the Chairman of the Special Board of Studies with which the proposed course of advanced study or research appears to be most nearly connected.

Applications for admission to courses of advanced study shall be considered and decided upon by the Chairman of the Special Board.

Applications for admission to courses of research, and exceptional applications under Regulation 3, shall be considered and decided upon by the Degree Committee of the Special Board.

(6) The application shall not be granted unless it shall appear

(i.) that the course or courses of advanced study or research can conveniently be pursued within the University; and

(ii.) that the applicant has produced adequate evidence that he is qualified to enter upon the proposed course or courses.

(7) When the application has been decided, the Chairman shall inform the Registrary of the decision; and the Registrary shall inform the applicant.

(8) Before a person is admitted as an advanced student, he shall become a member of a College or Hostel, or a non-collegiate student [for this admission he must present satisfactory testimonials of character and attainments]. He shall not be allowed to count any term before that in which he has matriculated [by signing the matriculation book of the University, and paying a fee of £5: there is no "matriculation examination"], unless he has satisfied the Council of the Senate that his matriculation had been deferred for grave and sufficient cause.

(B) *Courses of Advanced Study.*

(9) An advanced student, who has kept two terms by residence, may in his third term of residence or in any subsequent term become a candidate for any of such Tripos examinations or parts of Tripos examinations as shall have been opened to advanced students under the provisions hereinafter contained.

The name of every such candidate shall be sent to the Registrary by the Prælector of his College or Hostel, or by the Censor of non-collegiate students, as the case may be, at the same time and in the same manner as the names of other candidates; but a mark shall be added to his name showing that he is an advanced student.

(10) It shall be the duty of each Special Board of Studies from time to time to consider whether the Tripos examination or a part only of the Tripos examination with which that Board is connected shall be open to advanced students, and also what standard in the examination must be attained by an advanced student in order that his name may be included in the list mentioned in the next Regulation: and their recommendation after approval by the General Board of Studies shall be submitted for adoption by Grace of the Senate.

In cases where two or more Special Boards are connected with a Tripos examination, the duty prescribed by this Regulation shall be performed by such Boards in joint meeting assembled.

(11) The names of such advanced students as satisfy the Examiners that they have attained the required standard in the examination shall be placed in alphabetical order on a list, written or printed, signed by all the Examiners and distinct from the Tripos list, which shall be regarded as the authoritative list and shall be preserved in the Registry. The Chairman of the Examiners shall send both to the Vice-Chancellor and to the Registrary a printed copy certified by him to be a correct copy of the authoritative list.

(12) An advanced student who has satisfied the Examiners as prescribed in Regulation 11 shall be qualified to enter upon a course of research, provided that the subject of his research be approved by the Degree Committee of one of the Special Boards.

(13) An advanced student who has satisfied the Examiners as prescribed in Regulation 11 and has kept by residence at least six terms shall be entitled to proceed to the degree of B.A. and thereafter under the usual conditions to the degree of M.A. and to other degrees in the University [*i.e.* for example, M.D., Sc.D., or Litt.D.].

(14) An advanced student who has satisfied the Examiners in the Law Tripos as prescribed in Regulation 11 and has kept by residence at least six terms, shall also be entitled to proceed to the degree of LL.B. and thereafter under the usual conditions to the degree of LL.M. and to other degrees in the University [for example, LL.D.].

(C) *Courses of Research.*

(15) An advanced student who has been admitted to a course of research shall pursue that course under such direction and supervision and under such other conditions as may be prescribed by the Degree Committee.

(16) An advanced student, who has kept two terms by residence, may in his third term of residence, or in any subsequent term, submit to the Degree Committee, not later than the division of the term, a dissertation containing an account of and embodying the results of his research. The dissertation shall be referred to one or more persons appointed by the Committee, who shall have power to examine the student orally or otherwise upon the subject thereof, and shall report thereon to the Committee. Each of the persons so appointed shall receive a fee of two guineas from the University Chest.

The Committee shall have power to take into consideration together with the dissertation any memoir or work [previously or subsequently] published by the student which he may desire to submit to them.

(17) If the Degree Committee be of opinion that the work submitted by the student is of distinction as an original contribution to learning or as a record of original research, they shall draw up a statement to this effect, indicating therein the subject or subjects of the student's research.

(18) The statement drawn up by the Degree Committee shall be forwarded by the Chairman to the Registrary, who shall embody it in a Certificate of Research in a form approved by the General Board of Studies. No such Certificate shall be granted unless and until three terms have been kept by residence.

Each candidate before receiving his Certificate of Research shall deposit in the University library a copy of his dissertation in a form approved by the Degree Committee.

(19) A student who has obtained a Certificate of Research and has kept by residence at least six terms shall be entitled to proceed to the degree of B.A. and thereafter, under the usual conditions to the degree of M.A. and to other degrees in the University [see Regulation 13, above].

(D) *Admission to Courses of Research of Persons who are already Graduates of the University.*

(20) A graduate of the University who desires to be admitted as an advanced student with a view to obtaining the Certificate of Research described in Regulation 18, shall make application to the Chairman of the Special Board of Studies with which his proposed course of research appears to be most nearly connected; and the application shall be considered and decided upon by the Degree Committee of the Special Board.

(21) The Degree Committee shall not grant the application unless they are satisfied

(i.) that the course or courses of research can conveniently be pursued within the University; and

(ii.) that the applicant has produced adequate evidence that he is qualified to enter upon the proposed course or courses.

(22) If the application be granted, the student shall become entitled to a Certificate of Research upon satisfying the requirements of Regulations 15–18.

(E) *Table of Fees for Matriculation, Examinations, and Degrees.*

	£	s.	d.
MATRICULATION.			
Advanced student (at any time, whether fellow-commoner or not)	5	0	0
[Certain Colleges, *e.g.* St. John's, Trinity, and King's, have recently admitted senior students, generally graduates of other Universities, as "fellow-commoners." These dine with the fellows, and have certain special privileges. Fellow-commoners not admitted as "advanced students" pay to the University a matriculation fee of ten guineas.]			
EXAMINATIONS.			
Advanced Students:			
On admission to a Tripos examination or a part of a Tripos examination ...	3	0	0
On submitting a dissertation for the Certificate of Research, on each occasion [*i.e.* the fee has to be paid again if the candidate is unsuccessful the first time]	5	0	0
DEGREES.			
Advanced Students:			
B.A. or LL.B. at any congregation for degrees	7	0	0

[The fee for these degrees, except at "general admissions," is ten guineas for students not admitted as "advanced students."]

Advanced students shall pay to the University Chest the same capitation tax as other members of the University, and under the same conditions as to standing (Graces June 1, 1893, and February 14, 1895); provided that the quarterly payment to be made by an advanced student, who has obtained a certificate of research but has not been admitted to a degree, and who has ceased to reside in the University, shall from and after the end of the eighth quarter from the commencement of residence be four shillings and threepence.

[The "capitation tax" referred to is thus in general ten shillings a quarter during the two years of residence, and four shillings and threepence a quarter thereafter until the advanced student removes his name from the boards of his College.]

The outcome of these regulations is this, that a graduate of a British, American, or other University, who can show evidence of special qualifications for advanced study in literature, law, history, or other like subject, or for scientific research, may be admitted under exceptionally favourable conditions to the University of Cambridge. He will not be required to pass the "previous examination" in Greek, Latin, elementary mathematics, and other subjects of preliminary education. He may reside two years instead of the three required of ordinary undergraduates. He will probably be allowed special privileges in respect of the University library, the museums, and the laboratories. He may become a candidate in the parts of certain of the Triposes concerned with his particular subject, or he may engage from the outset in independent research. If he approves himself sufficiently in the Tripos examination, or achieves results in relation to his research which may fairly claim "distinction," he may proceed to the degree of B.A. without further examination. Thereafter he need not reside further, but after the ordinary period of probation, pass to the higher degree of M.A. This opens the way to the doctorate in science or in letters for those whose after-work is of sufficient merit. A point of importance is contained in the second clause of Regulation 16, which provides that work published elsewhere may be taken into account in deciding whether an advanced student is qualified for his certificate or degree.

The "Degree Committee" of a Special Board consists of the professors and other elected members of the Board, but not the examiners for Triposes, &c., who are appointed for a year at a time. The special Boards deal respectively with theology, law, medicine, classics, oriental studies, mediaeval and modern languages, mathematics, physics and chemistry, biology and geology, history and archaeology, moral science, and music. The Triposes are the mathematical, classical, moral sciences, natural sciences, mechanical sciences, theological, law, historical, oriental languages, and mediaeval and modern languages. It has yet to be determined what parts of these shall be specially opened to advanced students, but as most of them are divided into two parts, it is likely that the second or more advanced and specialised parts will as a rule be made available. The University has made concessions as to the fees to be paid by advanced students, and there is no doubt that as the scheme comes into working order, the colleges will follow the lead of the University in this respect.

The scheme is one which should lead to important developments in the future. Graduates of other universities, whether they came from Oxford or Dublin, or were specially "attested," could share in the advantages which Cambridge has to offer, only on condition of becoming once more undergraduate students, and so beginning their academic course over again. Now, if they are sufficiently qualified by previous study and attainments, they are admissible on a higher and definitely recognised footing, and may at once enter on post-graduate work. It is to be hoped that, at least in English-speaking countries, the opportunities thus offered for higher study in Cambridge may soon be appreciated; and that a steadily increasing number of those who now from our colonies and the United States proceed to continental universities in pursuit of learning may find in one of the old English universities a more natural and a more interesting academic resort.

THE HEALTH OF LONDON.

THE immense strides which have been made in sanitary science, the well-nigh feverish eagerness with which all questions relating to health are pursued, causes the layman to turn with interest and, indeed, curiosity to any reliable record he can obtain of statistics relating to the public health.

"What," he asks, "is the actual practical result of all these efforts on the part of municipal authorities and other responsible public bodies on the health of our great cities?"

It is thus that statistics become invested with an interest even to the uninitiated, and there is no more striking tendency in the hygienic crusade which prevails than the sense of individual responsibility which it has succeeded in arousing in the conduct of sanitary matters, and the participation of the people themselves in measures of sanitary reform. Hence the compilation and issue by the London County Council of periodic reports on a variety of hygienic subjects; and the appearance of "County Council Orange Books" may now be regarded as a familiar feature in the administration of that democratic body.

One of the most recent of these is the annual report of the London County Council's Medical Officer of Health for the year 1893.

This weighty document bristles with figures, and embraces a variety of subjects, but to only a few of the more important of these can we briefly refer here.

Perhaps the most appropriate point to start from, is the consideration of some interesting data dealing with the *expectation of life*, actuarily calculated, enjoyed by Londoners from five years upwards in the period of 1881–90 and 1861–70 respectively.

These statistics go to show that the expectation of life of males at five years of age has improved from 47·49 years to 50·77; or, in other words, during the last period there has been a gain of 3·28 years. As regards females, we find the expectation of life has risen from 50·87 to 54·43, or a gain of 3·55 years. At subsequent ages there is also, in all cases, an improvement, though relatively less than at age five, showing that the greater part of the gain is in the periods of youth and early maturity.

If we compare these tables with those of a similar nature, which have been compiled for each sex in Manchester and Glasgow from 1881–90, we find that the expectation of life in London exceeds that enjoyed by the inhabitants of both these large cities.

Londoners may also congratulate themselves upon the fact that the death-rate in London was lower than that of the majority of the capitals of Europe and of New York; thus, we can contrast a death-rate of 21·3 per 1000, with 21·8 in Paris, 22·3 in Rome, 24·0 in Vienna, and 30·6 in St. Petersburg, and in New York 23·9 per 1000.

As compared with our five largest cities—Manchester, Liverpool, Birmingham, Leeds, and Sheffield—London again can boast of the lowest death-rate; whilst our infant mortality, compared with that of other English towns having more than 200,000 inhabitants, was also lower in every case with the single exception of Bristol.

If we look more closely into the particulars of the death rate, we find that, as regards the principal zymotic

diseases, London shows an increased mortality over the average for the preceding ten years, the rate having risen from 2·10 to 2·28 per 1000; and although this zymotic death-rate compares favourably with that of the largest of our towns, yet as regards foreign capitals it is only exceeded in two cases, *i.e.* by that of Stockholm and Vienna.

This increase is largely due to the alarming rise which has taken place in deaths from diphtheria, a rise represented by a death-rate of 0·12 per 1000 in the years 1871–80, 0·26 in 1881–90, 0·31 in 1891, 0·44 in 1892, and, lastly, 0·74 in 1893. Such a diphtheria death-rate is markedly in excess of that of other large English towns having a population of more than 200,000, being, in fact, more than double that of any with the exception of West Ham (virtually a part of London); it was even ten times as great as the diphtheria death-rate of Nottingham, and six times as great as that of Liverpool.

Small-pox also appears to be on the increase, and influenza and pneumonia claimed a number of victims greatly in excess of the average of the preceding ten years; and there is, also, a substantial increase registered in the scarlet-fever death-rate.

But the most serious problem which we have to face is our diphtheria epidemic; various attempts have been made to ascertain to what it can be traced, but so far, it must be confessed, we are without any satisfactory clue as to its source. It has been attributed by some to alterations in the classification of diseases, more especially by transference to diphtheria of deaths which in former years were registered as croup, by others to increased facilities for the spread of infection afforded by increased school attendance, to sewer ventilators, &c.; but the fatal objection to all these explanations is that they are circumstances which are shared by all the other great cities and towns of the country, and yet London alone is pre-eminent in its death-rate from diphtheria.

There appears, however, to be a very decided tendency in England for diphtheria to increase in densely inhabited centres, whilst in the more sparsely populated districts there is a decrease, which has been especially emphasised of late years.

Curiously, this is not the experience of our neighbours in Germany. Dr. Hecker has quite recently conducted an elaborate inquiry into the diphtheria death-rate during the years 1883–93 in a number of German cities, and he states that it is a decreasing one.

The problem of diphtheria in London is as yet unsolved, neither is its solution likely to be accomplished through such isolated, individual investigations such as have hitherto prevailed. What is required is the appointment of a Commission, composed of men abreast of the time, acquainted with modern methods, and capable of pursuing experimentally, if necessary, the course of this scourge.

Fortunately, as regards cholera, our past experience has enabled us to cope satisfactorily with what was at one time our most dreaded foe, and although Europe has suffered severely, England has escaped since the outbreak of cholera in London in the year 1866.

The freedom of London from this, to a large extent, water-borne disease brings us to the consideration of another malady in the communicability of which water is also largely responsible, *i.e.* typhoid fever.

In this connection it is satisfactory to read the following: "A point well deserving of observation, is the diminishing London typhoid fever death-rate."

Although it cannot be assumed that it is entirely due to improvement in the water supply of London, yet the evidence of the connection between typhoid fever and impure water supplies, has been too firmly established not to permit of the London water companies obtaining some credit for this improved hygienic condition.

On this point, the evidence afforded by the city of Zürich is instructive, for it has been distinctly found that since the establishment of the new filtration works in 1886, and the consequent greatly improved bacterial quality of the water distributed, a very marked diminution has taken place in the number of cases of typhoid fever. This fact has been vouched for after most careful investigation of facts and statistics by the city authorities.

Again, we have only to recall the invariable increase in cases of typhoid fever in Paris, when in consequence of an insufficient supply of purer sources of water, recourse has to be had to that of polluted river Seine water. Now Dr. Percy Frankland, in his reports to the Local Government Board, showed, for the first time in this country, the bacterial purification which Thames water undergoes at the hands of the London water companies; and although in his recent report to the Royal Society on the vitality of the typhoid bacillus in various waters, he points out that, whilst unable to increase in numbers, it can yet remain alive for days and weeks in water, yet we may assume that the typhoid bacillus will submit, as all ordinary water microbes, to the purification processes which Thames water undergoes before delivery, processes which Dr. Percy Frankland has repeatedly shown, removes frequently as many as 99 per cent. of the bacteria present.

Under the heading of "Administration," we read that the Council's inspectors made numerous inspections of dairies and milk-shops, as well as cow-sheds; as a result of these investigations, no less than 133 cases of scarlet fever were discovered as occurring on milk-shop premises, 46 cases of diphtheria and membranous croup, 21 cases of typhoid fever, 10 cases of small-pox, 5 cases of erysipelas, and 2 cases of measles. These probably represent only a proportion of the actual number of cases which took place in such establishments. Knowing as we do that milk offers every facility for the growth and abundant multiplication of pathogenic germs, it may be easily conceived how much zymotic disease may have been disseminated broadcast from these centres of infection.

In the recent report issued by the Royal Commissioners on tuberculosis, we find the following significant paragraph: "In regard to milk, we are aware of the preference by English people for drinking cow's milk raw, a practice attended by danger on account of possible contamination by pathogenic organisms. The boiling of milk, even for a moment, would probably be sufficient to remove the very dangerous quality of tuberculous milk."

We quote these words in full, not only because of the official weight which attaches to them, but because it is of such great hygienic importance that these facts should be known and realised by the general public.

On the continent, the practice of drinking raw milk is fast becoming obsolete, and sterilised milk is an article of commerce, and successful so-called "milk sterilising associations" have been formed for its distribution.

We have seen that, as regards the zymotic-disease death-rate, London is less favourably situated than the majority of the capitals of Europe. May we not possibly find at least one cause of this, to us humiliating fact, in the insular prejudice which prevails in favour of raw milk?

In conclusion, valuable as statistics may be and undoubtedly are, it must be remembered that there is yet much which statistics cannot reveal, that a lower death-rate cannot express the whole result of hygienic enterprise and progress. To adequately measure the value of sanitary reform to the community at large, we must look as well to the numerous and important improvements which have resulted in the increased comfort and well-being of the individual, and it is in such directions that the London County Council has accomplished some of its most useful and meritorious work.

THE RECENT RACE OF AUTO-MOBILE CARRIAGES IN FRANCE.

LAST month a most interesting race of auto-mobile carriages took place in France. The course taken was from Versailles to Bordeaux, and then back to Paris. June 11 was fixed for the day of starting, and forty-six carriages were to have taken part in the race, but only twenty-eight arrived in time, twenty-two of these taking active part, and nine performing the journey within a hundred hours; eight of the latter were worked by petroleum or "gazoline," and one by steam.

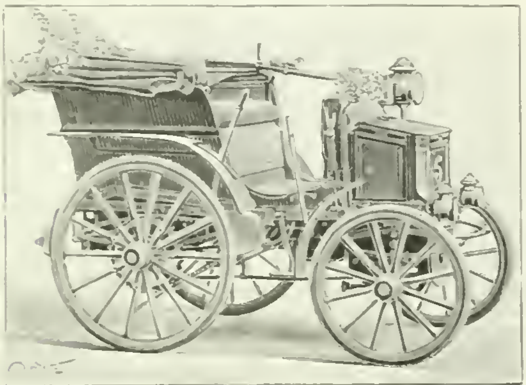


FIG. 1.—No. 5. MM. Panhard and Levassor's carriage, worked by gazoline, and seated for two persons (2nd prize, 12,000 francs). Arrived June 13, at [illegible]

The accompanying illustrations, which we are enabled to reproduce by the courtesy of the Editor of *La Nature*, are from photographs taken at the exhibition of the carriages on their return. No. 5 (Fig. 1) is the one which was the first to arrive back in Paris. It received the second prize, for it only seats two persons, and a regulation had been made, that no carriage seating less than four persons could receive the first. No. 16 (Fig. 2) reached Paris fourth, but received the first prize, for on reckoning up the time taken in the journey, it was found to be [illegible] less than that taken by No. 8. The third prize was won by No. 15 (Fig. 3).

FIG. 2.—No. 16. MM. Peugeot Frères, worked by gazoline, and [illegible] four persons (1st prize, [illegible] francs). Arrived June 14, at [illegible]

Taking all the facts into consideration, it appears that the lighter carriages travelled best. This proves the advantage of using petroleum or gazoline, for in order to produce one horse power it requires per hour $1\frac{1}{11}$ lbs. of gazoline, whereas, if it were worked by steam, at least $6\frac{3}{5}$ lbs. of coal and $39\frac{3}{5}$ lbs. of water would be necessary per hour, and if worked by electricity, there would have to be accumulators of the weight of 220 lbs.

Light carriages have many advantages, for besides having to be less careful about the weight of fuel, they can also have lighter constructed wheels. M. Michelin's carriage, with pneumatic tyres, went the whole distance without an accident, whereas the steam vehicles, one and all, had mishaps, owing almost always to their great weight.

It would take up much time and space to relate the many incidents which occurred; suffice it to say that, apart from ordinary breakdowns, in some towns the travellers were hindered by the inhabitants, in others they were enthusiastically pelted with flowers.

These auto-mobile machines are evidently the carriages of the future. According to the *Times* of July 10, a journey has quite recently been performed in our own country by the Hon. Evelyn Ellis, who was accompanied by Mr. T. R. Simms, managing director of the Daimler Motor Syndicate. The carriage is a four-wheeled dog-cart, and will hold four persons, with room also for two portmanteaus. It was built by Messrs. Panhard and Levassor, of Paris, and is worked by petroleum, the cost being about a halfpenny an hour. The journey undertaken by Mr. Ellis, a distance of fifty-six miles, was performed in five hours and a half.

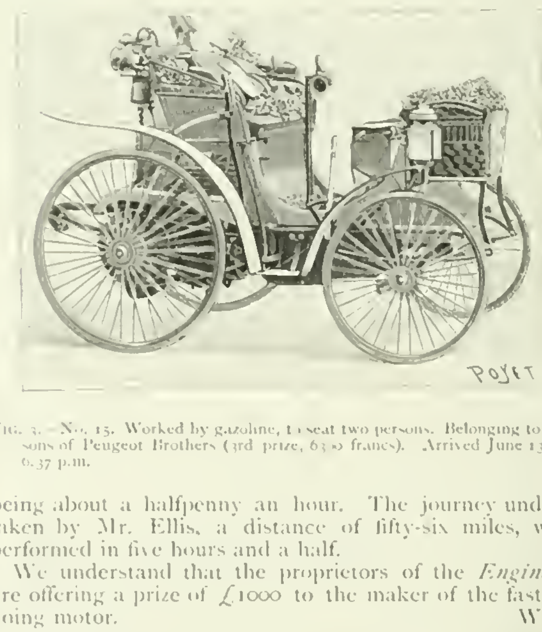


FIG. 3.—No. 15. Worked by gazoline, to seat two persons. Belonging to the sons of Peugeot Brothers (3rd prize, 6300 francs). Arrived June 13, at 6.37 p.m.

We understand that the proprietors of the *Engineer* are offering a prize of £1000 to the maker of the fastest going motor. W.

NOTES.

WE regret to notice that Prof. C. C. Babington, F.R.S., Professor of Botany in the University of Cambridge, died on Monday morning, at the age of eighty-six.

PROF. RAMSAY has been elected a Correspondant of the Paris Academy of Sciences, in the Section of Chemistry, and M. Sabatier has been elected a Correspondant of the Section of Anatomy and Zoology.

MR. H. J. CHANEY, of the Standards Department, Board of Trade, will attend the Sexennial Conference of the International Committee on Weights and Measures at Paris, on September 6 next, as the representative of Her Majesty's Government.

DR. CARL BARUS, of the Smithsonian Institution, has, says *Science*, accepted the Hazard Professorship of Physics in Brown University. It is stated that Brown University has recently spent £20,000 in the building and equipment of a physical laboratory.

THE death is announced of Prof. Baillon, Director of the Botanical Laboratory of the Faculty of Medicine at the Sorbonne. Prof. Baillon was one of the most distinguished of French botanists, and perhaps quite the most prolific author of works in that science of the last quarter of a century. The *Times* gives the following details of his life. He was born at Calais, November 30, 1827, and was destined for the medical profession. He prosecuted his studies at Paris, and soon obtained prizes for work in "L'École Pratique," and in the hospitals. In 1855 he received the double degree of doctor of medicine and of the natural sciences. In 1864 he was appointed Professor of Medical Natural History to the Faculty of Paris, and soon afterwards Professor of Hygiene to the Central School of Arts and Manufactures. He was decorated with the Légion d'Honneur on August 17, 1867, and promoted to Officer July 13, 1888. His chief publication was "Histoire des Plantes," a vast undertaking, in twelve fully-illustrated volumes, the publication of which commenced in 1866, and concluded only three years ago. It has been partly translated into English. His next great work was a "Dictionnaire de Botanique," which he begun in 1876; the first volume appeared in 1878, and the fourth in 1885. He also published a number of monographs and studies on various natural orders and groups of plants.

MR. W. N. MOORE has succeeded Prof. Mark W. Harrington as Chief of the U.S. Weather Bureau.

MR. O. A. L. PIHL, whose careful measurements of the stars in the cluster χ Persei are well known in astronomical circles, has just died at Christiania.

PROF. J. G. AGARDH has presented his fine collection of dried algæ to the University of Lund, on the condition that it remains there intact, and the specimens not be lent out.

MR. CHARLES LEIGH, assistant in the General Library of the Natural History Museum, South Kensington, has been appointed to the post of assistant secretary and librarian to the Manchester Literary and Philosophical Society, created under the Wilde Endowment Fund.

A SHARP earthquake shock was felt at Algiers at 11.25 on the night of Friday last, July 19. The direction of motion is said to have been from west to east.

THE National Herbarium of the United States at Washington has been transferred from the building of the Department of Agriculture, and now forms a part of the National Museum in the Smithsonian Institution. The collection of grasses remains, however, with the Department of Agriculture, as also do the collections of the Divisions of Vegetable Pathology and Forestry. A movement is now on foot among American botanists for providing the National Herbarium with a suitable building and a staff of scientific assistants.

THE adjudicators appointed under the provisions of the deed of settlement of the Daniel Hanbury Memorial Fund have, says the *Pharmaceutical Journal*, awarded the eighth Hanbury Gold Medal to Dr. August Vogl, Professor of Pharmacology and Pharmacognosy in the University of Vienna. The medal is awarded biennially for the prosecution or promotion of original work in the chemistry and natural history of drugs. On the last occasion, in 1893, it was awarded to the late Johann Michael Maisch, who received it just before his death.

THE following grants have been made by the Council of the Chemical Society on the recommendation of the Research Fund Committee:—£30 to Messrs. J. J. Hummel and A. G. Perkin, for the investigation of certain natural colouring matters. £10 to Dr. H. Ingle, for the purchase of various aldehydes, ketones, and hydrazine, to continue his work on stereoisomeric osazones. £10 to Dr. J. J. Sudborough, to continue his work on diortho-substituted benzoic acids. £15 to Mr. E. Haworth, for the synthesis of an acid having the composition $C_8H_{14}(COOH)_2$, and the comparison of its properties with those of camphoric acid. £5 to Mr. R. E. Doran, for a research on the preparation of mustard oils by the reaction of chlorocarbonic esters with lead thiocyanate. £15 to Dr. W. A. Bone, to continue a research on the substituted succinic acids, and on the behaviour of various trimethylene compounds on treatment with the sodium compound of ethylic malonate. £10 to Dr. B. Lean, to extend his work on the derivatives of ethylic butane tetracarboxylate. £20 to Dr. J. Walker, for an investigation of the conditions of equilibrium between the cyanates and the corresponding ureas.

MR. W. SAVILLE-KENT, who has recently returned from Western Australia, has presented and otherwise placed at the disposal of the Trustees of the British Museum a further collection of Madreporarian corals and sponges collected by him on the north-western coast-line of the above-named colony. The series includes many new species and specimens calculated to prove attractive exhibits in the public galleries. With this latest addition included, the Natural History Museum becomes possessed of the most complete collection of Australian Madreporaria that has yet been brought together, and which now comprises typical examples collected by the same authority from every region of the extensive coral-producing waters of the Australian continent. Mr. Saville-Kent will probably be engaged for the next few months in the compilation of a book dealing generally with the more interesting natural history observations and investigations he has recorded and prosecuted during the past ten years while holding the appointments of Commissioner of Fisheries to the several Governments of Queensland, Tasmania, and Western Australia.

BY the provisions of the will of the late Dr. William Johnson Walker, two prizes are annually offered by the Boston Society of Natural History for the best memoirs written in the English language on subjects proposed by a Committee appointed by the Council. For the best memoir presented, a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased to one hundred dollars, at the discretion of the Committee. For the next best memoir, a prize not exceeding fifty dollars may be awarded. The competition for these prizes is not restricted, but is open to all. Attention is especially called to the following points:—(1) In all cases the memoirs are to be based on a considerable body of original and unpublished work, accompanied by a general review of the literature of the subject. (2) Anything in the memoir which shall furnish proof of the identity of the author shall be considered as debarring the essay from competition. (3) Each memoir must be accompanied by a sealed envelope enclosing the author's name and superscribed with a motto corresponding to one borne by the manuscript, and must be in the hands of the Secretary on or before April 1 of the year for which the prize is offered. The subjects for 1896 are:—(1) A study of an area of schistose or foliated rocks in the eastern United States; (2) a study of the development of river valleys in some considerable area of folded or faulted Appalachian structure in Pennsylvania, Virginia, or Tennessee; (3) an experimental study of the effects of close-fertilisation in the case of some plant of short cycle; (4) contributions to our knowledge of the general morphology or the general physiology of any animal, except man. The subjects for 1897 are: (1) A study of glacial, fluviatile, or lacustrine phenomena associated with the closing stages of the glacial period; (2) original investigations in regard to the chalazal impregnation of any North American species of Angiosperms; (3) an experimental investigation in cytology; (4) a contribution to our knowledge of the morphology of the Bacteria.

REPORTS upon the circumstances attending an explosion which occurred in the Timsbury Colliery last February, prepared by Mr. J. Roskill and Mr. J. S. Martin, have just been published in a Blue Book. The explosion is interesting because fire-damp is practically unknown in the colliery. In this colliery, as throughout the Radstock series of the Somersetshire coalfield, naked lights are used; it is exempted from the application of the section of the Rule which prohibits explosives being taken down in mines except in cartridges, and gunpowder alone is used for blasting. It is evident from the inquiry that this exemption should be cancelled, and Mr. Roskill recommends that the use of gunpowder, except in cartridges, should be prohibited. Although before the explosion parts of the colliery were known to be dry, while more or less dust occurred in places, yet the mine was not regarded as a "dry and dusty mine." Judging from the explosion, however, the mine should come within that category. The explosion occurred at a spot which was apparently not dry and dusty within the meaning of the Act; but it was, if not caused, certainly intensified, by the presence of dust at much greater distances than twenty yards from the spot, though the Rule relating to shot-firing in a dry and dusty place, only prescribes watering within a radius of twenty yards. The moral drawn from the disaster is (1) that roburite, or one of the so-called flameless explosives, should, in future, be used instead of powder, and (2) that when places in a mine are admittedly dry and dusty, every place in the mine should be considered to be so, for the purpose of shot-firing, in order to make it imperative that, in such mines, the precautions prescribed by General Rule 12 should be observed in *all* places of firing.

WE have received a copy of the Report of the Epping Forest Committee presented to the Court of Common Council on June 13, of the present year, and containing the memorials which were reprinted in these columns a short time ago (June 13, p. 158). In presenting the Report the chairman, Mr. Deputy Halse, said that "if the action of your Committee were judged alone by the weight of authority attaching to those who have expressed themselves to be so entirely in accord with the past management of the Forest, a complete answer to the charges has already been made; but we prefer to await and present to your Honourable Court the Report of the eminent experts in Forestry whom we consulted last year, and by whose opinion and decision we are perfectly prepared to be judged and bound." We understand that the Committee of experts visited the Forest last week, and their judgment will be awaited with interest. Nothing could, however, strengthen the hands of the Committee more than the memorials which are now made public with their attached signatures. The value of the Report from a public point of view is greatly enhanced by a set of photographs reproduced from the illustrations in one of the daily papers, and placed opposite the views of the actual places which the newspaper artist is supposed to have represented. The article from the paper itself is reprinted *in extenso*, with a note stating that "the above article was accompanied by the illustrations reproduced on the annexed photographic sheet. Its accuracy may be judged from the photographs of those portions of the Forest supposed to be illustrated, which were taken within two days of the appearance of the article." The absurdity of the clamour, which is raised year after year by a small and irresponsible body of agitators, is well brought out by the article and its illustrations when confronted with the true representations. Any paper that lends itself in future to such perversions will justly forfeit public confidence. The keen interest taken by the people in the management of Epping Forest is a very healthy sign, but the case against the present Conservators must indeed have been feeble if it was found necessary to resort to such pictorial artifices as are exposed in the Report issued by the Common Council.

UNSETTLED weather has prevailed in most parts of the British Islands during the last week, and thunderstorms have occurred in various places, while falls of rain exceeding an inch in twenty-four hours have been recorded on several days. In London, there were two distinct thunderstorms on Sunday last, one of which, between two and three p.m., was accompanied by an exceptionally heavy fall of hail. The amount of rain in London on that day was about 1·3 inch, which is the heaviest fall in twenty-four hours since last October.

AT the recent meeting of the Australasian Association for the Advancement of Science at Brisbane, Mr. C. L. Wragge proposed the erection of a meteorological station on Mount Wellington, Hobart. The proposal was supported by Mr. H. C. Russell, Government Astronomer of New South Wales, and by the Royal Society of Tasmania, in consequence of which the Government voted the necessary funds. An experimental station has just been established by Mr. Wragge on the summit of the mountain at a height of 4166 feet above sea-level, and a permanent observatory-house is now in course of erection. There are also corresponding stations at the Springs (2495 feet), and at Hobart (160 feet); we have no doubt, therefore, that results of importance will be derived from them. Mount Wellington is about four miles distant from Hobart, in a straight line, and rises almost directly from the level of the sea: it consequently offers considerable advantages for meteorological research.

THE Pilot Chart of the North Atlantic Ocean for July contains monthly charts, representing graphically the regions where fog was experienced most frequently on the North Atlantic during 1894. As this year can be taken as a typical one to illustrate the distribution at different seasons, it is interesting to note that during the first three months of the year fog is experienced on the Grand Banks and to the westward, but not in large quantities. During April it begins to extend to the northward and eastward, increasing in frequency as the spring advances, and reaching its maximum, generally, in June or July, during which months it may be expected anywhere between the American coast and this country in large areas and of long duration. In August the fog begins to dissipate in the eastern part of the ocean, and in September the decrease is very perceptible. During the remaining three months the charts show that it reaches its minimum again, and is mostly restricted to the westward of 40° west longitude.

SOME brief telegrams in the daily papers announced the occurrence of an earthquake in the Meshed district of Persia on January 17, but gave little indication of its destructive character. The centre of the earthquake appears to have been near Kúchán, a town which has been damaged or destroyed by earthquakes several times during the present century, the last occasion being in 1893, when it was completely reduced to ruins. After this the town was rebuilt on the old site, but the houses were made very largely of wood. At the beginning of this year, the new town contained about 2000 houses and 8000 inhabitants. On January 17, shortly before noon, another disastrous earthquake occurred. It lasted about a minute, and the shock was so severe that it completely destroyed every house in the town, with the exception of a few small shanties. The wooden pillars of the better-built houses were all broken in the middle. Numbers of people were buried in the ruins, but, owing to the lightness of the materials, the loss of life was much less than it would otherwise have been. The local authorities estimated the number of deaths from two to six thousand, but the careful inquiries of an attaché at the British Consulate-General at Meshed have reduced this figure to about

700. Orders have been issued by the Persian Government for the town to be rebuilt near Hai Hai, a place six or seven miles to the south-east, which experience has shown to be safe from destructive shocks.

THE history of the Russian Biological Station, on the island of Solowetzk in the North Sea, has already been given in our columns (NATURE, November 1894, p. 83). One of the most interesting of the results achieved by the naturalists of the laboratory has been the discovery of a remarkable lake on the island of Kildine in the Arctic Ocean. This lake, which is completely separated from the sea by a narrow strip of land, was discovered by the Russian naturalist, M. Herzenstein, who was struck by finding in the lake a fish which is exclusively marine in habit, namely the common cod. Further observations by MM. Faussek and Knipowitsch have elucidated the peculiar features of the fauna of the lake. On the surface the water is fresh, and is inhabited by fresh-water animals, such as Daphnids, &c.; this water is brought to the lake by streams from a neighbouring marsh. Under the superficial layer of fresh water is found salt water, supporting a Marine fauna—Sponges, Sea-anemones, Nemertines, Polychætes, marine Molluscs (*Chiton*, *Æolis*, *Astarte*), Starfish, and Pantopods. There is even a regular littoral zone beneath the fresh water, characterised by small *Fuci*. The bottom of this lake is covered with mud exhaling an odour of sulphuretted hydrogen, and is not inhabited. The water of the lake shows a slight ebb and flow, attaining a vertical height of only a few inches, while the tides in the adjacent sea are considerably greater. This fact would appear to point to the existence of some subterranean communication between the lake and the sea.

SOME important additions to a knowledge of the latest Mesozoic and early Tertiary mammalia have recently been made from Patagonia and the Uinta Basin. From the former place a collection of ungulates of very late Cretaceous date is described by Señor F. Ameghin in the *Bol. Inst. Geografico Argentino*, t. xv., 11 and 12. The most important is a new genus, *Pyrotherium*, which is made the type of a new sub-order, regarded as ancestral to the Proboscidea, and showing marsupial affinities. A number of other new genera are also described, and it is anticipated that when the fossil localities, which are very difficult of access, have been more fully investigated, still more valuable information on the late Mesozoic mammalia will be obtained. Large Dinosaurs and birds also occur in these beds.

PROF. H. F. OSBORN reports in the *Bull. Amer. Mus. Nat. Hist.*, New York, vol. vii., art. 2, on a more extensive collection than has hitherto been obtained from the Eocene beds of the Uinta Basin. Beneath the true Uinta fauna comes one which is intermediate between it and the Bridger and Washakie faunas, and thus supplies a most important link in the faunal succession of this province, while at the same time it shows affinities to the Miocene fauna of the White River. Among the mammalia found in this transitional fauna are a monkey, and species of *Telmatotherium*, which definitely confirm the view that that genus was ancestral to the Titanotheria. It is expected that still more valuable results may be got from a more thorough exploration that is being made this year.

THE application of electricity to locomotion has recently made notable progress in the United States. At a trial of electric motors at Nantasket Beach, near Boston, a few days ago, it is stated that a speed exceeding sixty miles an hour was attained; and the experiment demonstrated the utility of this motor for suburban traffic. The system went into practical and regular operation on the Nantasket Beach Railway at the end of June. A successful test has also been made at Baltimore of the electric locomotive designed to draw trains through the tunnel, 7430 feet long, in that city. This and its companion—the first locomotives of the kind ever built—have each two trucks and eight wheels, sixty-two inches in diameter. Flexibly supported on each truck are two six-pole gearless motors, one for every axle. A maximum speed of fifty miles an hour is to be developed, and it is guaranteed that the locomotive will pull 1200 tons at a speed of thirty miles an hour. When coupled to a six-wheel New York Central locomotive, the electric locomotive pulled it up and down the track at will, against the pull of the steam locomotive.

AT a recent meeting of the Société Française de Physique, M. Pierre Weiss gave an account of the results of his experiments on the aelotropic magnetic properties of crystallised magnetite. The magnetisation curve of magnetite crystallised in the cubic system presents the same general features as those of iron, nickel and cobalt. The magnitude of the magnetisation (*i.e.* the permeability), however, varies with the inclination of the magnetising field to the crystallographic axes. Experiments have been made by a ballistic method suitably modified so as to permit of observations being made on very small specimens. The results thus obtained have been confirmed by other experiments in which a small disc of magnetite was rotated in a strong magnetic field, and the variations in the induction measured by means of a small coil surrounding the disc and connected to a ballistic galvanometer. The discs examined were cut parallel to the faces of the cube, octahedron and rhombic dodecahedron. If the results are expressed by drawing radii vectores from a given point of such length that they represent the magnetisation of the specimen in that direction when saturated, the surface which contains the ends of all these radii vectores is a cube with rounded edges, and with its faces slightly hollow. The magnetisation is the same in all directions contained in a plane parallel to one of the faces of the octahedron, so that the above-mentioned surface is cut by such a plane in a circular section. An experiment illustrating this aelotropic property of magnetite was shown before the Society. A small disc of magnetite placed on a plate of glass between the poles of a strong electro-magnet, turned so that one of its axes of maximum permeability was parallel to the field. Besides the difference which these experiments show between a body crystallised according to the cubic system and an isotropic body, they also show that the theories which regard magnetisation as resulting from the orientation of particles of fixed magnetic moment are insufficient to explain the magnetisation of crystalline bodies.

DURING his recent visit to the Algerian Sahara, M. Janssen made some decisive observations concerning the absorption bands near the D line of the solar spectrum, supposed to be due to atmospheric oxygen. The object was to test whether these absorption bands correspond to those observed on transmitting white light through a tube containing condensed oxygen. In some previous experiments on this question, M. Janssen had obtained these bands by means of a tube 60 m. long, containing oxygen compressed up to 6 atmospheres. An account of the Sahara observations is given in the *Comptes rendus*, together with a theoretical investigation concerning the equivalent height of the atmosphere. Starting with the remarkable law discovered by M. Janssen that the absorptive power of a gas is proportional to the thickness traversed and to the square of the density, the integration of the different layers of the atmosphere with their different densities gives 3981 m. as the equivalent thickness for a vertical ray of light. But since the density of oxygen is only 0·208 of that of the atmosphere, this number must be multiplied by 0·043, the square of that density. This gives 172 m. as the equivalent thickness of the oxygen layer. This thickness, at a pressure of one atmosphere, would not be sufficient

for showing the absorption bands, and this accounts for their absence when the sun is high in the heavens. But as the sun sets, the thickness of air traversed by its rays increases, and at an altitude of 4° the conditions are the same as those in the com. tube at 6 atmospheres pressure. At this altitude they do in fact appear, and the excessive dryness of the desert air precludes the possibility of their being due to water vapour. Thus both the terrestrial origin of these oxygen bands, and also the validity of Janssen's law of absorption, have received a striking confirmation.

THE fifth volume of the *Geographical Journal*, comprising the numbers issued during the first six months of this year, has just been published.

WE have received the Report for the year 1894-95 of the Royal Garden, Calcutta, by the Curator, Dr. G. King, issued by the authority of the Government of Bengal. It reports a considerable amount of work done in the improvement of the Gardens, and especially in the increase and arrangement of the Herbarium.

THE number of periodicals, both in Europe and America, dealing with electrical matters is considerable, the last addition to the list being the *Electrical Journal*, a new monthly published in San Francisco. The first number contains a long account of the "Express" system of telephone switchboard. Other articles appearing deal with the efficiency of electric plants, the electrical installation on board the cruiser *Olympia*, and the field of operations of an electrical engineer.

THE volume containing the *Proceedings* of the American Association for the Advancement of Science at the forty-third meeting, held at Brooklyn last August, has lately been issued. As we gave at the time a report of the work of the Sections, and printed some of the presidential addresses in full, it is only necessary for us now to say that the volume is very well produced, and contains many very valuable papers.

THE fourth and apparently concluding volume of the *Seismological Journal of Japan* has recently been published. It consists of a very valuable paper of nearly 400 pages, by Prof. Milne, "A Catalogue of 8331 Earthquakes recorded in Japan between 1885 and 1892." The materials were obtained from 968 stations, distributed over the whole empire, the total number of documents being perhaps not less than eighty or a hundred thousand. In the first catalogue are given for each shock the time of its occurrence, the land-area shaken, and data by which the position of the epicenter and the boundary of the disturbed area are approximately determined. The second catalogue states the seismic district to which each shock belongs, the lengths of the axes of the disturbed area in tens of miles, from which the total area can be roughly ascertained, and, when the shock is submarine, the distance of the epicenter from the shore. The chief object of the paper is to provide trustworthy materials for future investigations, but some results have been already obtained and are briefly described. Prof. Omori's work on after-shocks has been referred to in a previous number (vol. li. p. 423). The distribution of earthquakes in Japan forms perhaps the most important section. Earthquakes, it appears, are singularly rare in the central parts of the country, which includes the [illegible] districts where active volcanoes are numerous. The majority [illegible] originate along the eastern coast of the empire, [illegible] of submarine origin. A large number seem to [illegible] face of the steep monoclinal slope which Japan presents [illegible] the Pacific Ocean. Earthquakes are numerous where the slope is steep, and rare where it is comparatively gentle (see pp. 201-2). They are frequent in those districts where movements of secular elevation or depression are now taking place. Earthquake-sounds are often heard, but more so in the rocky mountainous districts than on alluvial plains. At the close of the paper is given a list of 301 seismic disturbances observed from 1889 to 1893 in Europe and at Teneriffe with the horizontal pendulum of Dr. von Rebeur-Paschwitz. Seven of these disturbances, and possibly five others, correspond to earthquakes in Japan.

THE flora of the Caucasus has lately been the subject of several interesting explorations and speculations by Russian botanists. The old data, contained in the works of Boissier and Ledebour, are now of little value, on account of the too broad remarks concerning the distribution of the different species, such as *Caucasus, provincia Caucasica*, and so on, which one finds in these otherwise classical works. On the other hand, such recent explorers as N. Kuznetsoff and A. Krasnoff, who have paid great attention to the composition of the floras of different parts of Caucasia, and their probable origin, have rather raised a series of most important geo-botanical questions than solved them definitively; while MM. Lipsky, Alboff, and Akinfieff have devoted their chief attention to the collection of positive systematic data, with exact indications relative to the distribution of different species. We have now in the "Memoirs (Trudy) of the Kharkoff Naturalists" (vol. xxvii.) a first instalment, by the last-named botanist, of a detailed list of plants in the middle parts of the Caucasus main ridge, with full indications concerning their vertical and horizontal distribution. Considering the generalisations of M. Kuznetsoff and M. Krasnoff as premature under our yet imperfect knowledge of the orography and geology of Caucasia, M. Akinfieff only ventures to formulate a few conclusions; namely, that the flora of Colchida is the youngest in Caucasia, as it has the least number of species, and especially of endemic forms, and that it contains but a small part of what constitutes the Mediterranean flora, as well as very little of what is found in other parts of Caucasia. The flora of Daghestan, Asiatic in its origin, has, on the contrary, in its steppe, sub-Alpine and Alpine representatives, a wide distribution over all Caucasia, with the exception of Colchida; four-fifths of the surface of Caucasia are thus genetically connected for their flora with Asia, and one-fifth only with Europe, the boundary between the two being, not the main ridge, but a broken line running approximately from Stavropol, or rather north of this town, along the water-parting between the Kuban and the Terek, to the Elborus, along the main ridge to the Adai-khokh, and further to the Mesques Mountains and the Suram Pass. It should be said that this conclusion seems to agree very well with what we now learn about the orgraphical structure of Caucasia, from which it appears more and more that the Mesques Mountains must be considered as a continuation of the border-ridge of the Asia Minor plateau, which ridge runs along the south-eastern coast of the Black Sea, and is continued north-east to meet the main ridge.

WE have received from Dr. Doberck, Government Astronomer of Hong Kong, the report of that observatory for 1894, containing *inter alia* an account of nineteen typhoons which occurred during the year, and the paths of which have been laid down on two plates. Information regarding storms is regularly exhibited and telegraphed whenever they can be justified by the observations received, but the work is apparently much interfered with by the tardy arrival of telegrams from the outlying stations. For the purpose of elucidating the behaviour of typhoons and other meteorological features, observations are regularly extracted from the logs of ships which visit the China seas, and tabulated for future use; in addition to these, observations are received from about forty land stations. The astronomical and magnetical work of the observatory has been regularly carried on, as in former years.

WHEREAS a few years ago the discovery of a new spirillum form was hailed as a bacteriological novelty, we are now constantly receiving fresh additions to this interesting group of microbes. With improved methods their detection and isolation have been rendered comparatively easy, and they are now found fairly widely distributed in water. Sanarelli isolated no less than thirty-two different vibrios from the river Seine, sewage-effluent, and pond water, and various authorities in Germany have detected such forms in rivers. So far the larger number have been obtained from river water, and have been but rarely met with in well water; but quite recently MM. A. Zawadzki and G. Brunner, of the Imperial Institute for Preventive Medicine in St. Petersburg, have discovered and isolated three vibrios from polluted well water, which do not liquefy gelatine, and in other respects are easily distinguishable from Koch's cholera vibrio. As regards their pathogenic properties, it is stated that white mice were quite unaffected when the vibrios were subcutaneously introduced. The investigations and descriptions have been carefully done and are fully recorded, and the authors are persuaded that they have discovered new forms. It is, however, difficult to decide this point, for only a slight acquaintance with the literature of the subject is apparent; and whilst the authors complain that Eisenberg's catalogue of bacteria is out of date, and those of Roux and Lustig are respectively incomplete, they do not appear to have any acquaintance with Percy Frankland's "Micro-organisms in Water," containing descriptions of over 200 bacteria found in water, neither have they consulted many important memoirs on vibrios which have been published in recent German and French journals.

THE writer of the note on p. 277, referring to hygrometric observations on the Sonnblick mountain, inadvertently wrote, "atmospheric electricity," instead of "atmospheric humidity," in the second line of the note.

THE additions to the Zoological Society's Gardens during the past week include a Mozambique Monkey (*Cercopithecus pygerythrus*) from East Africa, presented by Mrs. A. Canning Fysh; a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mr. A. Kagele; an Irish Stoat (*Putorius hibernicus*) from Ireland, presented by the Viscount Powerscourt; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Miss Dorothy Lowndes; a Bosch-bok (*Tragelaphus sylvaticus*) from South Africa, presented by Mr. W. Champion; six Orbicular Horned Lizards (*Phrynosoma orbiculare*) from Mexico, presented by Mr. E. J. Scarbrough; a West African Python (*Python sebæ*) from West Africa, presented by Mr. Edward Straw; a Red-sided Tit (*Parus varius*) from Japan, a White-browed Amazon (*Chrysotis albifrons*) from Honduras, two Adorned Terrapins (*Clemmys ornata*) from Central America, deposited; a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

ALTITUDE AND AZIMUTH OF POLARIS.—It is a matter of common knowledge that the Pole star is about a degree and a quarter from the true pole, so that azimuths and latitudes cannot be directly determined by observations of this star. The usual mode of procedure is to employ tables reducing the observations to the true pole; a graphical method of performing this rather tedious reduction, with an accuracy sufficient for most purposes, has been devised by A. Tanakadate, of Tokio (Sūgakub.-Kizi.) It is shown that the usual formula for the calculation of azimuth corresponds very nearly with the equation of a circle of radius $p \sec \phi$ (p being the polar distance of Polaris, and ϕ the latitude of the place of observation), and the centre of which is displaced above the origin by an amount equal to $\frac{p^2 \tan \phi}{\cos \phi}$. An origin being chosen near the middle of a sheet of squared paper, degrees and minutes are marked off along the axes in both directions, and a circle is drawn on the same scale with radius and displacement of centre adapted to the latitude as defined above. Radiating straight lines drawn from the origin correspond to different hour angles, the line $t=0$ being that along which the centre of the circle is displaced. The abscissa of the point where the line corresponding to the hour angle at which an observation is made cuts the circle, gives directly the azimuth of Polaris, the star being east or west of the true north according as the point lies to the right or left of the origin in the diagram. Neglecting errors of construction, the readings will only differ by a few seconds from the calculated results, and it is shown that even these errors can be reduced by slightly enlarging the radius of the circle.

If a circle be drawn from the origin as centre, with radius equal p, the diagram can also be used for reducing the latitude from observations of the Pole star by giving a small correction to the hour angle, $\frac{1}{2} p \tan h \sin t$, where h is the observed altitude, and t the hour angle. The ordinate of this circle gives the correction to be applied to the observed altitude in order to obtain latitude.

It is pointed out in the paper that these principles may easily be embodied in an instrument, and, in fact, such a contrivance is now in use among the students of astronomy in the Imperial University.

OBSERVATIONS OF DOUBLE STARS.—The measurements of position angles and distances of double stars made at the Paris Observatory from July 1890 to the end of last year, are published by M. Bigourdan in a very concise form in the *Bulletin Astronomique* for July. The telescope employed was that of the western tower, having an object glass 0·305 m. diameter and a focal length of 5·25 m., the magnifying power usually being 478. Most of the observations were made in the twilight or in the early night, at which times the star images are at their best. The list of stars observed includes about 150 from the Dorpat catalogue, 76 from the Pulkowa catalogue, and nearly 30 others; in many cases there are long series of measures of the same pair. A filar micrometer was employed.

At the Berlin Observatory, Dr. V. Knorre has used a double image micrometer in the measurement of double stars, and some of the results are given in *Ast. Nach*, 3300. The measures appear to agree very well with those of M. Bigourdan, in the case of stars common to the two sets of observations.

A GREAT NEBULA IN SCORPIO.—In the course of his work on the photography of the Milky Way, Prof. Barnard exposed a plate on the region near Antares for 2h. 20m. on March 25, 1895. The resulting negative showed a vast and magnificent nebula, intricate in form, and apparently connected with many of the bright stars of that region, including Antares and σ Scorpii. The nebula is gathered in cloud-like forms, the greatest masses being around ρ Ophiuchi and two neighbouring small stars. This photograph was taken with the Willard lens of 6 inches aperture, with which Prof. Barnard has previously obtained such splendid results.

Even more interesting is a photograph of the same region taken with a "lantern lens" of $1\frac{1}{2}$ inches aperture and 5 inches equivalent focus, the exposure being 2h. 18m. The scale of this photograph is about 10° to the inch, and in addition to bringing out some new points about the great nebula, it shows the sky itself in that region to be very wonderful. The first photograph had shown that the nebula occupied a singularly blank part of the sky, from which large vacant channels diverged towards the east, and the negative taken with the lantern lens showed that these channels ran irregularly eastward for 15° or 20°.

The photograph taken with the lantern lens shows that the new nebula extends southward for two or three degrees beyond Antares and σ Scorpii in a southward direction. An elongated nebula about 2° or 3° long, involving the star ν^2 Scorpii, is also seen on the photograph.

Prof. Barnard goes on to say that "this magnificent nebula is one of the finest in the sky, and as it involves so many of the bright stars in that region it would imply that they are essentially at the same distance from us." (*Ast. Nach.* 3301). The unpretentious character of one of the instruments employed by Prof. Barnard is not the least remarkable feature about this new discovery.

New Variable Stars.—*Wolsingham Observatory Circular*, No. 42, received from the Rev. T. E. Espin, announces that a red star of Secchi's Type III., magnitude 8·4, was detected at his Observatory on July 14, in R.A. 10h. 52·4m., Decl. 2° 11′ (1900). The star is probably a new variable, and is not in the southern *Durchmusterung*. The star designated Espin 1021 is also probably variable.

THE BRITISH MEDICAL ASSOCIATION.

As already noted, the sixty-third annual meeting of the British Medical Association will be held in London next week. From the programme of final arrangements published in the current number of the *British Medical Journal*, it is evident that the meeting will be of exceptional interest and importance. The President-elect is Sir J. Russell Reynolds, Bart. An address in Medicine will be delivered by Sir William Broadbent, Bart.; an address in Surgery by Jonathan Hutchinson, F.R.S.; and an address in Physiology by Prof. Edward Albert Schafer, F.R.S. The scientific business of the meeting will be conducted in fifteen sections: Numerous papers have been received by each Section, and specific points have been selected for discussion. In the Section of Medicine, presided over by Dr. F. W. Pavy, F.R.S., the following subjects have been selected for discussion: (1) Diphtheria and its treatment by the antitoxin; acute lobar or croupous pneumonia, its etiology, pathology, and treatment; the causes of acute rheumatism and its relation to other affections. The President of the Surgery Section is Sir William MacCormac, who will make some introductory remarks, in which he will refer to the effects produced by modern rifle bullets on the human body. The following subjects have been selected for discussion: The diagnosis and treatment of fractures of the upper third of the femur, including the neck; the surgical treatment of cysts, tumours, and carcinoma of the thyroid gland and accessory thyroids. Sir William Priestley presides over the Section of Obstetrics and Gynæcology. The President of the Section of Public Medicine is Dr. Ernest Hart. The regular business of this Section will commence each day with a formal discussion by gentlemen who have been invited to open the debates. The subjects selected are as follows: Presidential address: Water-borne disease and its prevention; discussions upon the regulation of the slaughter of animals for human food and the inspection of animals before and during slaughter; the insecurity of tenure of extra-Metropolitan Medical Officers of Health under the Public Health Act, 1875. The Section of Psychology has for its President Dr. W. J. Mickle. The President will open the section with an address on the brain. A discussion has been arranged to take place on each day, the subjects being: On the treatment of melancholia; on insanity, in relation to criminal responsibility; on epilepsy, and its relation to insanity. The President of the Physiology Section is Dr. David Ferrier, F.R.S. In this Section a discussion on the mechanics of the cardiac cycle will be introduced by Prof. Haycraft and Dr. D. Paterson; the following will take part: Dr. Noel Paton, Dr. Lauder Brunton, F.R.S., and Dr. Gibson. The Anatomy and Histology Section has for its President Mr. Henry Morris. The following subjects have been selected for discussion: Art in its relation to anatomy; the development and structure of the placenta; the topographical anatomy of the abdomen. The President of the Section of Pathology and Bacteriology is Dr. Samuel Wilks, F.R.S. The work of the Section includes the demonstration of the malaria parasite by Dr. P. Manson, with some facts as to its life-history. There will be a discussion upon this, and upon neuritis; vaccinia and variola; pernicious anæmia; and lymphadenoma. The President of the Section of Ophthalmology is Mr. H. Power. The following discussions have been arranged in this Section: On [illegible] of recurrent ophthalmia; on the diagnosis of [illegible]; on the question of operating in chronic glaucoma. The Section of Diseases of Children has for its President Mr. John H. Morgan; and the President of the Section of Otology is Sir W. Dalby. The Section of Pharmacology and Therapeutics has for its President Sir William Roberts, F.R.S. In this Section there will be a discussion upon serum therapeutics, and [illegible] requirements of the profession with reference to the revision of the *British Pharmacopœia*. Dr. Felix Semon is the President of the Section of Laryngology; and Dr. H. Radcliffe Crocker, of the Dermatology Section. Finally, the ethics of the medical profession has a Section to itself, presided over by Dr. W. F. Cleveland. Only members of the British Medical Association, invited guests, and accredited strangers, will be allowed to attend the general meetings or the meetings of Sections. The reception-rooms will be opened on Monday, July 29, at 12 o'clock noon. The members' reception-room is in the large hall of King's College. A separate reception-room has been provided for invited foreign guests next to the members' reception-room, and another for ladies at the Royal Society's Rooms, Burlington House. The arrangements for the conduct of the work of the Sections, and for the comfort of the members, have been admirably arranged, so there is every promise that the meeting will be a very successful one.

HELIUM, A CONSTITUENT OF CERTAIN MINERALS.[1]

I.

The gas obtained from the mineral cleveite, of which a preliminary account has been communicated to the Royal Society (*Proceedings*, May 2, 1895), has been the subject of our investigation since the middle of April. Although much still remains to be done, enough information has been gained to make us believe that an account of our experiments, so far as they have gone, will be received with interest.

We have attempted to ascertain, in the first place, from what minerals this gas, showing a yellow line almost, if not quite, identical in wave-length with the line D_3 of the chromospheric spectrum, and to which one of us has provisionally given the name "helium"—a name applied by Profs. Lockyer and Frankland some thirty years ago to a hypothetical solar element, characterised by the yellow line D_3 of wave-length 5875·982 (Rowland). We may state at once that it is not our purpose to attempt to prove this coincidence, but willingly to leave the subject to those who are more practised in such measurements.

We propose therefore, first, to discuss the terrestial sources of this gas; second, to describe experiments on products from several sources; and last, to propound some general views on the nature of this curious substance.

1. *The Sources of Helium.*

It is usual in a memoir of this kind to cite previous work on the subject. It would be foreign to our purpose to discuss observations on the solar spectrum; our memoir deals with terrestrial helium. And we have been able to find only one short note of a few lines on the subject; it is a statement by Signor Palmieri (*Rend. Acc. di Napoli*, xx. 233), that on examining a lava-like product ejected by Vesuvius, he found a soft substance which gave a yellow spectral line of wave-length 587·5; he promised further researches, but, so far as we know, he did not fulfil his promise. He does not give any details as to how he examined the mineral.

An account has already been given in Part I. of Dr. Hillebrand's investigations on the gases occluded by various uraninites which he was so unfortunate as to mistake for nitrogen. Dr. Hillebrand was so kind as to supply us with a fair quantity of the uraninite he employed; and it is satisfactory to be able to confirm his results so far; for it is beyond doubt that the gas evolved from his uraninite by heating it in a vacuum or by boiling with sulphuric acid contains about 10 per cent. of its volume of nitrogen. It is therefore not to be wondered at, that he formed the conclusion that the gas he had was nitrogen; for he obtained some evidence of the formation of nitrous fumes on passing sparks through a mixture of this gas with oxygen; he succeeded in obtaining a weighable amount of ammonium platinichloride from the product of sparking it with hydrogen in presence of hydrochloric acid; and, in addition, he observed a strong nitrogen spectrum in a sample of the gas transferred to a vacuum tube. Had he operated with cleveite, as will be shown later, he would have in all probability discovered helium (*Bull. U.S. Geological Survey*, lxxviii. 43).

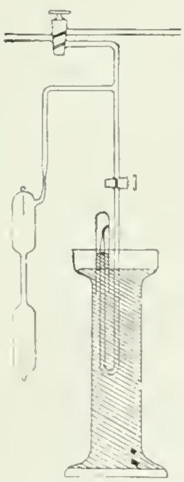
To extract the gas from small quantities of minerals, from 2 to 5 grams of the coarsely powdered substance was heated in a small bulb of combustion-tubing, previously exhausted by a Töpler's pump. As it was found that water and carbon dioxide were often evolved, a soda-lime tube and a tube filled with phosphoric anhydride were often interposed between the bulb

[1] A paper by Prof. William Ramsay, F.R.S., Dr. J. Norman Collie, and Mr. Morris Travers, read before the Chemical Society on June 20.

and the pump. After most of the gas had been evolved, the temperature was raised until the hard-glass bulb began to collapse.

Many of the minerals evolved hydrogen; hence, after the gas had entered the pump, the bulb was completely exhausted, and the gas was sparked with oxygen, no alkali being present. The oxygen was then absorbed with caustic soda and pyrogallic acid, and the gas was transferred to a vacuum-tube. As this process of transference proved very convenient, it is worth while to describe it in full.

The apparatus is shown in the annexed figure. It consists of a tube provided with a perfectly-fitting stop-cock; this tube is connected with a Töppler's pump. The vacuum-tube or tubes to be fitted are sealed to a lateral branch above the stop-cock. The lower part is bent into a sharp U, and the end drawn out to a point and sealed. The stop-cock is then turned full on, and the whole tube is completely exhausted, until the vacuum-tube shows brilliant phosphorescence, or, indeed, as often happens, ceases to conduct the discharge; the stop-cock is then closed. A mercury trough is placed below the bend of the tube, and the latter is sunk until the closed end disappears below the mercury. A small tube, which need not contain more than 1 c.c. of the gas to be introduced into the vacuum-tube, is then placed over the closed end of the bent tube, and the mercury trough is lowered. The sealed end is then broken by pressing it against the interior of the gas-tube, when gas enters up to the stop-cock. On carefully opening the stop-cock a trace of gas is passed into the vacuum-tube; this gas is then



pumped out and collected below the delivery tube of the Töppler's pump. One such washing with gas is usually sufficient. The stop-cock is again opened, and a sufficient amount of gas introduced into the vacuum-tube to show the spectrum. The vacuum-tube is then removed by sealing, and the gas still remaining in the bent tube may be transferred to the pump and collected. It is seen that this method permits of the filling of a vacuum-tube absolutely without loss, and it may be added with great expedition.

The results obtained with the minerals examined are given in the following table.

The spectrum of helium is characterised by five very brilliant lines; these occur in the red, the yellow, the blue-green, the blue, and the violet. In every case, except with hjelmite, fergusonite, and xenotime, in which cases the lines were merely seen, all these lines were identified by simultaneous comparison in the same spectroscope with the spectrum of helium from clèveite. With the gas from samarskite and in some other cases a still more careful comparison was made, and the absolute coincidence of every visible line was ascertained.

From many of these minerals, a hydrocarbon was extracted; this was manifested by the non-absorption of the gas by caustic potash until after explosion with oxygen. It would be interesting to ascertain whether the hydrocarbon is present as such in the mineral, or is formed during the heating, for in all cases where a hydrocarbon was evolved, a large quantity of hydrogen was also obtained. If a vacuum-tube be charged with the crude gas, merely deprived of carbon dioxide by caustic alkali, the spectrum consists almost wholly of the fluted bands of carbon.

Name of mineral.	Source.	Result.
Yttrotantalite ...	Rachwane, Ceylon	Hydrogen and helium.
Samarskite	Unknown	A little hydrogen and nitrogen. After sparking with oxygen over caustic soda, 15 grams yielded approximately 4 c.c. of helium. At high pressure (4 mm.) the unsparked gas shows fluted carbon spectrum. At low pressures this is invisible.
Hjelmite	Fahlun, Sweden...	No hydrogen: trace of helium.
Fergusonite ...	Ytterby, Sweden..	Do. do.
Tantalite	Fahlun, Sweden...	Trace of helium.
Pitchblende ...	Cornwall	50 grams yielded about 0·5 c.c. of helium. After fusion with hydrogen potassium sulphate a further very small quantity was obtained.
Pitchblende ...	Unknown	Small quantity of helium.
Polycrase	Hitterö, Norway..	Do. do.
All these minerals contain uranium.		
Monazite... ...	N. Carolina	Contains hydrogen and helium in fair quantity.
,, ...	Fahlun, Sweden...	Do. do.
,, ...	Bahia	Do. do.
,, ...	Skrotorp, near Moss, Norway...	Do. do.
Xenotime	Brazil	Hydrogen, and, after explosion with oxygen, a trace of helium.
Orangeite	Near Arendal......	Easily gave a good spectrum of pure helium.
Columbite	N. America	Much hydrogen: no helium.
Perofskite ...	Magnet Cove, Arkansas	Very little gas; partly hydrogen.
Wazite...........	Sweden	Hardly any gas: trace of hydrogen.
Thorite	Norway	Fair quantity of hydrogen.
Fluocerite	Unknown	Carbon dioxide; glass etched.
Orthite	Hitterö, Norway..	Carbon dioxide and small quantity of hydrogen.
Gadolinite	,, ,,	Carbon dioxide and small quantity of hydrogen.
Euxenite	,, ,,	Do. do.
Cerite	Unknown	90 grams gave 50 c.c. of gas, leaving 1·3 c.c. after explosion with oxygen. After sparking and absorbing oxygen, 0·1 c.c. remained. Not examined.
Blende ..	Unknown	No gas.
Rutile	,,	,,
Gummite.. ...	Flat Rock Mine, Mitchell Co., N. Carolina	No gas, except a trace of carbon dioxide.
Pyrolusite .	Unknown	Only oxygen.
Native platinum	Brazil	Trace of oxygen.
,, ,,	Siberia	Trace of oxygen in larger quantity, and trace of nitrogen.

It is here of interest to inquire which constituent of these minerals is effective in retaining helium. For this purpose, it is necessary to know their composition: but it has not been possible to make accurate analyses of all the samples of minerals treated. Hillebrand supposed that the gas was retained by the uranium, and states that its volume varies roughly with the amount of uranium oxides present. To decide the question, it is necessary to consider the composition of these minerals in some detail.

Yttrotantalite is essentially a tantalate of yttrium and calcium, containing a little tungstic acid, and small amounts of iron and uranium. The yield of helium was here small.

Samarskite is a niobate of uranium, iron, and yttria, containing smaller amounts of tungsten, zirconium, and thorium. The amount of uranium oxide is about 11 or 12 per cent.; of thorium oxide about 6, of yttrium 13, and of cerium 3. It yields a moderate amount of helium.

Hielmite closely resembles tantalite in composition, but contains stannic oxide. The yield of helium was minute.

Fergusonite is a niobate of yttrium and cerium, containing only a small amount of uranium, zirconium, tin, tungsten, &c. The yield of helium was here minute.

Tantalite consists of tantalate of iron and manganese; the helium obtained was a mere trace.

Pitchblende consists mainly of the oxide, U_3O_8. The rare metals are present in English pitchblende in very minute amount. The helium obtained was very minute in quantity, and had a large amount of the mineral not been used it would doubtless have escaped detection.

Polycrase is a niobate of uranium, containing titanium, iron, yttrium, and cerium. The amount of helium obtained from it was small.

These minerals, it will be seen, all contain uranium. To them must be added cleveite and broggerite, from which by far the best yield was obtained.

Monazite, which gave a good yield of helium, is a phosphate of cerium, lanthanum, and thorium, but does not contain uranium. It might serve, if necessary, as a source of helium, for it is comparatively cheap; it would form a more economical source than either cleveite or broggerite.

Xenotime is a phosphate of yttrium, and yields a trace of helium.

Orangite and *Thorite* are silicates of thorium containing small quantities of uranium and lead. The former of these yielded a fair amount of helium, but none could be obtained from a larger quantity of the latter.

From these details, it may be concluded that the helium is retained by minerals consisting of salts of uranium, yttrium, and thorium. Whether its presence is conditioned by the uranium, the yttrium, or the thorium, we are hardly yet in a position to decide. To judge by the Cornish ore, oxide of uranium alone is sufficient to retain it; but that its presence is not absolutely necessary is shown by its existence in monazite and xenotime. The high atomic weights of uranium and thorium, and the low atomic weight of helium suggest some connection; and yet yttrium, which possesses a medium atomic weight, sometimes appears to favour the presence of the gas; for yttrium is present in yttrotantalite, which, however, contains uranium, and in cleveite, in which uranium is present in relatively large amount.

None of the oxides of uranium, when heated in helium and allowed to cool, retains the gas; but similar experiments have not yet been made with oxides of thorium and yttrium, or with mixtures of these with uranium oxide.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

M. Liard, Director of higher education in France, has been raised to the rank of Commander in the Legion of Honour.

By the will of the late Mrs. Fraser, widow of the late Bishop of Manchester, a sum of £3000 is bequeathed to Oriel College, Oxford, for the foundation of a Scholarship.

Mr. Henry [illegible], who was an evening student in the Chemical Department of the Finsbury Technical College, has been elected by the Technical Instruction Committee of Accrington Town Council to the post of Principal and Head Master of the Accrington Municipal Technical Schools, just erected at a cost of £12,000.

After ten years of quiet and unostentatious work in temporary buildings, the authorities of the Cambridge Training College for Women Teachers have been able to erect large and handsome college buildings by means of a grant from the Pfeiffer Bequest and voluntary subscriptions. The new buildings will be formally opened on Saturday, October 19, by the Marquess of Ripon, and other well-known persons interested in education have promised to take part in the proceedings. Practical demonstrations will be arranged to illustrate some of the latest developments of educational method, both in teaching and training, so as to make the occasion one of special interest to those who are taking a share in the development of secondary education in England. The experiment of training teachers under new conditions, and to some extent on new lines, under the shadow of an old University, is of special interest, and the opening ceremony will afford a unique opportunity to those interested in secondary education to learn something of the nature and results of this experiment.

Her Majesty's Commissioners for the Exhibition of 1851 have made the following appointments to science research scholarships for the year 1895, on the recommendation of the authorities of the respective universities and colleges. The scholarships are of the value of £150 a year, and are tenable for two years (subject to a satisfactory report at the end of the first year) in any university at home or abroad, or in some other institution approved of by the Commissioners. The scholars are to devote themselves exclusively to study and research in some branch of science, the extension of which is important to the industries of the country: University of Edinburgh, John D. F. Gilchrist; University of Glasgow, Walter Stewart; University of St. Andrews, Henry C. Williamson; University College, Dundee, James Henderson; Mason College, Birmingham, Robert H. Pickard; University College, Bristol, Samuel R. Milner; University College, Liverpool, John T. Farmer; University College, London, Emily Aston; Owens College, Manchester, William H. Moorby; Durham College of Science, Newcastle-on-Tyne, Alexander L. Mellanby; University College, Nottingham, Martin E. Feilmann; Queen's College, Belfast, William Hanna; M'Gill University, Montreal, Robert O. King; Queen's University, Kingston, Canada, Thomas L. Walker; University of Sydney, John A. Watt; University of New Zealand, Ernest Rutherford.

SCIENTIFIC SERIALS.

American Meteorological Journal, June. The principal articles are:—The Thermophone, by H. E. Warren and G. C. Whipple. This is an instrument for measuring temperature, particularly of distant or inaccessible places. It was devised by the authors for the purpose of obtaining the temperature of the water at the bottom of a pond, but is also suitable for obtaining the temperature of the soil at various depths. The apparatus resembles Siemen's resistance thermometer, advantage being taken of the fact that different metals have different electrical temperature coefficients. The instrument is not yet self-recording. California electrical storms, by J. D. Parker. The object of the paper is to inquire into the causes of the infrequency of electrical storms in California. At San Diego, for instance, the Weather Bureau has only reported two electrical storms in the last sixteen years. Among the principal causes, the author mentions the humidity of the atmosphere, the absence of excessive heat during the rainy season (September to May), and the absence of cyclones during the dry season (May to September).

Wiedemann's Annalen der Physik und Chemie, No. 6. Survey of the present position of energetics, by Georg Helm. The two directions in which the conversion of physics into a science of energy has been most successfully carried out are those of mechanics and of thermodynamics. Two views of energy are at present struggling for supremacy, that which regards energy as a mathematical abstraction, non-existent except in equations, and that which regards energy as a concrete reality, filling space, and migrating continuously from one place to another. One of the chief generalisations of the science of energetics is this: In order that something may happen it is

sufficient and necessary that uncompensated differences of intensity exist.—Influence of gases in solution upon the silver voltameter, by John E. Myers. (See p. 276). –The aureole and stratification in the electric arc, and in discharges in rarefied gases, by O. Lehmann. The appearance of the electric arc with horizontal carbons is that of two gas jets burning against each other, and flaring vertically upwards. This is due to the currents of hot air ascending between them, and is the same as if the carbons were joined by a white-hot wire. There is no fundamental difference between the arc and discharges in rarefied gases, as may be shown by taking very small terminals or very large discharge vessels for the latter. That the current travels not only through the arc proper, but also through the surrounding "aureole," may be proved by approaching a magnet, which bends the aureole aside.—Magnetism of asbestos, by L. Bleekrode. The grey variety of asbestos is highly magnetic. Strips of so-called asbestos paper 4 by 3 cm. are attracted at 1 cm. distance by an electromagnet capable of carrying 5 kgr., and fibres of pure asbestos attract small particles of the same substance. Asbestos should only be used with great care in sensitive magnetic instruments.

Bulletin de l'Académie Royale de Belgique, No. 4.—On the specific heat of peroxide of hydrogen, by W. Spring. The method of cooling was employed, and aqueous solutions of various strengths were experimented upon. A 74 per cent. solution gave the value 0·6893, which fell to 0·6739 at 71 per cent., 0·6276 at 60 per cent., and 0·6208 at 34 per cent. On further dilution to 31 per cent. the specific heat rose again to 0·8065. Peroxide of hydrogen thus behaves very much like a solution of alcohol. Chemical decomposition probably exerts a strong influence upon the values at high concentrations, and 0·6208 must be taken as a superior limit. Woestyn's law would give 0·6840. Hence it follows that the internal work of hydrogen peroxide must be less than that of water. On metageometry and its three subdivisions, by P. Mansion. The author gives a sketch of a system of geometry of n dimensions, by which the three varieties, those due to Euclid, Riemann, and Lobatchevski, respectively, can be deduced from elementary considerations. The theorem that a straight line, two of whose points lie in a plane, lies in that plane altogether, applies to all the varieties. But Riemann's geometry is characterised by the proposition: If, in a plane, two straight lines which intersect in a point A also intersect in a second point B, all straight lines passing through A will also cut the line A B a second time. If the sum of the three angles of a single triangle is equal to two right angles, the same applies to all triangles, and the space will be Euclidean. In Riemann's curved space this sum is greater, and in Lobatchevski's curved space it is less than two right angles.—On the period of frost extending from January 27 to February 17, 1895, by A. Lancaster. This amount of frost is unprecedented since 1838, when the mean of the minima for the days between January 8 and 27 was $-13^{\circ}\cdot 4$ C. at Brussels. This year the mean was -11° C. The isothermals of mean temperatures during this period for Belgium show maxima of frost on the frontier of Limburg and north of Hasselt, the least cold being along an isothermal of -5° passing along the coast through Ostende.—On a silicate which probably constitutes a new mineral species, by G. Cesaro. This mineral, which accompanies hexagonite (a violet manganiferous tremolite), comes from St. Lawrence County, N.Y. It is colourless, or a delicate opaline-pink. Its hardness is 45. It crystallises in the orthorhombic system, and presents two cleavages along two planes of symmetry. Hitherto it has probably been taken for enstatite, but it is distinguished from this by the sign of its bisectrix, by the absence of well-defined prismatic cleavages, by its fusibility before the blowpipe, and by its angles. From anthophyllite it is distinguished by the absence of iron.—Lunar topographical measurements taken on photographs, compared with the records of Lohrman and Madler, by W. Prinz. A table is given of twelve craters near the centre of the disc, with the values of their diameters from the maps and photographs. The greatest difference between the two cartographers appears in the case of Ptolemæus, whose crater is given 21,518 m. broader by Madler than by Lohrmann, and the latter observer is confirmed by the photographs.

Bulletin de l'Académie Royale de Belgique, No. 5.—Chlorobromomalic anhydride, by Dr. A. J. J. Vandevelde. This is obtained by the action of bromine upon chlorofumaryl chloride. It is easily sublimed, even at ordinary temperatures, in a current of dry air, and can be purified in this manner. Its formula appears to be $CCl.CBr.(CO)_2O$. It fuses at 113° and boils at 203°. It has a very irritating but not disagreeable odour, and violently attacks the mucous membranes. It is soluble in alcohol, ether, chloroform, carbon bisulphide, and benzol, and easily crystallises in needles by concentration. When sublimed, it crystallises in plates. Water only dissolves it slowly, and an aqueous solution, when spontaneously evaporated, gives a very soluble deliquescent substance, which only crystallises when nearly dry.

Proceedings of the St. Petersburg Society of Naturalists, vol. i. No. 1–3.—The St. Petersburg Society of Naturalists has introduced this year a most welcome improvement in its publications. The *Proceedings* of the Society are now published separately, in advance of the *Memoirs*, and all the communications are summed up by the authors themselves in French or in German. We have already received three fascicules of the *Proceedings*, which contain a number of interesting communications: on the petrography of central Caucasus and on Vesuvian lavas, by M. Loewinson-Lessing; on the morphology and phylogenetic relations of the Myriapoda, by P. Schmidt, from which we learn that the *Pauropus Huxleyi* is possessed of a pair of tracheas, of a very plain structure, which open under the mandibles: on the flora of the Zerafshan region in Turkestan, by W. Komaroff; on the embryology of the Diplopodes, by N. Cholodkovsky; on the lymph glands of the earthworms, by G. Schneider; on geological researches in the Altai, by Prof. Inostrantseff; on the formation of the egg in the *Dytiscus*, by K. Saint-Hilaire; and on the Pantopodes of the Arctic Ocean and the White Sea, being a review of the species described and collected both by the author and different previous explorers, with a description of one new species and two new varieties.

Memoirs (Trudy) of the St. Petersburg Society of Naturalists, vol. xxiv., Section of Botany.—Besides the *Proceedings*, this volume contains two important works:—The sub-genus *Eugentiana* of Tournefort's genus *Gentiana*, by N. Kuznetsov, being an elaborate work of 530 pages, with a plate and geographical maps, and containing the systematic description of this sub-genus, established by the author, its morphology, and the geographical distribution of its species.—The flora of Crimea, by W. Agéenko, part ii., first fascicule, containing the tribes from the Ranunculaceæ to the Capparideæ. In the first volume of this work the author gave a review of the literature of the subject, as well as a review of the collections of Crimean plants which he had at his disposal, and an excellent sketch of the flora of Crimea and its dependency from the local physical and geological features of the country. Now he gives the full list of the vascular plants of Crimea, which will be followed by a review of the geological changes undergone by Crimea and their influence upon the present composition of the flora.

Bollettino della Società Sismologica Italiana, vol. i., 1895, Nos. 1, 2.—Whether, and to what extent, an earthquake-wave can afford criteria for reasoning with regard to the nature of the formations traversed by it, by Prof. P. M. Garibaldi.—On conical or horizontal pendulums, by Prof. G. Grablovitz. In this paper is described a simple form of horizontal pendulum designed for timing, or calling attention to, the beginning of a disturbance. The mode of suspension resembles that adopted by Gerard and Milne, and from the mass at the free end of the horizontal rod there projects downwards a wire into a small cup of mercury. When the pendulum is disturbed, an electric circuit is closed, and a bell is rung, or the time determined by stopping a clock, &c. The Lecco earthquake of March 5, 1894, by Dr. M. Baratta. This earthquake was a very slight one, and its interest lies in the discovery by its means of a new centre of disturbance in Lombardy, with which other slight shocks may also be connected. The relation between these earthquakes and the geological structure of the district is discussed.—Vesuvian notes (1892–93), by Prof. G. Mercalli.—Seismoscope for electrical registration, by Prof. G. Mugna.—Geodynamic levels for continuous registration, by Prof. G. Grablovitz. The author has had two water-levels constructed for the geodynamic observatory at Ischia. They are each $2\frac{1}{2}$ metres long, and are arranged north-south and east-west. The movements of the ground are indicated by floats, whose displacements are magnified fifty times by levers carrying pens at their free ends. Copies of the record obtained from the Laibach earthquake of April 14 are given.—On the velocity of propagation and on the length of seismic waves, by Prof. F.

Omori. (See p. 275.)—Notes on the state of Etna, by Prof. A. Riccò.—Notes on Italian earthquakes (January, February, 1895), by Dr. M. Baratta. These are inserted as an appendix to each number, and form a catalogue of all earthquakes, tremors and pulsations recorded at the Italian geodynamic and meteorological observatories, &c. They are a continuation of the valuable *Supplementi* to the *Annali* of the Ufficio Centrale di Meteorologia e Geodinamica.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 20.—"A Dynamical Theory of the Electric and Luminiferous Medium. Part II.: Theory of Electrons." By Joseph Larmor, F.R.S.

In a previous paper on this subject,[1] it has been shown that by means of a rotationally elastic æther, which otherwise behaves as a perfect fluid, a concrete realisation of MacCullagh's optical theory can be obtained, and that the same medium affords a complete representation of electromotive phenomena in the theory of electricity. The ponderomotive electric forcives were, on the other hand, deduced from the principle of energy, as the work of the surplus energy in the field, the motions of the bodies in the field being thus supposed slow compared with radiation. It was seen that in order to obtain the correct sign for the electrodynamic forcives between current systems, we are precluded from taking a current to be simply a vortex ring in the fluid æther; but that this difficulty is removed by taking a current to be produced by the convection of electrons or elementary electric charges through the free æther, thus making the current effectively a vortex of a type whose strength can be altered by induction from neighbouring currents. An electron occurs naturally in the theory as a centre or nucleus of rotational strain, which can have a permanent existence in the rotationally elastic æther, in the same sense as a vortex ring can have a permanent existence in the ordinary perfect fluid of theoretical hydrodynamics.

In the present paper a further development of the theory of electrons is made. As a preliminary, the consequences as regards ponderomotive forces, of treating an element of current $\iota\delta s$ as a separate dynamical entity, which were indicated in the previous paper, are here more fully considered. It is maintained that a hypothesis of this kind would lead to an internal stress in a conductor carrying a current, in addition to the forcive of Ampère which acts on each element of the conductor at right angles to its length. Though this stress is self-equilibrating as regards the conductor as a whole, yet when the conductor is a liquid, such as mercury, it will involve a change of fluid pressure which ought to be of the same order of magnitude as the amperean forcive, and therefore capable of detection whenever the latter is easily observed. Experiments made by Profs. FitzGerald and Lodge on this subject have yielded purely negative results, so that there is ground for the conclusion that the ordinary current-element $\iota\delta s$ cannot be legitimately employed in framing a dynamical theory.

This result is entirely confirmed when we work out the properties of the field of currents, considered as produced by the convection of electrons. It is shown that an intrinsic singularity in the æther, of the form of an electron e, moving with velocity $(\dot{x}, \dot{y}, \dot{z})$ relative to the quiescent mass of æther, is subject to a force e (P, Q, R), given by equations of the form

$$P = c\dot{z} - b\dot{y} - dF/dt - d\Psi/dx;$$

in which (a, b, c) is the velocity of flow of the æther where the electron is situated, and is equal to the curl of (F, G, H) in such way that the latter is Maxwell's vector potential given by the formula of the type

$$F = \int \frac{u}{r} d\tau + \int \left(B\frac{d}{dz} - C\frac{d}{dy} \right) \frac{1}{r} d\tau;$$

and where Ψ is the electrostatic potential due to the electrons in the field, so that $\Psi = c^2 \Sigma e/r$, where c is the velocity of radiation. These equations are proved to hold good, not merely if the motions of the electrons are slow compared with radiation, as in the previous paper, but quite irrespective of how nearly they approach that limiting value; thus the phenomena of radiation itself are included in the analysis.

An element of volume of an unelectrified material medium contains as many positive electrons as negative. This force (P, Q, R) tends to produce electric separation in the element by moving them in opposite directions, leading to an electric current in the case of a conductor whose electrons are in part free, and to electric polarisation in the case of a dielectric whose electrons are paired into polar molecules. In the former case, the rate at which this force works on a current of electrons (u', v', w'), is $Pu' + Qv' + Rw'$; it therefore is identical with the electric force as ordinarily defined in the elementary theory of steady currents. In the case of a dielectric it represents the ordinary electric force producing polarisation. So long as a current is prevented from flowing, the ponderomotive force acting on the element of volume of the medium is the one of electrostatic origin due to such polarisation as the element may possess, for as the element is unelectrified it contains as many positive electrons as negative. But if a current is flowing, the first two terms of (P, Q, R), instead of cancelling for the positive and negative electrons, become additive, as change of sign of the electron is accompanied by change of sign of its velocity: so that there is an electrodynamic force on the element of volume,

$$(X, Y, Z) = (v'c - w'b,\ w'a - u'c,\ u'b - v'a),$$

where, however, (u', v', w') is the *true* current composed of moving electrons, not the total circuital current (u, v, w) of Maxwell, which includes the rotational displacement of the free æther in addition to the drift of the electrons.

The electric force (P, Q, R) as thus deduced agrees with the form obtained originally by Maxwell from the direct consideration of his concrete model of the electric field, with idle wheels to represent electrification. It has been pointed out by von Helmholtz and others, that the abstract dynamical analysis given in his *Treatise* does not really lead to these equations when all the terms are retained; this later analysis proceeds, in fact, by the use of current-elements, which form an imperfect representation, in that they give no account of the genesis of the current by electric separation in the element of volume of the conductor.

The ponderomotive force (X, Y, Z) is at right angles to the direction of the true current, and is precisely that of Ampère in the ordinary cases where the difference between the true current and the total current is inappreciable. It differs from Maxwell's result in involving true current instead of total current; that is, the forcive tends to move an element of a material body, but there is no such forcive tending to move an element of the free æther itself. In this respect it differs also from the hypothesis underlying von Helmholtz's recent treatment of the relations of moving matter to æther.

The theory is applied (1) to the determination of the electric and magnetic stresses in material media and of the mechanical pressure caused by radiation, (2) to optical propagation, including detailed theories of dispersion and metallic reflexion, including also the influence of motion of the material medium. It is shown that if electrons are accepted as the basis of material atoms, the latter topic is fully elucidated; also that the theory is not at a loss when explanations of the phenomena of inertia, gravitation and spectra are demanded.

June 20.—"An Inquiry into the Nature of the Vesicating Constituent of Croton Oil." By Wyndham R. Dunstan, F.R.S., and Miss L. E. Boole.

The vesicating constituent, or more strictly, the pustule-producing constituent of croton oil, has been the subject of investigation by numerous chemists and pharmacologists during the past forty years. According to the researches of Buchheim, and more recently of Kobert and Hirscheydt, the vesicating action is due to an acid closely allied to oleic acid, which has been given the name of crotonoleic acid. This substance is now prepared on a large scale in Germany for medical use, being extracted from croton oil by the method devised by Kobert and Hirscheydt. This consists in saponifying with barium hydroxide that part of croton oil which readily dissolves in strong alcohol. The resulting barium salts are washed with water, then dried, and repeatedly extracted with ether, which dissolves the barium salts of oleic and crotonoleic acids. These salts are separated by means of ether, which dissolves only the barium crotonoleate, and this, when decomposed with dilute sulphuric acid and extracted with ether, furnishes the crotonoleic acid as a viscid oil.

Since very little is known about this acid, even its composition being undetermined, the authors prepared it with the object of studying its properties and, if possible, of determining the constitution since no fatty acid of known constitution exhibits the property of vesicating. Starting with the crotonoleic acid

[1] Printed in abstract in NATURE, xlix. pp. 260, 280.

prepared as described above, the lead salt was obtained and submitted to a process of fractional precipitation by adding successive quantities of water to its solution in alcohol. By this means crotonoleic acid was proved to be a mixture composed for the most part of inactive oily acids, the lead salts of which are precipitated first, whilst the true vesicating constituent (or its lead salt) is principally contained in the last fractions, and represents but a small proportion of the original material. It was observed that the conversion of the crotonoleic acid into a lead salt did appreciably affect its vesicating power.

The supposed active constituent of croton oil, crotonoleic acid, having thus been shown to be a mixture, the authors proceeded to attempt to isolate the vesicating constituent from croton oil direct.

By saponifying that part of croton oil which is soluble in strong alcohol with a mixture of lead oxide and water, and repeatedly fractionating an alcoholic solution of the lead salts with water, the later fractions, which possessed the greatest vesicating power, ultimately furnished, when submitted to a series of fractionations, a resinous substance having extraordinary power as a vesicant. This substance could not be further resolved by repeating the process of fractional precipitation of the alcoholic solution with water. The same substance was isolated from the so called "crotonoleic acid," and the authors propose to name it "croton-resin." To its presence the vesicating property of croton oil is due. The composition of croton-resin is expressed by the empirical formula $C_{13}H_{18}O_4$. So far all attempts to crystallise it, or to obtain crystalline derivatives from it, have been unsuccessful. It is a hard, pale yellow, brittle resin, nearly insoluble in water, light petroleum, and benzene, but readily dissolved by alcohol, ether, and chloroform. When heated it gradually softens, and is quite fluid at 90° C. Croton-resin has neither basic nor acidic properties: it may be boiled with a mixture of lead oxide and water without being appreciably affected. Ebullition with aqueous potash or soda gradually decomposes it, destroying its vesicating power. The products of this action are several acids, some of which are members of the acetic series. By oxidation of the resin with nitric acid a mixture of acids is obtained. The constitution of croton-resin is therefore complicated, and its molecular formula would appear to be at least $(C_{13}H_{18}O_4)_2$ or $C_{26}H_{36}O_8$. Since it is not saponified by a mixture of lead oxide and water, and as no glycerol could be detected among the products of its decomposition by alkalis, it is not a glyceride, and as it does not react with hydroxylamine or phenylhydrazine or sodium bisulphite, it is probably neither a ketone nor an aldehyde. The evidence so far obtained points to the conclusion that the constitution of the vesicating constituent of croton oil may be that of a lactone or anhydride of complicated structure.

"On the Magnetic Rotation of the Plane of Polarisation of Light in Liquids. Part I. Carbon Bisulphide and Water." By J. W. Rodger and W. Watson.

The aim of this investigation is the determination in absolute measure of the magnetic rotation of liquids at different temperatures, the effect of the chemical nature of the liquid on this property, and its correlation with other physical properties.

The present communication contains a description of the apparatus and method of experiment, and the results obtained with the standard liquids, carbon bisulphide and water, for sodium light, in a magnetic field of constant intensity, and at different temperatures between 0° and the ordinary boiling point.

In the case of carbon bisulphide three different samples were used, and identical results were obtained with three separate coils. In the following table are collected the mean values of the boiling point (b. p.), density at 0° (ρ_0), and Verdet's constant at 0° (γ_0). Verdet's constant may be defined as the rotation in minutes of arc produced in a column of liquid when the difference between the magnetic potentials at the ends of the column is equal to one C.G.S. unit.

		B. p.	ρ_0.	γ_0.
CS_2 No. 1	...	46°·25	1·29271	0·04348
CS_2 No. 2	...	46·26	1·29282	0·04347
CS_2 No. 3	...	46·26	1·29283	0·04347

It will be seen that the three different samples give practically identical values for the three physical constants.

The results obtained for the rotation of carbon bisulphide may be summed up in the following equation, where γ_t is the value of Verdet's constant at the temperature t,

$$\gamma_t = 0{\cdot}04347\,(1 - 0{\cdot}001696t).$$

The expression connecting rotation and temperature is therefore linear.

In the case of water the results are best represented by

$$\gamma_t = 0{\cdot}01311\,(1 - 0{\cdot}0_4305t - 0{\cdot}0_5305t^2).$$

Here the rate of change of the rotation with temperature increases as the temperature rises.

In the case of water the quotient γ/ρ, where ρ is the density is practically constant up to 20°, it then very slowly *increases*, the rate of increase between 20° and 100° being practically constant.

For carbon bisulphide the quotient γ/ρ *decreases* at a constant rate as the temperature rises, the rate of decrease being very much greater than the rate of increase in the case of water.

The measure of the molecular rotation which is usually employed in chemical investigations is

$$(M\gamma/\rho)_{\text{substance}} / (M\gamma/\rho)_{\text{water}},$$

where M is the molecular weight. Although the authors postpone a detailed discussion of the validity of this expression, they show that for carbon bisulphide, at any rate, its value changes with the temperature, and hence the conclusions obtained by its use regarding questions of chemical constitution, especially of tautomerism, are affected on this account.

They also point out that the above expression involves the properties of water. The only justification for the use of water in relative observations is the elimination of variations in the strength of the magnetic field in which the observations are made. If the temperature of observation is always the same, this can readily be done. If, on the other hand, the temperature varies, it is essential to know how the rotation of water alters with the temperature. In the past this alteration was unknown, and the arbitrary measure of the molecular rotation above referred to has come into use. Since an expression for the temperature variation has now been obtained it is to be hoped that observers will employ a measure of the molecular rotation which does not involve the properties of water. Indeed, other considerations make such a measure all the more desirable. Up till now the authors have made observations on eight liquids, besides water and carbon bisulphide, and in all cases except that of water the relation between rotation and temperature is linear, and the quotient, rotation divided by density, diminishes as the temperature rises. It is highly probable, therefore, that as regards magnetic rotation, as in the case of so many other properties, the behaviour of water is exceptional, and hence it is particularly ill-suited for the use to which it has been put. Again, on account of the smallness of the rotation in water, the unavoidable inaccuracies in determining its rotation, and thus estimating the strength of the magnetic field, produce a larger percentage error in the results than if a liquid, such as benzene, having a considerably higher rotation than water, were used for this purpose.

Chemical Society, June 20.—Mr. A. G. Vernon Harcourt, President, in the chair. The following papers were read:—On the "isomaltose" of C. J. Lintner, by H. T. Brown and G. H. Morris. Lintner's isomaltose is shown to be merely impure maltose, and the isomaltosazone derived from it is maltosazone; maltose is the only substance produced in the diastatic conversion of starch which yields a crystallisable osazone.—Action of diastase on starch: nature of Lintner's isomaltose, by A. R. Ling and J. L. Baker.—The transformation of ammonium cyanate into urea, by J. Walker and F. J. Hambly. The velocity of interconversion of urea and ammonium thiocyanate under various conditions in aqueous solutions has been quantitatively studied; the numbers obtained can be interpreted by the dissociation hypothesis.—Note on the transformation of ammonium cyanate into urea, by H. J. H. Fenton.—Some derivatives of humulene, by A. C. Chapman. A number of derivatives of humulene, the sesquiterpene contained in the essential oil of hops, are described.—Note on thio-derivatives from sulphanilic acid, by Miss L. E. Walter. The parasulphonate-xanthate, $SO_3K.C_6H_4.S.CS.OEt$, obtained by the interaction of potassium xanthate and diazotised sulphanilic acid, is readily converted into derivatives of the sulphydride, $SO_3K.C_6H_4.SH$, a number of which are described together with their oxidation products.—Helium, a constituent of certain minerals (part ii.), by W. Ramsay, J. N. Collie, and M. Travers. Fifteen out of about thirty minerals studied were found to yield helium, the density of the several samples of gas examined being about 2·2; the wave-length of sound in the gas corresponds to 1 : 1⅔, so that the atomic weight should be 4·4. Helium has the solubility 0·007 in water at 18°, and is hence the

least soluble gas known.—New formation of glycollic aldehyde, by H. J. H. Fenton. The acid, $C_4H_4O_6.2H_2O$, previously prepared by the author, yields glycollic aldehyde when heated with water: the aldehyde readily polymerises, yielding an amorphous hexose, $C_6H_{12}O_6$.—Ethereal salts of ethanetetracarboxylic acid, by J. Walker and J. K. Appleyard.—On the occurrence of argon in the gases enclosed in rock salt, by P. P. Bedson and S. Shaw. The nitrogen given off by the Middlesburgh brine contains about the same proportion of argon as does atmospheric nitrogen.—On the dissociation of gold chloride, by T. K. Rose.—On some physical properties of the chlorides of gold, by T. K. Rose.—The dissociation of liquid nitrogen peroxide (part ii.); the influence of the solvent, by J. T. Cundall. The dissociation of nitrogen peroxide in solution is dependent on the temperature and on the nature of the solvent; solutions in fourteen "inactive" solvents have been quantitatively examined.—Condensation of benzil with ethylic acetoacetate, by F. R. Japp and G. D. Lander.—On a method for preparing the formyl derivatives of the aromatic amines, by H. R. Hirst and J. B. Cohen. The primary aromatic amines yield formyl derivatives when treated with formamide in acetic acid solution.—A modification of Zincke's reaction, by H. R. Hirst and J. B. Cohen. The condensation of aromatic hydrocarbons with benzyl chloride, chloroform, &c., is readily brought about by amalgamated aluminium foil.—A method for preparing cyanuric acid, by W. H. Archdeacon and J. B. Cohen. Cyanuric acid and hydrogen chloride are obtained on heating urea and phosgene in toluene solution at 230° in sealed tubes.—The oximes of benzaldehyde and their derivatives, by C. M. Luxmore.—On a colouring matter from *Lomatia ilicifolia* and *Lomatia longifolia*, by E. H. Rennie. A yellow colouring matter, which seems to be hydroxylapachol, is found adhering to the seeds of the two species of Lomatia.—The colouring and other constituents contained in Chay root (part ii.), by A. G. Perkin and J. J. Hummel. In addition to the constituents previously isolated from Chay root, the authors now describe a hystazarin monomethyl ether and the three anthragallol dimethyl ethers.—The six dichlorotoluenes and their sulphonic acids, by W. P. Wynne and A. Greeves.—The disulphonic acids of toluene and of ortho- and para-chlorotoluene, by W. P. Wynne and J. Bruce.—Contributions to our knowledge of the aconite alkaloids. Part xii. The constitution of pseudaconitine: preliminary notice, by W. R. Dunstan and F. H. Carr.

PARIS.

Academy of Sciences, July 15.—M. Marey in the chair.—Researches on the electric discharge of the torpedo, by M. d'Arsonville. The author has investigated this discharge by means of self-registering apparatus, and has rendered it apparent by passing the current through various dispositions of a set of small incandescent lamps. At 10° C. the mean duration of a discharge is from 0·1 to 0·05 second. With torpedos from 25 to 35 cm. in diameter, kept for eight days in the laboratory basins, the E.M.F. oscillates between 8 and 17 volts, and the intensity between 1 and 7 amperes. There is no difference of potential between the two faces of the organ in repose. The two organs function synergically and with the same intensity, each organ having several sections giving independent discharges. During a discharge, the organ rises from 0·2° to 0·3° in temperature if short-circuited, but does not become heated if in open circuit. The electricity is produced in the organ itself, and not in the nerves serving it. M. Marey followed up this paper with a few appreciative remarks, emphasising the author's point that new light on the nature of muscular action might be expected from [illegible] on the electric organs of the torpedo, and [illegible] that the author intends studying the effect of certain [illegible] physical agents of which the action on muscles is [illegible].—On a bed of potassium and aluminium phosphate [illegible] in Algeria, and on the genesis of these minerals, by M. Ad. Carnot.—Calculation of fluid trajectories, by M. [illegible].—A comparison between electric motors with continuous currents and those with alternating currents, by M. [illegible].—On the absorption spectrum of liquid air, by Profs. Liveing and Dewar. Janssen's law that the intensity of the bands increases as the square of the density of oxygen would appear to indicate that these particular bands are due either to complex molecules produced by condensation, or to the encounters of molecules of ordinary mass, encounters which are more frequent as their free path is diminished. An examination of the absorption spectrum of liquid air and comparison with that of liquid oxygen under ordinary pressures shows that a thickness of 0·4 cm. of liquid oxygen gives a much greater band intensity than 1·0 cm. of liquid air. The bands in the liquid air spectrum become more intense as the nitrogen boils off. Mixtures of liquid air and oxygen confirm Janssen's law at low temperatures. Solid air, whether containing solid oxygen or not must remain doubtful, shows practically the same character and intensity of absorption as liquid air, hence the encounter theory is not borne out by experiment.—Action of the infra-red rays on silver sulphide, by M. H. Rigollot. Using silver sulphide as an electrochemical actinometer, its sensitiveness to infra-red rays has been recognised far beyond the last visible radiations. The E.M.F. produced may possibly be due to a calorific action.—On the detection and presence of laccase in plants, by M. G. Bertrand. Laccase has been recognised in many plants; a list is given. It appears only to be found in the rapidly developing parts, the older portions of plants not yielding this diastase-like substance.—On the essence of Linaloë, by MM. Ph. Barbier and L. Bouveault. This essence consists essentially of licareol with small quantities of a sesquiterpene, of licarhodol, and diatomic and tetratomic terpenes, together with 0·1 per cent. of a ketone, $C_9H_{14}O$.—On the penetration of embryos of "l'anguillule stercorale" into human blood and the relation between the presence of these embryos and certain fevers of hot countries, by M. P. Teissier.—On a transition form between cartilaginous and osseous tissues, by M. Joannes Chatin. The Gecko (*Platydactylus fascicularis*, Daud.) has furnished the tissue described.—On pelagic fishing in the deep sea, by MM. L. Boutan and E. P. Racovitza. The author is not able to confirm the existence of types specially adapted for life at great depths. He gives a list of forms found at from 400 to 500 metres below the surface, and shows that they are nearly the same as the forms asserted by Chun to be characteristic of great depths (1400 metres); the same types have even been collected near the surface. M. de Lacaze-Duthiers made some remarks on this paper, and particularly called attention to the suitability of the Banyuls station for this kind of work.—The phenomena of karyokinesis in the Uredineæ, by MM. G. Poirault and M. Raciborski.

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THURSDAY, AUGUST 1, 1895.

LINEAR DIFFERENTIAL EQUATIONS.

Handbuch der Theorie der linearen Differentialgleichungen. Von Prof. Dr. Ludwig Schlesinger, Privatdocenten an der Universität zu Berlin. Erster Band. (Leipzig: Teubner, 1895.)

DE MORGAN is reported to have said of the subject of differential equations, that it illustrated the proverb that he who hides knows how to find. This was true enough at a time when the sole aim of the analyst was to "solve" differential equations by reducing them to quadratures, or to construct ingenious puzzles for the benefit of undergraduates. Integration by series was known, of course; but this was regarded as a mean device, useful indeed for purposes of calculation, especially to the physicist, but unworthy of the serious attention of the pure mathematician.

A new era began with the foundation of what is now called function-theory by Cauchy, Riemann, and Weierstrass. The study and classification of functions according to their essential properties, as distinguished from the accidents of their analytical forms, soon led to a complete revolution in the theory of differential equations. It became evident that the real question raised by a differential equation is not whether a solution, assumed to exist, can be expressed by means of known functions, or integrals of known functions, but in the first place whether a given differential equation does really suffice for the definition of a function of the independent variable (or variables), and, if so, what are the characteristic properties of the function thus defined. Few things in the history of mathematics are more remarkable than the developments to which this change of view has given rise. Leaving out of account the theory of partial differential equations, which is still beset with many and serious difficulties, it is not too much to say that in the course of less than half a century the theory of ordinary linear differential equations alone has attained a degree of extent and importance which makes it comparable with almost any of the main branches of analysis.

The landmarks of the new departure are the memoir of Briot and Bouquet in the *Journal de l'École Polytechnique* (cap. 36), Riemann's paper on the generalised hypergeometric series, and Fuchs's memoir in the sixty-sixth volume of *Crelle's Journal.* Since the publication of this last work, more especially, the progress made has been exceedingly rapid: the general principles of the subjects have been permanently established, so as already to admit of methodical treatment, and numerous particular applications, all of great interest and beauty, have attracted and continue to invite the attention of mathematical explorers. Thus there is the problem of discovering whether a given equation has an algebraic integral, and, if so, of finding it; there is the theory of equations with doubly periodic coefficients; and there is the theory of differential invariants. Each of these is associated with some of the most brilliant discoveries of modern analysis, and each offers abundant opportunity for further research.

The wide extent of the subject, and the immense number of memoirs relating to it, have created an urgent need for systematic treatises to serve as an introduction to the theory, and presenting its main outlines in a proper perspective. Fortunately this want seems likely to be supplied before long; various excellent works, dealing wholly or in part with linear differential equations, have recently appeared or are in course of publication, and among these the book now under review will take an honourable place.

Dr. Schlesinger's work, to be completed in two volumes, is intended to give a coherent and comprehensive account of the theory in the light of its most recent developments. This first volume is divided into eight sections, exclusive of two introductory chapters, one historical, the other treating of the existence of an integral, and the general nature of the singular points. Of the eight sections, the first contains the first principles of the theory, mostly after Fuchs; the second discusses systems of independent integrals, reduction when particular integrals are known, Lagrange's adjoint equation, non-homogeneous equations, and Frobenius's theorems on irreducibility; the third relates to the fundamental equation; the fourth to unessential singular points; the fifth to equations of the "Fuchsian" class, that is to say, of which the coefficients are rational functions of x and all the integrals are regular; the sixth treats of the development of integrals within a circular annulus; and, finally, the seventh and eighth contain the general theory of equations with rational coefficients.

The treatment is entirely analytical, and is based principally on the methods of Weierstrass as expounded by Fuchs, Frobenius, Hamburger and others; thus the integrals are obtained in the form of power-series valid within a certain region of the plane of the complex variable, and no use is made of geometrical diagrams such as those employed by Schwarz, Klein, and Goursat. Moreover, except in the fifth section, which contains a brief discussion of Riemann's P-function and of the hypergeometric series, the author confines himself to the *general* theory, and does not consider special cases, or particular applications. The demonstrations, for the most part, are concise, and free use is made of the sign of summation and double suffixes. For these reasons the book is perhaps hardly suitable for those who are approaching the subject for the first time; but any one who has read, let us say, Goursat's thesis on the hypergeometric series, or Klein's lectures on linear differential equations of the second order, and is moderately familiar with the Weierstrassian function-theory, will be able to consult it with advantage. To those who are engaged in research, Dr. Schlesinger's treatise will be of great value, because those parts of the subject which are included within the author's plan are discussed with sufficient thoroughness, with a consistent notation, and in logical order; while the analytical table of contents gives references to the original sources in direct connection with the articles of the book which are based upon them. It is rather a pity, by-the-by, that the dates have not always been given in these references; the reader may very possibly wish to know the date of a paper, and not be able to consult the volume of the journal in which it appeared.

Mathematicians will look forward with interest to

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the appearance of the concluding volume of the treatise, which will contain, *inter alia*, a discussion of the group of an equation, and of the classification of equations according to the nature of the groups belonging to them. Until the work is complete, it is premature to express an opinion as to the degree of success with which the author has attained the object he has in view; but there can be no doubt of the valuable service which is rendered to science by the composition of a methodical treatise like this. So far as we are able to judge, account has been taken of all the most important researches which come within the scope of the present volume; the three last sections, in particular, include an account of the recently published papers of Helge von Koch, Poincaré, and Mittag-Leffler.

The proof-sheets appear to have been very carefully revised, so that the book is happily free from the crowd of misprints with which mathematical text-books, otherwise excellent, are not unfrequently disfigured. G. B. M.

THE RESEARCHES OF TESLA.

Inventions, Researches, and Writings of Nikola Tesla. By Thomas Commerford Martin. (New York: *The Electrical Engineer*, 1894.)

We have here an account of Nikola Tesla, his scientific inventions and work, by a devoted admirer. Mr. Martin is not a Boswell, and from the nature of the case his book could hardly have about it all that human interest which pervades the life and achievements of a veteran discoverer in science. Mr. Tesla is a young man whose career has been somewhat romantic, and whose ingenuity is such as to rank him very high indeed among the electrical workers and discoverers of the day. Born in Austro-Hungary, educated at the Realschule at Carstatt and the Polytechnic at Gratz, and professionally first in the Government Telegraph Department, and afterwards in Paris, his career as an engineer really began when he arrived in America little more than ten years ago.

In two or three years from the day on which he took off his coat in the Edison Works, Tesla motors had attracted attention, and he leaped at once to a position as a successful experimenter and inventor, which his subsequent work has only secured and made more important. His researches on the effects of alternating currents of high potential and frequency, in particular, though they had the misfortune to be made the subject of the speculations of the ordinary journalist, are of great scientific interest, and continued by Mr. Tesla himself and the army of enthusiastic workers we now have, cannot fail to yield theoretical results and practical applications which will more than fulfil the anticipations of those who took a sober and rational view of their possibilities. None of those who listened to Mr. Tesla at the Royal Institution will soon forget the almost [illegible] experiments performed, their clear exposition in what was to the lecturer manifestly a foreign language, and the enthusiasm which the results displayed excited in those present who were best able to judge of their scientific interest and importance.

Mr. Martin's account of Mr. Tesla's work is interesting, and yet perhaps it might have been in some respects better than it is. He has had excellent materials, such as the various lectures delivered by Mr. Tesla on his researches generally, the papers read from time to time to scientific societies on particular inventions and points of interest, and apparently the specifications of Mr. Tesla's patents. Our complaint, if we have one, is that this material has hardly been sufficiently worked up. Many of the lectures and papers were, as was inevitable, hurriedly composed, and the expression of Mr. Tesla's theoretical views contained in them is not always so clear and complete as it might have been made by one not so rapidly carried forward by the stream of discovery. A great inventor can hardly be expected to spend time weighing words and phrases, at any rate he has a title to be excused from doing so, which others who expound him do not possess. As it is, Mr. Martin's book is on the whole a reproduction of articles which appeared from time to time in the *Electrical Engineer* (of New York), and all we wish is that he could have spared the time and trouble necessary to cast the matter into a more homogeneous and symmetrical form.

For the lectures which are reproduced we are very grateful. They give Mr. Tesla's own description of his inventions, and his views on points of theory—views, which if not always orthodox, and sometimes expressed in language which appears strange, are always fresh and suggestive. The unavoidable repetitions of the same ideas, and recurring descriptions of the same apparatus, are not without some advantage, though they interfere with the unity of Mr. Martin's book, as they enable the lecturer's meaning to be made out more completely than would otherwise be possible.

The book is divided into four parts: Polyphase currents; Tesla effects with high frequency and high potential currents; miscellaneous inventions and writings; early phase motors and the Tesla oscillators. The two first parts are of course much more interesting than the remaining two, which have to do with such things as oil condensers, anti-sparking dynamo brushes, unipolar generators, the Tesla exhibit at the World's Fair, and the Tesla mechanical and electrical oscillators.

The discussion of polyphase currents, which occupies the first 115 pages of the book, has more unity of treatment about it than the second part, which consists mainly of the lectures Mr. Tesla delivered in this country and America. After a short introductory and biographical chapter, Mr. Martin proceeds to expound the principle of the rotating magnetic field and the construction of synchronising motors. A paper by Tesla, on a "New System of Alternate Current Motors and Transformers," is reproduced in this connection, and contains the foundation on which is based the remaining twenty-one chapters which make up Part i. These contain numerous modifications of the original idea, many of them exceedingly ingenious. A motor "depending on 'magnetic lag' or hysteresis" is described in Chapter xii. The peculiarity of this is stated in an introductory paragraph to be "that in it the attractive effects or phases, while lagging behind the phases of current which produce them, are manifested simultaneously and not successively." This statement itself seems to want some little exposition, though the arrangement is really very simple. An iron disc is pivoted within a fixed coil, wound just

large enough to admit the diameter of the disc one way, and a little more than its thickness the other. The coil carries two pole-pieces, one at each end, which project from opposite sides a little way round the disc. Thus opposite poles are stretched out as it were from the coil round the disc in the same direction. An alternating current passed round the coil magnetises both these pole-pieces and the disc, and the repulsion between the adjacent similar polarities of the disc and pole-pieces produces the rotation, the polarities of both being of course reversed with the current. The disc is wound with closed coils, so that the induced currents augment the turning couple developed. This arrangement is further developed into a "multipolar motor"; but in neither case is there any clear statement of how the action depends on hysteresis.

In connection with these and similar devices it would have been interesting to have had some estimate of efficiency, but generally speaking, in no part of the book is there any discussion of this most important question. Indeed, when the word energy is used it seems to bear a somewhat peculiar sense. For example, at p. 81 we have a statement as to the "energies" of the field and the armature, and the importance of these being equal if for a given sum the motor is to have the greatest efficiency. This passage is a little difficult of interpretation, if the word energy is to be taken as it ought to be in its technical sense throughout, though it is not very hard to make out the idea intended.

By far the most interesting portion of the book to a student of electricity generally is Part ii. The alternator of high frequency which Mr. Tesla used is fully described, and the arrangements for using it explained in the first of the lectures already referred to. The phenomena produced are set forth in the remaining chapters with numerous illustrations which render the descriptions very easy to follow. The whole subject of high frequency phenomena is very intimately connected with the researches of Hertz on the one hand, and the work of Mr. Crookes on the other, and forms a most inviting field of research for experimentalists who possess the necessary equipment. Whether always the theoretical view taken by Mr. Tesla is correct, is matter for legitimate difference of opinion. For one thing, we do not think that there is any difference at all between electric force produced by what is properly called electrostatic action and that produced by electro-magnetic action. The distinction is only mathematical the former force can be derived from a potential function, the latter cannot—and in a sense only expresses our ignorance of the mode of production of the force. But perhaps we are mistaken in supposing that Mr. Tesla regards the electric forces in these two cases as different in nature.

To every physical inquirer the perusal of these lectures cannot but be of the greatest benefit. It will again remind him that the field of research is unlimited, and quicken his scientific enthusiasm, if not to taking part in the work of this particular part of it, to at least prosecuting with renewed vigour the inquiry, whatever it is, which lies ready to his hand.

It was reported a few weeks ago that all the apparatus and machinery belonging to Mr. Tesla had been destroyed by fire. Every reader of his researches must sincerely sympathise with Mr. Tesla in his loss of valuable appliances and still more valuable time. That he at once set himself to repair the loss is only what was to be expected from his character; let us hope that it may result in such improvements of his means of experimenting as may, in some measure at least, make up for his disappointment, if it is not, what is perhaps too much to suppose, turned into a blessing.

A. Gray.

OUR BOOK SHELF.

An Introduction to Chemical Crystallography. By Andreas Fock, Ph.D., translated and edited by William J. Pope, with a preface by N. Story-Maskelyne, M.A., F.R.S. Pp. 189 and xvi. 8vo. (Oxford: Clarendon Press, 1895.)

This little book is issued by the Clarendon Press as a companion volume to Maskelyne's "Morphology of Crystals," which was recently reviewed in these columns. It is far from being a mere translation of Fock's "Einleitung in die chemische Krystallographie," which was published in 1888. That book contained a useful summary of the leading facts known about the origin and growth of crystals, and the general relations between their chemical composition and other properties, especially as regards isomorphism and the properties of mixed crystals. All this is contained in the present volume, which is, moreover, less sketchy than the earlier book, and the somewhat numerous inaccuracies which disfigured the German edition have been corrected. But it is in the additional matter that the chief alteration is to be found. About fifty pages have been introduced, containing a survey of those important contributions to our knowledge of crystals which have recently been made from the side of physical chemistry; the remarkable theoretical researches of Van t'Hoff and Willard Gibbs, and the quite recent experimental investigations of Bakhuis Roozeboom, to which they gave rise, are here very happily summarised and brought within the reach of the English elementary student.

In order to give a comprehensive survey of the origin and growth of crystals, it is necessary to take into account the properties of the solutions from which they separate, and several chapters are accordingly devoted to such subjects as the relations between osmotic pressure and concentration, the separation of double salts and those containing water of crystallisation, the conditions of equilibrium in a solution containing various *solutes* (to employ a convenient word suggested by Prof. Maskelyne in his preface as a term for the substances dissolved), and the resulting variations in the isomorphous mixtures which crystallise from such solutions; all these are subjects of great importance, which have up to the present time met with no adequate treatment in English text-books.

A treatise which merely summarises without criticism loses much piquancy and interest, and also some value as a guide to students. This objection may fairly be urged against Fock's book, which appears to accept without question all the observations reported by the author. It would have been better, for example, to indicate the insecure nature of some of the evidence which rests only upon microscopical observation, such as that of Lehmann and Vogelsang.

This book remains, nevertheless, an excellent survey of chemical crystallography, brought fully up to date, and one which will, we hope, open the eyes of English chemists to a new field of work.

Mr. Pope's translation is both fluent and accurate; he is further responsible for some of the new matter introduced into this edition. The book is lucid, readable, and interesting, and is one which does credit to the Clarendon Press.

Laboratory Exercises in Botany. By Prof. Edson S. Bastin, A.M. Philadelphia: W. B. Saunders, 1895.

For a laboratory manual this book is of great extent, for it includes more than 500 octavo pages, with no less than 87 plates. Yet it is more remarkable for what is omitted than for what is contained in it.

The first half of the book is devoted to organography, and consists of descriptions of the gross structure of a number of types of flowering plants, fully illustrated in the first 37 plates. This part of the book seems to us decidedly well done.

The second half, with 50 plates, is on vegetable histology. Strange to say, it deals simply and solely with the *vegetative* structure of phanerogams and vascular cryptogams. This branch of the subject is illustrated in great detail, and the anatomical work is sound, if not quite up to the highest modern standard.

Not a word, however, is said as to reproduction, development, or life-history. The words *pollen-tube, ovule, embryo-sac, archegonium, antheridium*, and *growing-point*, are sought in vain in the index, nor have we found any reference to them in the text, except that ovules are of course mentioned in the descriptive part. In fact, just those subjects which are most important in a scientific course of laboratory work are entirely passed over. The utter absence of any account of the lower cryptogams is also astonishing, for there is no indication that a second volume may be looked for.

The author is professor at a pharmaceutical college, and this fact may help to account for the extraordinary unevenness with which he has treated his subject. Students of pharmacy in America are no doubt required to have some acquaintance with the external characters of the higher plants, and some anatomical training may also be expected of them, with a view to the identification of drugs. Beyond this it would appear that their botanical education is not meant to go. The author has expended great pains on his work, but its manifest one-sidedness renders it quite valueless as a scientific guide to laboratory botany. Students of pharmacy in England are happily accustomed to a very different system of botanical teaching.

D. H. S.

The Source and Mode of Solar Energy. By I. W. Heysinger, M.A., M.D. (Philadelphia: J. B. Lippincott and Co., 1895.)

On the strength of an acquaintance with popular astronomical literature, in many cases not up to date, the author of this work offers a theory which he states to be capable of interpreting all the phenomena presented to us in the heavens. Briefly, we are asked to believe that all interstellar space is filled with attenuated water vapour, and that this vapour is decomposed into its constituents by the electricity generated by the movements of planetary bodies; the oxygen remains on the planets, while the hydrogen goes to maintain the incandescence of the central suns. The author deals very ingeniously with many of the apparent difficulties, such, for example, as the absence of an atmosphere from the moon; but his anxiety to leave nothing unexplained, has occasionally demanded other assumptions, and led to self-contradictions. Thus, in regard to comets, it is necessary to suppose, from the repulsion of the tails, that when they enter our system, they do not behave electrically as planets do, but like suns, and so they should have hydrogen atmospheres; on the other hand, since carbon is assumed to be a "planetary" element (p. 69), they should not contain carbon. This is in complete contradiction with the facts. The author is so much behind the times in spectroscopic matters as to imagine that nebulæ abound in free nitrogen, and possibly oxygen, and that free nitrogen and hydrogen are characteristic of comets. It would serve no good purpose to discuss a theory based on such misconceptions.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The Huxley Memorial.

I trust you will allow me through the medium of your columns to make it known that at the meeting of the Provisional Committee, which was held at the rooms of the Royal Society on Tuesday afternoon, it was announced that a large number of acceptances had already been received to the invitation which was issued a few days ago to a number of gentlemen to serve on the General Committee which it had been decided to form to inaugurate a National Memorial to the late Right Hon. T. H. Huxley, F.R.S.

A list of the Committee will shortly be published.

Owing to the lateness of the season, it has been decided to defer until after the autumn recess the meeting of the General Committee, at which the proposals of the Provisional Committee with regard to the form which the National Memorial shall take may be discussed and decided.

With a view of assisting the Provisional Committee in arriving at some general ideas on the subject, it is suggested that those who propose to contribute to the fund might be willing to inform the Treasurer of the probable amount of their subscriptions.

Subscriptions will be received and acknowledged by advertisement in *The Times* by the Treasurer, Sir John Lubbock.

J. D. Hooker,
Chairman of the Provisional Committee.

July 30.

The Kinetic Theory of Gases.

We shall all agree with Dr. Boltzmann's views as expressed in Nature of July 4, that if in a system of elastic sphere molecules the free paths be very long, and if at the same time the system be of unlimited extent, condition A will always be satisfied. The system will go on till it attains Nirvana in the Maxwell-Boltzmann distribution.

It is only for a finite system that it appeared to me that occasional disturbances from the outside were necessary to produce this result. I agree with Mr. Bryan that contact with the refrigerator or with the reservoir, such as is supposed to take place in thermodynamics, is for this purpose a disturbance.

But it is this very length of free path, and condition A which follows from it, that restricts our kinetic theory to the limiting case of a rare gas.

We have, as I maintain, to abandon condition A altogether if we wish to present our theory in a form applicable to dense media. We must consider, not single spheres, but groups of spheres to begin with. Given that there are at this instant n spheres, and no more within a spherical space S, but nothing is known of their position within S, what is the chance that their component velocities shall at this instant be

$$u_1 \ldots u_1 + du_1 \quad \ldots \quad \ldots \quad \ldots \quad w_n \ldots w_n + dw_n?$$

I assume that chance to be

$$C\epsilon^{-Q} du_1 \ldots dw_n,$$

in which $Q = a\Sigma(u^2 + v^2 + w^2) + b\Sigma\Sigma(uu' + vv' + ww')$, the summation including the n spheres and every pair of them. The coefficient b excludes condition A.

But whatever be the values of a and b, this distribution of velocities remains undisturbed by collisions. And by suitably choosing a and b, we can satisfy all other necessary conditions.

The same thing can be done for two sets of spheres of unequal masses m and m'. In that case we must put Q in the form

$$\begin{aligned} Q = {} & a\Sigma(u^2 + v^2 + w^2) + a'\Sigma(u'^2 + v'^2 + w'^2) \\ & + b\Sigma\Sigma(u_p u_q + v_p v_q + w_p w_q) + b'\Sigma\Sigma(u'_p u'_q + v'_p v'_q + w'_p w'_q) \\ & + \beta\Sigma\Sigma(uu' + vv' + ww'), \end{aligned}$$

in which the accents ′ refer to the m' set, and $\Sigma\Sigma u_p u_q$, &c., means summation over all pairs of spheres m, &c.

Here we have five coefficients, a, b, a', b', β. But the condition for permanence, notwithstanding collisions between m and m', requires

$$2am' - 2a'm + \beta(m' - m) = 0$$

$$b = \frac{m}{m'}\beta \qquad b' = \frac{m'}{m}\beta$$

three conditions reducing the five coefficients to two independent ones. It will be found that $mu^2 = m'u'^2$, as in the ordinary theory.

I doubt not that Boltzmann's minimum theorem can with some modification be applied to this system, at all events if he will take up the theory of dense gases himself.

S. H. Burbury.

On Skew Probability Curves.

In a memoir, entitled "Contributions to the Mathematical Theory of Evolution. II. Skew Variation in Homogeneous Material" (*Phil. Trans.* 186, A, pp. 343–414), and noticed in your columns by Mr. Francis Galton (January 31, 1895), I have dealt with four types of skew frequency curves.

Last Tuesday, Prof. Edgworth drew my attention to the fact that a portion of my results has been anticipated by Mr. E. L. De Forest in vols. vi., ix., and x. of *The Analyst*, an excellent American mathematical journal, the acquaintance of which, I am ashamed to say, I have only to-day made for the first time.

So far as Mr. De Forest's priority is concerned, it covers the special class of curve I have in my memoir termed Type III. He has fully worked out the geometry of this type, and I consider his deduction of it, if somewhat more lengthy than mine, to have the advantage of greater generality. So far as my own memoir is concerned, a knowledge of Mr. De Forest's memoir would not have led me to rewrite pp. 373–6 of mine, which deal with this type, because my discussion there is only a branch of my general treatment of a series of skew frequency curves. I should, however, have referred to Mr. De Forest's priority and the excellency of his work. In particular I should have cited the whole of his numerical table iii. x. p. 69, which gives the values of the frequency in excess and defect of the mode, and the probable errors in excess and defect, for a considerable range of values. These results are only given by algebraic or empirical formulæ in my paper. The statisticians among your readers, who may be proposing to deal with skew frequency, would find a copy of Mr. De Forest's Table III. of considerable service should they come across a curve of Type III.

Karl Pearson.

University College, London, July 24.

Evolution, or Epigenesis?

In the English translation of Prof. Hertwig's book "The Cell," it is stated (p. 295), "When the female gamete of the Alga *Ectocarpus* comes to rest, for a few minutes it becomes receptive. If the egg is not fertilised at this time . . . parthenogenetic germination begins to make its appearance . . . It may be accepted as a *law of nature* (italics mine) for mammals, and for the majority of other organisms, that their male and female sexual cells are absolutely incapable of development by themselves." Thus, what occurs in the lower organisms is no criterion of what occurs in the higher, and *vice versâ*. Then why does Hertwig remark (p. 348), "It is quite sufficient for our purpose to acknowledge, that in the plants and lower animals, all the cells which are derived from the ovum contain *equal quantities of the hereditary mass*. . . . All idioblasts must divide and must be transmitted to the daughter-cells, in *equal proportions both as regards quality and quantity*" (italics mine). According to the above, it is "quite sufficient" for Hertwig's purpose of discrediting Weismann's contention for differentiated distribution of hereditary elements among somatic cells, to show that there is undifferentiated distribution in the case of plants and lower animals. But, reverting to the earlier quotation, if it is not sufficient to prove sexual reproduction in the case of the higher organisms, in order to disprove parthenogenesis in the case of the lower organisms, why should it be "quite sufficient," in order to disprove distribution through germ-cells, in the case of the higher organisms, to show that, in plants and the lower animals one cell contains the same hereditary constituents as another? It is permissible to infer that differentiation in regard to germ-cells, in the higher animals, is no more disproved by the assumed demonstration that, in plants and the lower animals, there is no such differentiation, than that asexuality in lower is disproved by sexuality in higher organisms. Weismann, in my opinion, has proved to rational satisfaction that differentiation of germ from other cells must occur in the higher organisms, and he has offered a rational explanation, conformable with the theory of germ-plasm, of the apparently summational distribution of hereditary elements through somatic cells. Until Weismann's position is seriously undermined, which, so far, is not even a likely contingency, we must decline to accept Hertwig's assumed demonstrations in regard to plants and lower animals as invalidating the theory of germ-plasm. Similarly, that environment may affect the hereditary character of a primitive organism is no more evidence that it may so affect a mammal, than sexuality in the latter is evidence against parthenogenesis in the former. On page 348 we are told: "Johannes Müller has raised the question, 'How does it happen that certain of the cells of the organised body, although they resemble both other cells and the original germ-cell, can produce nothing but their like, *i.e.* cells which are (in-?) capable of developing into the complete organism? Thus epidermal cells can only, by absorbing material, develop new epidermal cells, and cartilage cells only other cartilage cells, but never embryos or buds.' To which he has made answer: 'This may be due to the fact that these cells, even if they possess the power of forming the whole, have, by means of a particular metamorphosis of their substance, become so specialised, that they have entirely lost their germinal properties, as regards the whole organism, and when they become separated from the whole, are unable to lead an independent existence.'" The above is simply a restatement of Weismann's doctrine regarding the origin of germ-cells. All cells which have not, as Müller states, "lost their germinal properties, as regards the whole organism," are Weismann's germ-cells.

So far as regards the essential question of heredity, Hertwig agrees with Weismann. Special units (idioblasts) are the bearers of hereditary qualities. This is "evolution," and no superstructural epigenetic thesis attributing modifying effects by environment, as the cause of a somatic cellular development, can affect the point that differentiation, through hereditary units, is the fundamental condition of morphological development. To accept "hereditary units," in my opinion, excludes "hereditary effect through environment," never mind to what matter-system the latter assumption be applied, whether the systems be, for instance, unicellular organisms or somatic cells. On the other hand, if we accept "hereditary extraneous influence," we need not trouble ourselves with "hereditary units." If "extraneous influences" have hereditary effect, "hereditary units" have no logical existence. All we then need for a theory of heredity are primordial homogeneous matter and environment. Mr. Herbert Spencer's earlier hypothesis, in which he attributed all variation to extraneous influence, would have been logical had he excluded "physiological units." With these, it became illogical. For this reason: if all organic variability depended on the effect of extraneous influences, why should such influences not have produced the differentiations called physiological units? Why should the only logical "unit" not be homogeneous *primordium*? That the conception "hereditary unit" shall be logical, involves that the "unit" shall be as unchangeable as an "atom." If, on the contrary, we have a variable "unit," it is not a genuine "hereditary unit," but merely the equivalent of any later variable "unit." Hertwig's "hereditary units," or "idioblasts" (p. 340), "are the smallest particles of material into which the hereditary mass or idioplasm can be divided, and of which great numbers and various kinds are present in this idioplasm. They are, according to their different composition, the bearers of different properties." They are not indivisible, like atoms, but assimilate food, grow and divide, as do Weismann's "biophors," from which they appear to differ only to the extent that they are complex organisms. The hereditary factor in Weismann's theory which corresponds with these "idioblasts" of Hertwig appears to be the "determinant." All the functions of the latter seem to be performed by the former. These "idioblasts" (p. 343) "must evolve in regular sequence during the process of development." As sentences are formed from words, so are organisms formed from these "idioblasts." We can attain a clear conception of the formation of sentences from words, but Hertwig does not enable us to apprehend how organisms can arise from "idioblasts." As he very truly observes (p. 344), "this portion of the theory is the most difficult to understand."

Hertwig, like Spencer, takes his stand on epigenesis. It may be asked, wherein is the epigenetic character of his (Hertwig's) theory? Unlike Spencer's "physiological units," Hertwig's "idioblasts" are intrinsically differentiated organisms with specific tendencies. Now, for a genuine epigenetic theory, hereditary units must merely compose a plastic mould to take the impress of environment, whereas these "idioblastic" cells are composed of elements with predetermined peculiarities. Accordingly they must function in a predetermined manner, and

thus the products of their activity must issue through evolutionary processes; what they will become after millions of generations must be determined so soon as these "idioblasts" combine as the first cell. If, however, we are to assume that the hereditary qualities of "idioblasts" can be eliminated by environment—as we must assume if we attempt to combine evolution with epigenesis—I reply, as in my earlier proposition, we have no need for "idioblasts" or any other "hereditary unit." All we then need for a theory of heredity is some plastic *primordium* and environment. Then, as such *primordium* would have no hereditary predisposition, there would be no room for predeterminism, and it would remain for ingenious theorists in love with epigenesis and the tape-measure system of estimating the cosmos, to explain the persistence of types under variable environment, and the differentiation of types under identical environment.

I can appreciate the eagerness of the "mechanical school" to welcome any loophole of escape from predeterminism. A genuine epigenetic theory is, no doubt, their great desideratum. If they "won't be happy till they get it," I venture to predict that they are doomed to a lengthened spell of dumps! The main issue raised by Hertwig in "The Cell," is: evolution or epigenesis? He tries to accept both, basing epigenesis on evolution. Thereby, in my opinion, he stultifies both doctrines. All biologists, so far as I am aware, start their theories from the basis of differentiated units. Equally they all evade the attempt to account for the differentiation. This omission I have endeavoured to rectify in "Rhythmic Heredity" (Williams and Norgate).

H. Croft Hiller.

A Sound-producing Insect.

In your issue of June 13, Mr. S. E. Peal speaks of a lepidopterous insect in Assam which makes a tapping noise when flying. His description so closely resembles an insect in Gorakhpur, that I think it must be identical or closely allied.

The alar expanse is about three inches. The wings are broad, not indented, of a very dark chocolate-brown colour on both sides, with one small yellowish-brown blotch on the costa of fore-wing on upper surface. The body is thin, like a butterfly, but the antennæ are not clubbed. It is apparently a Geometer or slender-bodied Bombyx. It flies in the darkest parts of woods, just as twilight is settling into night, and is very hard to see when standing up. By lying down, so as to get the sky as a background, it is easily visible. It cannot be netted in the ordinary way, as the eye cannot follow it, but by standing still till one is heard near, and then striking in the direction of the sound, one may sometimes be successful. I first succeeded in striking one down with my "solah topi"; afterwards I netted two, and brought them home alive, to see how the noise was made. The sound is a sort of clicking, which may be fairly imitated by striking the nails of the thumb and fore-finger together. From the thorax, between the bases of the wings, a stiff bristle (like a pig's) projects about a quarter of an inch. The noise is made by this bristle catching in the hind-margin of the fore-wings and the costal margin of the hind-wings. I fancy it must be of a warning character, as if the insect is eatable, it would help to enable bats and birds to find it. I think I have noticed that the insect is attracted by imitating the clicking sound with the nails, but could not satisfy myself on this point.

J. R. Holt.

A FEW MORE WORDS ON THOMAS HENRY HUXLEY.

TWO scenes in Huxley's life stand out clear and full of meaning, amid my recollections of him, reaching now some forty years back. Both took place at Oxford, both at meetings of the British Association. The first, few witnesses of which now remain, was the memorable discussion on Darwin in 1860. The room was crowded though it was a Saturday, and the meeting was excited. The Bishop had spoken; cheered loudly from time to time during his speech, he sat down amid tumultuous applause, ladies waving their handkerchiefs with great enthusiasm; and in almost dead silence, broken merely by greetings which, coming only from the few who knew, seemed as nothing, Huxley, then well-nigh unknown outside the narrow circle of scientific workers, began his reply. A cheer, chiefly from a knot of young men in the audience, hearty but seeming scant through the fewness of those who gave it, and almost angrily resented by some, welcomed the first point made. Then as, slowly and measuredly at first, more quickly and with more vigour later, stroke followed stroke, the circle of cheers grew wider and yet wider, until the speaker's last words were crowned with an applause falling not far short of, indeed equalling, that which had gone before, an applause hearty and genuine in its recognition that a strong man had arisen among the biologists of England.

The second scene, that of 1894, is still fresh in the minds of all. No one who was present is likely to forget how, when Huxley rose to second the vote of thanks for the presidential address, the whole house burst into a cheering such as had never before been witnessed on any like occasion, a cheering which said, as plainly as such things can say: "This is the faithful servant who has laboured for more than half a century on behalf of science with his face set firmly towards truth, and we want him to know that his labours have not been in vain." Nor is any one likely to forget the few carefully chosen, wise, pregnant words which fell from him when the applause died away. Those two speeches, the one long and polemical, the other brief and judicial, show, taken together, many of the qualities which made Huxley great and strong.

Among those qualities perhaps the most dominant, certainly the most effective as regards his influence on the world, were on the one hand an alertness, a quickness of apprehension, and a clear way of thinking, which, in dealing with a problem, made him dissatisfied with any solution incapable of rigid proof and incisive expression, he seemed always to go about with a halo of clear light immediately around him; and, on the other hand, that power of foreseeing future consequences of immediate action which forms the greater part of what we call sagacity. The former gave him his notable dialectic skill, and mark all his contributions to scientific literature: the latter made him, in addition, an able administrator and a wise counsellor, both within the tents of science and beyond. These at least were his dominant intellectual qualities; but even more powerful were the qualities in him which though allied, we distinguish as moral; and perhaps the greater part of his influence over his fellows was due to the fact that every one who met him saw in him a man bent on following the true and doing the right, swerving aside no tittle, either for the sake of reward or for fear of the enemy, a man whose uttered scorn of what was mean and cowardly was but the reciprocal of his inward love of nobleness and courage.

Bearing in mind his possession of these general qualities, we may find the key to the influence exerted by him on biological science in what he says of himself in his all too short autobiographic sketch, namely, that the bent of his mind was towards mechanical problems, and that it was the force of circumstances which, frustrating his boyish wish to be a mechanical engineer, brought him to the medical profession. Probably the boyish wish was merely the natural outcome of an early feeling that the solution of mechanical problems was congenial to the clear decisive way of thinking, to which I referred above, and which was obviously present even in the boy; and that it was not the subject-matter of mechanical problems, but the mode of treating them which interested him, is shown by the incident recorded by himself, how when he was a mere boy a too zealous attention to a post-mortem examination cost him a long illness. It is clear that the call to solve biologic problems came to him early; it is also clear that the call was a real one; and, as he himself has said, he recognised his calling when, after some years of desultory reading and lonely irregular mental activity, he came under the influence of Wharton Jones at Charing Cross Hospital. That made him a biologist, but con-

firmed the natural aptitude of his mind in making him a biologist who, rejecting all shadowy intangible views, was to direct his energies to problems which seemed capable of clear demonstrable proof. In many respects the biologic problems which lend themselves most readily to demonstrable solutions capable of verification are those which constitute what we call physiology; and if at the time of his youth the way had been open to him, Huxley would probably have become known as a physiologist. But at that time careers for physiologists were of the fewest. His master, Wharton Jones, a physiologist of the first rank, whose work in the first half of this century still remains of classic value, had been driven to earn his bread as an ophthalmic surgeon, and an even greater physiologist, William Bowman, was following the same course. There was no opening in physiology for the young student at Charing Cross, and he was driven by stress of circumstances to morphological rather than to strictly physiological problems; but it was not until long after, when he had achieved eminence as a morphologist, that he finally abandoned his old wish to hold a physiological chair.

Looking back on the past, we may now be glad that circumstances were against his wishes; for (though in every branch of science there is need at all times of a great man) there was at the middle of the century, in the early fifties, a special need in morphology for a man of Huxley's mould. Richard Owen was then dominant, and it is an acknowledged feature of Owen's work that in it there was a sudden leap from most admirable detailed descriptive labour to dubious speculations, based for the most part on, or at least akin to, the philosophy of Oken. Of the "new morphology" in which Johannes Müller was leading the way, and the criteria of which had been furnished by the labours of von Baer, there was then but little in England save, perhaps, what was to be found in the expositions of Carpenter. Of this new morphology, by which this branch of biology was brought into a line with other exact sciences, and the note of which was not to speculate on guiding forces and on the realisation of ideals, but to determine the laws of growth by the careful investigation, as of so many special problems, of what parts of different animals, as shown among other ways by the mode of their development, were really the same or alike, Huxley became at once an apostle. His very first work, that on the Medusæ, wrought out amid the distractions of ship life, written on a lonely vessel ploughing its solitary way amidst almost unknown seas, away from books and the communion of his fellow workers, bears the same marks which characterise his subsequent memoirs; it is the effort of a clear mind striving to see its way through difficult problems, bent on holding fast only to that which could be proved. This is not the occasion to insist in detail on the value of the like morphological work which he produced in the fifties and the sixties, or to show how he applied to other forms of animal life, to echinoderms, to tunicates, to arthropods, to molluscs, and last though not least to vertebrates, the same method of inquiry which guided the work on the Medusæ. Nor need I dwell on the many valuable results which he gained for science by attacking in the same spirit the problems offered by the remains of extinct forms. Moreover, he strengthened the effect of his own labours by admirable expositions of the results of others. Further, unlike his great predecessor who formed no school and had few if any disciples, it was Huxley's delight to hold out his hand to every young man whom he thought could profit by his help, and before many years were over his spirit was moving in the minds of many others. Thus it came about that during the latter half of this century, owing largely to Huxley's own labours and to the influence which he exerted not only in England but abroad, there has been added to science a large body of morphological truths, truths which have been demonstrated and must remain, not mere views and theories which may be washed away.

The excitement of the Darwinian controversy, with its far-reaching issues, has been apt to make us forget how great has been the progress of animal morphology during the past half century. Undoubtedly the solution of special problems touching animal forms, and the great theory of Natural Selection through the Struggle for Existence have been closely bound together: the special learning has furnished support for the general theory, and the general theory, besides strongly stimulating inquiry, has illumined the special problems. But the two stand apart, each on its own basis; and were it possible to wipe out, as with a sponge, everything which Darwin wrote, and which his views have caused to be written, there would still remain a body of science touching animal forms, both recent and extinct, acquired since 1850, of which we may well be proud. In the gaining that knowledge Huxley, as well by his own labours as by his influence over others, stands foremost, Gegenbaur being almost his only peer; and had Huxley done nothing more, his name would live as that of one of the most remarkable biologists of the present century.

As we all know, he did much more; his influence on England and on the world went far beyond that of his purely scientific writings. But when we reflect that a hundred years hence the image of the man as he went to and fro among men, so bright and vivid to-day, will have become dim and colourless, a shadow as it were, and that then the man will be judged mainly by the writings which remain, we must count these writings as the chief basis of his fame. And, though we may think it possible that the world of that day, much that is unwritten having been forgotten, may find it in part difficult to understand how great a power Huxley was in his time, the lapse of years will, we may be sure, in no way lessen, it may be will heighten, the estimate of his contributions to exact science.

As we all know, he did much more. To the public outside science he first became known as the bold, outspoken exponent and advocate of Darwin's views, and indeed to some this is still his chief fame. There is no need here to dwell on this part of his work, and I speak of it now chiefly to remark that the zeal with which he threw himself into this advocacy was merely a part of the larger purpose of his life. Science, or, to use the old phrase of the Royal Society, Natural Knowledge, had a two-fold hold on Huxley. On the one hand he felt deeply all the purely intellectual, and if we may use the word, selfish joys of fruitful progressive inquiry after truth. This was dominant in his early days, and to it we owe the long list of valuable researches, of which I just now spoke, and which followed each other rapidly in the fifties and the sixties. On the other hand, feeling deeply, as he did, his duties as a citizen of the world, science laid hold of him as being the true and sure guide to conduct man in all his ways; and this latter working of science in him, evident even in early days (witness his Address to Working Men at St. Martin's Hall in 1854), grew stronger and stronger as the years went on, until at last it took almost entire possession of him. To him, indeed, it may be said, science was all in all. He saw, as others see, in science a something which is broadening and strengthening human life by unceasingly bending nature to the use of man, and making her resources subservient to his desires; he saw the material usefulness of science, but he saw something more. He saw also, as others see, in science a something in which the human mind, exercising and training itself, makes itself at once nimble and strong, and dwelling on which is raised to broad and high views of the nature of things; he saw in science a means of culture, but he saw something more. He saw in science even as it is, and still more in science as it will be, the sure and trustworthy

guide of man in the dark paths of life. Many a man of science goes, or seems to others to go, through the world ordering his steps by two ways of thinking. When he is dealing with the matters the treatment of which has given him his scientific position, with physical or with biological problems, he thinks in one way; when he is dealing with other matters, those of morals and religion, he thinks in another way; he seems to have two minds, and to pass from the one to the other according to the subject matter. It was not so with Huxley. He could not split himself or the universe into two halves, and treat the one and the other half by two methods radically distinct and in many ways opposed; he applied the one method, which he believed to be the true and fruitful one, to all problems without distinction. And as years came over him, the duty of making this view clear to others grew stronger and stronger. Relinquishing, not without bitter regret, little by little, the calm intellectual joys of the pursuit of narrower morphological problems, he became more and more the apostle of the scientific method, driven to the new career by the force of a pure altruism, not loving science the less but loving man the more. And his work in this respect was a double one; he had to teach his scientific brethren, at least his biologic brethren, the ways of science, and he had to teach the world the works of science. It was this feeling which, on the one hand, led him to devote so much labour to the organisation of biologic science in order that his younger brethren might be helped to walk in the straight path and to do their work well. It was this feeling, on the other hand, which made him urgent in the spread of the teaching of science. It was this, and no vain love of being known, which led him to the platform and the press. The zeal with which he defended the theory of Natural Selection came from his seeing the large issues involved; to him the theory was a great example of the scientific method applied successfully to a problem of more than biologic moment; while the fierceness of his advocacy was a natural expression of resentment on the part of one who saw a scientific conclusion, gained with unstinted pains and large reasoning, judged contemptuously by men who knew nothing of science according to methods in which science had no part.

Science, under this aspect, is a part of what is sometimes called philosophy; and though Huxley felt, in common with others, and felt deeply the pleasures of the intellectual wrestler, struggling with problems which, seemingly solved and thrown to the ground, spring up again at once in unsolved strength, it was not these pleasures alone which led him, especially in his later years, to devote so much time and labour to technical philosophic studies. He hoped out of the depths of philosophy to call witnesses to the value of the scientific method. Indeed, nearly all the work of the latter part of his life, including the last imperfect fragment, written when the hand of disease which was to be the hand of death was already laid upon him, and bearing marks of that hand, was wrought with one desire, namely to show that the only possible solutions of the problems of the universe were such as the scientific method could bring. This was at the bottom of that antagonism to theology which he never attempted to conceal, and the real existence of which no one who wishes to form a true judgment of the man can ignore. He recognised that the only two consistent conceptions of man and the universe were the distinctly theologic one and the scientific one; he put aside as unworthy of serious attention all between. He was convinced that the theologic conception was based on error, and much of his old age was spent in the study of theologic writings whereby he gathered for himself increasing proof that there was no flaw in the judgment which had guided his way from his youth upward. Not only so, but he was no less convinced that, owing to what he believed to be the essential antagonism of the theologic and the scientific methods, the dominance of the former was an obstacle to the progress of the latter. This conviction he freely confessed to be the cause of his hostile attitude; he believed it to be the justification of even his bitter polemics.

But while on the objective side his scientific mode of thought thus made him a never-failing opponent of theologic thought of every kind, a common tie on the subjective side bound him to the heart of the Christian religion. Strong as was his conviction that the moral no less than the material good of man was to be secured by the scientific method alone, strong as was his confidence in the ultimate victory of that method in the war against ignorance and wrong, no less clear was his vision of the limits beyond which science was unable to go. He brought into the current use of to-day the term "agnostic," but the word had to him a deep and solemn meaning. To him "I do not know" was not a mere phrase to be thrown with a light heart at a face of an opponent who asks a hard question; it was reciprocally with the positive teachings of science the guide of his life. Great as he felt science to be, he was well aware that science could never lay its hand, could never touch, even with the tip of its finger, that dream with which our little life is rounded, and that unknown dream was a power as dominant over him as was the might of known science; he carried about with him every day that which he did not know as his guide of life no less to be minded than that which he did know. Future visitors to the burial-place on the northern heights of London, seeing on his tombstone the lines—

> "And if there be no meeting past the grave,
> If all is darkness, silence, yet 't is rest.
> Be not afraid ye waiting hearts that weep,
> For God still 'giveth his belovèd sleep,'
> And if an endless sleep He wills, —so best"—

will recognise that the agnostic man of science had much in common with the man of faith.

There is still much more to say of him, but this is not the place to say it. Let it be enough to add that those who had the happiness to come near him knew that besides science and philosophy there was room in him for yet many other things; they forgot the learned investigator, the wise man of action, and the fearless combatant as they listened to him talking of letters, of pictures, or of music, always wondering which delighted them most, the sure thrust with which he hit the mark whatever it might be, or the brilliant wit which flashed around his stroke. And yet one word more. As an object seen first at a distance changes in aspect to the looker-on who draws nearer and yet more near, features unseen afar off filling up the vision close at hand, so he seemed to change to those who coming nearer and nearer to him gained a happy place within his innermost circle; his incisive thought, his wide knowledge, his sure and prompt judgment, his ready and sharp word, all these shrunk away so as to seem but a small part of him; his greater part, and that which most shaped his life, was seen to be a heart full of love which, clinging round his family and his friends in tenderest devotion, was spread over all his fellow men in kindness guided by justice.

M. FOSTER.

DR. FRIEDRICH TIETJEN.

AT a time when astronomical knowledge is being extended at so rapid a rate, and in so many directions, as has been the case during the last few years, it is natural and right that the highest honour should be paid to those astronomers to whose genius and industry are due discoveries possible on account of original suggestion

or ingenious execution. But at the same time, and on the other hand, there is no small danger that we may fail to give proper recognition to those other astronomers whose lives, unmarked by brilliant achievements, have been devoted to labours which are none the less valuable because they have been accomplished while quietly pursuing recognised lines, and are therefore devoid of conspicuous originality. In particular, the work of computation and arithmetical reduction of observations, without which the observations themselves either cannot be made or must remain almost entirely useless, is apt to fall into disrepute, as being wholly mechanical and unenterprising. This is certainly to be regretted; for just as a victorious general marching forward in the enemy's country must depend for his very safety on the fidelity and capacity of those officers who hold the conquered territory, so our scientific knowledge is liable to become disconnected and fragmentary unless we have capable men ready to perform the task of computing from the observations, and co-ordinating the results achieved in more exciting spheres of scientific work. If the pursuit of such unostentatious work lead to the effacement of the worker, our gratitude should be even all the greater for the self-denial exhibited and practised. Of such a man we have recently had to lament the loss, owing to the sad death of Dr. Tietjen, of Berlin.

Friedrich Tietjen was born in Oldenburg, in the year 1834; we therefore lose his services at the comparatively early age of sixty-one. He studied mathematics and astronomy at Göttingen, and subsequently at Berlin, with which latter city he has been continuously connected. In 1861, he became attached to the staff of the Berlin Observatory, and in one or other capacity this connection remained unbroken till the time of his death. He was appointed Professor of Astronomy in the University of Berlin, and Director of the *Recheninstitut*, allied to the Berlin Observatory. In his earlier career, Dr. Tietjen occupied himself with the observations of comets and asteroids, discovering in this way the asteroid *Semele*. To his activity and devotion the pages of the *Astronomische Nachrichten* abundantly testify. He is also known as the calculator of several cometary orbits, and also of the orbits and ephemerides of many asteroids. Some twelve years later, Dr. Tietjen became superintendent of the *Berliner Astronomisches Jahrbuch*, and his reputation in that capacity is not less assured than that of Dr. Powalky, who had preceded him in that office. As official director he paid great attention to shortening the labour of the necessary calculations as far as possible. Some of his methods have been published, others are not so well known, ill-health having prevented him from giving them to the world. Of the value and of the accuracy of this publication under the superintendence of Dr. Tietjen it is unnecessary to speak here, for it is sufficiently well known. Probably his most useful work was that done in superintending the preparation of the ephemerides of the small planets, the continual and rapid increase in the number of which, while it enormously increased his work, had likewise the effect of lessening the interest in this class of discoveries. While the national almanacks of other countries practically discontinued the publication of this class of ephemerides, Dr. Tietjen loyally struggled to supply sufficient information to ensure the observation of the small planets. Those who have attempted the determination of the mass of Jupiter from the perturbations of these bodies, and similar kinds of work, know how to appreciate the labours of Dr. Tietjen, by which the continuous observation from opposition to opposition has been rendered possible.

This skilled mathematician and remarkably facile computer died at Berlin, on June 21, deeply lamented by his numerous friends, and regretted by many who have profited by the devotion of his quiet unambitious life to the service of astronomy.

THE MAXIM FLYING MACHINE.

On Friday, July 5, a large party of scientific men paid a visit, by invitation of Mr. Hiram Maxim and Mr. Brodrick Cloete, to Baldwyns Park, Bexley, to witness a trial of the celebrated flying machine, and the latest development in the direction of mechanical flight.

The invitations were carefully distributed among those who were competent to judge of the magnitude of the task to be attempted, and who were prepared to examine closely the ingenious mechanical details by which it was clearly demonstrated that the machine had ample power to lift itself off the ground, carrying with it a supply of fuel and water, and a crew for the navigation.

An unscientific crowd of spectators might have become unmanageable, and might have developed iconoclastic tendencies (like the Weser boatmen with Denis Papin's original steam vessel) when the machine did not take to flight immediately and disappear from their astonished gaze.

> "As lewed people demeth comunly
> Of thinges that ben maad more subtilly
> Than they can in her lewednes comprehende
> They demen gladly to the badder ende——"

But the Bexley machine is purposely designed of extreme size, with the intention of thoroughly testing and elaborating the details of the mechanism, and of measuring the lifting power, within immediate reach of a workshop and skilled mechanics, more than of actually taking to the air; this will probably be first attempted with a much smaller machine, capable of lifting one man, of jockey-like proportions, and mounted on a boat on a lake, so that short flights, like those of a flying fish, can be attempted for initial practice.

The lifting force of the machine is measured automatically as it runs along a railway track about half a mile in length, as shown in the accompanying illustration (Fig. 1), and the machine is prevented from taking to flight by wheels running underneath the outer wooden rails, seen in the figure; for much yet remains to be done in the way of practice in vertical steering before taking leave of the earth; the chief difficulties of the Aviator beginning when he wishes to descend and alight on the ground again.

Chaucer did not realise the difficulties of the problem when describing so jauntily the Bronze Horse in the Squieres Tale:—

> "This same stede shall bere yow ever-more
> With-outen harm, til ye be ther yow leste,
> Though that ye slepen on his bak or reste;
> And turne ayeyn, with wrything of a pin."
>
> "But whan yow list to ryden any-where,
> Ye moten trille a pin, stant in his ere—"
> "Bid him descend, and trille another pin,"
> "Trille this pin, and he wol vanishe anon."

The "wrything of a pin" is not inapt in describing the dominating gyrostatic brain of the Aviator, designed by Mr. Maxim to perform the vertical steering automatically.

The Bexley machine, complete with the water, naphtha fuel, and crew of three men on board, weighs 8000 lb.; and running at forty miles an hour with a pressure of 275 lb. per square inch, the engines develop 360-horse power, the thrust of the screws is 2000 lb., and the lifting effect of the aeroplanes and wings, 4000 square feet in area, is 10,000 lb.

A thrust of 2000 pounds at 45 miles an hour gives 240 thrust horse-power; or, with a speed of advance of the screw of 60 miles an hour, 320 indicated horse-power.

The total projected disc area of the screws is 500 square feet, each screw being nearly 18 feet in diameter, with a pitch of 16 feet; and thus requiring 330 revolutions a minute to give a speed of advance of 60 miles an hour.

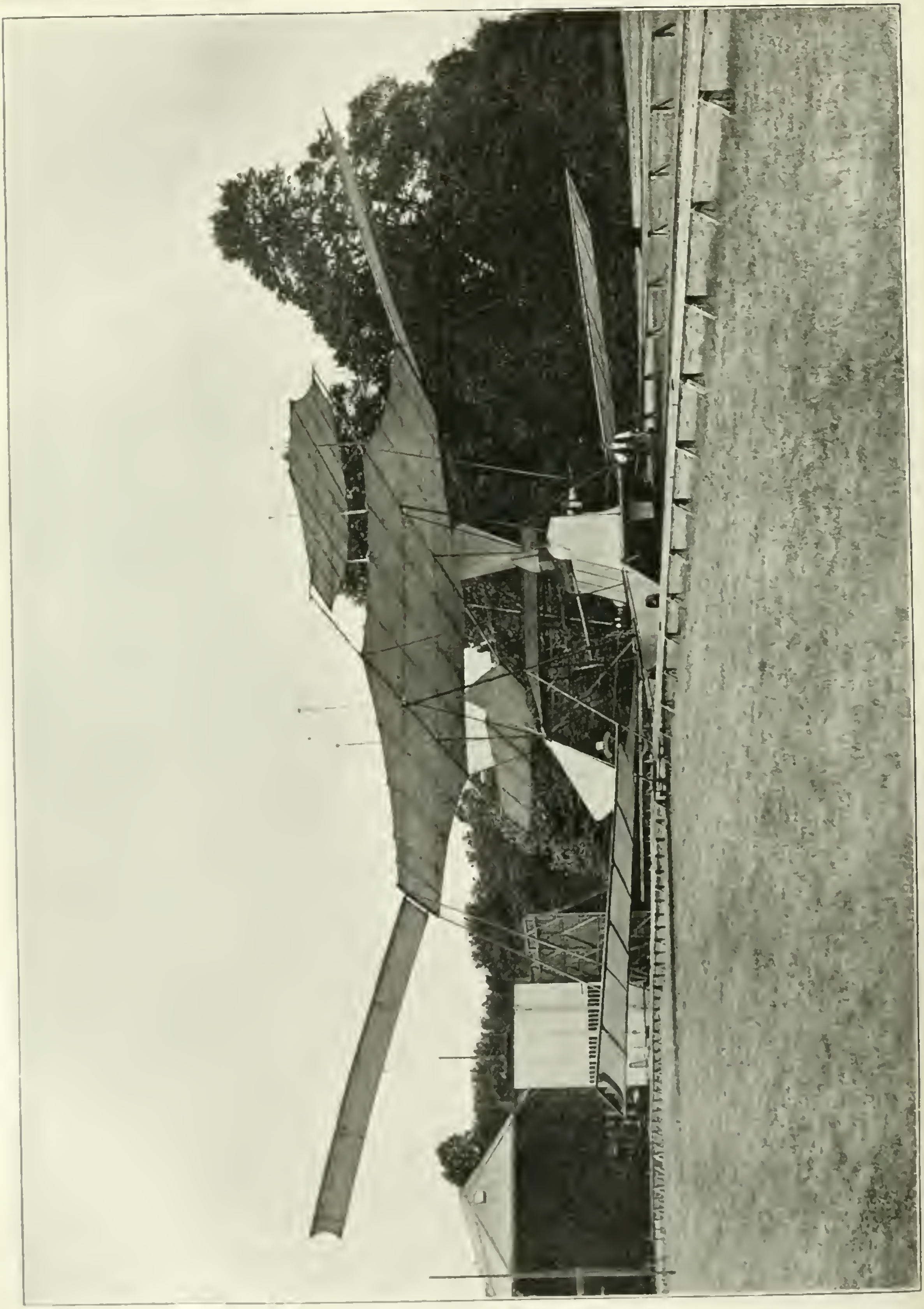


Fig. 1.—General view.

Fig. 2.—One of the screws, showing the connection with the engines.

Mr. Maxim calculates that, after making all allowances, he can at present lift 28 pounds per horse-power; but that, with improvements, he hopes to raise this figure to 50 or 60 pounds, and then a machine could take a flight of 500 or 600 miles.

When the machine is perfected, Mr. Maxim claims that the railway track may be dispensed with; and that a short run over a moderately level field will enable it to attain the velocity necessary to rise. As far as landing is concerned, he says that the aerial navigator will touch the ground while moving forward, and the machine will be brought to rest by sliding on the ground for a short distance. In this manner very little shock should result, whereas if the machine is stopped in the air and allowed to fall directly to the earth without advancing, the shock, though not strong enough to be dangerous (?) to life or limb, might be sufficient to disarrange or injure the machinery.

These numbers are taken from Mr. Maxim's lecture on "Experiments in Aeronautics," before the Society of Arts, November 28, 1894, where a full account of the mechanical details will be found. Each engine is a two-cylinder compound, with the cranks set at 180°; in this way the inertia stresses are self-contained, and racking of the framework is avoided; a similar arrangement is adopted by Mr. Thornycroft in his recent torpedo boats. A photograph showed Mr. Maxim lifting with ease one of these engines, from which 180-horse power can be developed. The boiler is, if possible, a still more wonderful miracle of lightness for its power, weighing only 1000 lb., and providing 360-horse power; the fire is given by a steel burner with 14,000 jets, made from the naphtha vapour delivered from an automatic gas generator. For details the reader must be referred to Mr. Maxim's lecture; but the chief result arrived at may be summarised as a performance of one-horse power for every 11 lb. of weight in the motor complete.

At this rate a 10-horse-power motor can be produced, which will weigh considerably less than an ordinary man; so that when Mr. Maxim can spare a little leisure from this fascinating problem of flight, he can beat easily the performance of the steam carriages recently competing in France, and carry off, we hope, the prize of £1000 offered in this country by the proprietors of the *Engineer*; and some day we may see his motor utilised for purposes of military traction, and galloping round the smartest battery of artillery on Woolwich Common.

Mr. Maxim eschews the gas bag of balloons and the use of vertical screws for securing levitation, and he relies entirely on the upward thrust on the aeroplane and wings, mounted at a slope of about 1 in 8, due to the currents of air rushing past them.

These surfaces are formed of canvas, stretched on a skeleton framework of hollow steel rods for the struts and thin steel wire for the ties; the large central aeroplane is composed of two parallel canvas surfaces, with a space between, and in this way the shape is preserved better, and the general set of the wings, smooth like cardboard, should excite the envy and stimulate the imitation of our sailmakers for yacht racing. The front and rear wings are shown pivoted about a horizontal axis, so as to act as rudders in a vertical plane.

The machine is started from the position in the photograph, being tied up to the indicator post shown in its rear; the propellers are then set in motion, and soon drive a gale of wind in their wake; when the pull of the rope has reached a definite amount, say 2000 lb., a hook is released, and the machine starts on its journey along the track. Mr. Maxim can now carry out his original notion of experimenting with a model machine, tied to a post in a gale of forty miles an hour, to be found every afternoon in the cañons of California, in an artificial gale produced in the wake of his propellers. Dynamometers register simultaneously the thrust of the propellers, so that much interesting information concerning the dynamics of screw propulsion can be obtained here, especially if Mr. Maxim will stretch a wire carrying ribbons across the axes of the propellers, in front and in rear, to measure the direction of the air currents. The speed in air Mr. Maxim deals with is about double the speed of the torpedo boat in water; but the effect of "cavitation" in water, which is beginning to trouble the naval architects, is one which will not concern the propeller working in air.

Now that the main mechanical difficulties of construction have been overcome, a longer track is required for the purpose of practice in vertical steering while the machine is off the ground, but bearing upwards against the outer rails. It is unfortunate that difficulties should have been thrown in the way of making an extension of the present track beyond the domain of Baldwyns Park; so another practice ground, perhaps a sheet of water, must be found, not too far from headquarters or from skilled assistance.

During a short interval of delay, caused by a refractory pump, an adjournment was made to a gravel-pit close by, to witness a performance of the Maxim automatic gun.

Ancient and mediæval mythology is full of references to flying machines, from Dædalus and his son Icarus, and Archytas of Tarentum, to

> "The story of Cambuscan bold
> . . . And of the wondrous horse of brass
> On which the Tartar king did ride"

of Chaucer's Squieres Tale; and to Johnson's "Rasselas," Peter Wilkins, Baron Munchausen, and Auber's opera "le Cheval de Bronze."

"Rasselas," chapter vi., "A Dissertation on the Art of Flying," is so curiously apposite that some extracts may well find a place here.

"Among the artists that had been allured into the Happy Valley, to labour for the accommodation and pleasure of its inhabitants, was a man eminent for his knowledge of the mechanic powers, who had contrived many engines, both for use and recreation." "This artist was sometimes visited by Rasselas, who was pleased with every kind of knowledge, imagining that the time would come when all his acquisitions would be of use to him in the open world. He came one day to amuse himself in his usual manner, and found the master busy in building a sailing chariot. He saw that the design was practicable upon a level surface, and with expressions of great esteem solicited its completion. 'Sir,' said the master, 'you have seen but a small part of what the mechanic arts can perform. I have long been of opinion that instead of the tardy conveyance of ships and chariots, man might use the swifter migration of wings, that the fields of air are open to knowledge, and that only ignorance and idleness need crawl upon the ground." "'The labour of rising from the ground will be great,' said the artist, 'as we see it in the heavier domestic fowls; but as we mount higher the earth's attraction and the body's gravity will be gradually diminished, till we arrive at a region where man shall float in the air without any tendency to fall; no care will then be necessary but to move forward, which the gentlest impulse will effect.' 'Nothing,' replied the artist, 'will ever be attempted if all possible objections must be first overcome. If you will favour my project I will try the first flight at my own hazard. I have considered the structure of all volant animals, and find the folding continuity of the bat's wings most easily accommodated to the human form. Upon this model I will begin my task to-morrow, and in a year expect to tower into the air beyond the malice and pursuit of man.'" "The Prince visited the work from time to time, observed its progress, and remarked many ingenious contrivances to facilitate motion and unite levity with strength. The artist was every day more certain that he should leave vultures and eagles behind him, and the contagion seized upon the Prince. In a

year the wings were finished, and on a morning appointed the maker appeared, furnished for flight, on a little promontory; he waved his pinions awhile to gather air, then leaped from his stand, and in an instant dropped into the lake. His wings, which were of no use in the air, sustained him in the water, and the Prince drew him to land half dead with terror and vexation."

These extracts show that Dr. Johnson had realised to some extent the difficulty of the problem to be solved; although Herr von Lilienthal's experiments, recently attempted by Prof. Fitzgerald, have to a certain extent falsified the universal application of his final catastrophe.

But, viewed with the cold calculating eye of mechanical science, the poetical descriptions are seen to be hopelessly absurd and impossible; now that Mr. Maxim has taken up the subject, and proved to demonstration the enormous power required, out of all proportion to the size, if man is ever to emulate the birds.

A. G. Greenhill.

NOTES.

The Organising Committee of the third International Zoological Congress, to be held at Leyden, September 16-21, has sent us a copy of the provisional programme. The programme contains some details with reference to the work proposed, not given in our previous notes on the forthcoming Congress. At the first general meeting, a discourse will be delivered by Dr. Weismann; Mr. Haviland Field's scheme for bibliographical reform will be reported upon by M. E. L. Bouvier; and a report on the prize instituted in 1892, at the Moscow meeting, will be made by M. Blanchard. At the second general meeting, Prof. Milne Edwards will give a discourse, and Dr. F. E. Schulze will propose the nomination of a commission of three members to draw up, in three languages, the code of zoological nomenclature. Dr. John Murray will address the third general meeting. With regard to the sections: up to the middle of July, the first section had been promised a communication on Weismanism, by M. A. Giard; on cellular theory, by Mr. A. Sedgwick; on Plankton studies, by Prof. Victor Hensen; and a paper by Dr. S. Apathy. Dr. Bowdler Sharpe will address Section II. upon the classification of birds; and there will be papers on the origin of the lacustrine fauna of European Russia, by Prof. N. Zograf (Moscow); on the fauna of Borneo, by J. Buttikofer; and on *Pithecanthropus erectus*, by Dr. E. Dubois. In the third section, Prof. W. Leche (Stockholm) will read an odontological paper, and there will also be papers by Prof. R. Semon (Jena) and Prof. O. C. Marsh. In the fourth section, papers referring to the classification of living and fossil invertebrates, and bionomy, will be read by Dr. V. Salensky, Dr. C. W. Stiles, M. Blanchard, and Prof. S. J. Hickson. The section of entomology has received papers by M. E. de Selys-Lonchamps, Father E. Wasmann, Dr. A. Fritze, and Prof. G. Canestrini. In Section VI., papers on the comparative anatomy and embryology of invertebrates will be read by A. de Korotnev, M. E. Perrier, Prof. J. W. Spengel, and Prof. Herdman. We understand that up to now the following delegates have been officially announced by the respective foreign Governments:—Belgium, Prof. Ed. van Beneden, Prof. Ch. van Bambeke, Prof. Gilson, and Prof. Lameere; France, Prof. Milne Edwards, MM. R. Blanchard, E. Bouvier, A. Certes, J. de Guerne, H. Filhol, Ch. Schlumberger, and L. Vaillant; Great Britain, Sir W. H. Flower, Prof. Sydney J. Hickson, Dr. J. Anderson, Dr. St. George Mivart, and Dr. P. L. Sclater; Sweden, Prof. F. A. Smith; Switzerland, Prof. Th. Studer, and E. Jung; United States (Department of Agriculture), Dr. C. W. Stiles.

A desire is widely felt among the pupils of Prof. Leuckart that the occasion of the fiftieth year of his doctorate should not pass without some durable mark of recognition from those who have known and valued his inspiring influence. It is proposed that the memorial should take the form of a marble bust, and an appeal for contributions is being circulated as widely as possible. There is naturally some difficulty in obtaining the addresses of all old pupils; and it is hoped that those who receive the appeal will make it generally known. Contributions should be sent to Herr Carl Graubner (C. F. Winter's Verlag, Leipzig, Johannes-gasse 8), who has consented to act as treasurer of the memorial fund.

It is proposed to honour Sir Joseph Lister by presenting his portrait to the Royal College of Surgeons for England, to be placed by the side of the portraits of John Hunter and other great surgeons of the past. On Tuesday last, in the presence of a large company, Sir Joseph was presented with a testimonial, in the form of a portrait of himself, subscribed for by his past colleagues and pupils, as a mark of esteem and admiration, on his retirement from the chair of clinical surgery at King's College Hospital.

The sixty-third annual meeting of the British Medical Association was opened on Tuesday, when Dr. E. Long Fox retired from the presidential chair, and Sir J. Russell Reynolds was installed as his successor. Dr. Ward Cousins, in moving the report of the Council, said that when they last met in London, in 1873, they numbered only 1500, whereas now their membership exceeded 16,000. The financial position of the Association is most satisfactory, the assets exceeding the liabilities by more than £60,000. In his opening address, Sir Russell Reynolds dwelt chiefly upon the great advances that have been made, during the past twenty years, in the elucidation of both structure and function—such, for example, as in the researches upon the thyroid, the adrenal bodies, the spleen, and the liver; the advance of bacteriology; the function of the axis-cylinder of nerves; and the development of a new field of therapeutics in the serum-treatment of disease.

The death is announced of Prof. H. Witmeur, Professor of Mineralogy and Geology in the University of Brussels, and of Prof. Josef Loschmidt, at Vienna.

Sir John Tomes, F.R.S., died at Caterham on Monday, at eighty years of age. He was elected into the Royal Society in 1850, after carrying out valuable work referring to dental physiology and surgery. In 1883, with the late Prof. Huxley, he was elected an honorary fellow of the Royal College of Surgeons; and three years later the honour of knighthood was conferred upon him, in recognition of his services to his profession.

We have already noted that an international conference for the protection of birds useful in agriculture, by helping to destroy injurious insects, has recently been held in Paris. Most of the countries in Europe were represented at the conference; and it was agreed that various measures should be taken to preserve useful birds, and to protect their nests and eggs from destruction. A list of birds considered useful has now been published by the Commission, and as this includes a number of our caged friends as well as other birds at present ruthlessly sacrificed for ornamental purposes, the trade in birds in various directions will naturally be curtailed. We learn from the *Revue Scientifique* that a period of three years is to be accorded to the different countries of Europe to allow them to arrange their laws in accordance with the principles agreed upon by the International Commission.

The prospectus is issued of a proposed complete directory of living botanists of all countries, inclusive of the officers of botanic gardens, institutes, and societies, as also of their works and the botanical papers issued by them. Any communication should be made to Herr J. Dörfler, III Barichgasse 36, Vienna, of the botanical section of the Imperial Museum of Natural History.

Mr. F. T. Coville, the honorary curator of the Department of Botany of the United States National Museum, issues an appeal for information on the aboriginal uses of plants by the natives of North America, accompanied by instructions as to the collecting of specimens, and the arrangement of the information under various heads.

We learn from the *Botanical Gazette* that the Division of Vegetable Physiology and Pathology in the United States Department of Agriculture has had under cultivation during the past year over 1000 varieties of wheat and oats. The grains have been collected from nearly all parts of the world, and have been grown chiefly for the purpose of obtaining information upon their rust-resisting qualities. Numerous crosses have been made, and material and facts obtained which will be used in further work.

A valuable memoir on the earthquakes of the Philippine Islands has recently been published by P. Miguel Saderra Masó, the director of the seismic section of the Observatory of Manila. The work consists of 122 quarto pages, and is illustrated by 48 plates, representing the instruments used in the observatory, the disturbed areas and isoseismal lines of sixty-one important earthquakes, and copies of some of the seismographic records, one of them somewhat resembling a bank manager's signature. With a seismological observatory so well equipped as that of Manila, a network of seismic and meteorological stations already established over the country, an energetic and capable director, and numerous shocks, the Philippine Islands promise to become as important a district for studying earthquakes as the neighbouring empire of Japan.

Some beautiful enlargements of phonograph traces are given by Dr. John G. McKendrick in the *Journal of Anatomy and Physiology*, illustrating his paper "On the Tone and Curves of the Phonograph." The accuracy of the phonograph records is strikingly exemplified by the traces of four Koenig tuning-forks, giving 64, 128, 256, and 512 vibrations per second respectively. In each case, the length of indentations is half of that of the previous set, and they are of the same character. Traces of the sounds of a violin, flute, organ, military band, and human voice, singing and speaking, are reproduced. But these traces do not show the exact motion of the vibrating disc. To exhibit this, the phonograph traces were converted into curves by a lever arrangement. The lever ended in a fine point of a hard needle, which translated the up-and-down motion of the reproducing style into a to-and-fro wave motion. To get rid of all disturbing vibrations due to the needle itself, the latter was firmly fixed in a lead block to which the reproducing style was attached, and the phonogram cylinder was turned so slowly that its motion was almost imperceptible to the eye. By this contrivance the uniform curves due to a tuning-fork, the smooth notes of a piccolo, the strong undulations of a bassoon, and the highly over-toned ripples of an old English coach horn were very effectively made visible to the eye.

A recent number of *Modern Medicine and Bacteriological Review* contains an article on Prof. Bunge's important paper on the therapeutic value of iron, read at the German Congress of Internal Medicine last spring. An interesting table is quoted showing the amount of iron found in various food substances. Spinach contains considerably more iron than the yolk of eggs, while the latter, again, is superior in this respect to beef, next in order coming apples, lentils, strawberries, white beans, peas, potatoes, wheat, etc., and almost at the bottom of the list we find cow's milk. That this article of food, of such great importance in infant life, should contain so small a quantity of iron, led Prof. Bunge to conduct a series of experiments on animals, to ascertain in what quantity iron was present in the system of animals of different age. The interesting fact was established that younger animals contain a much greater quantity of iron than adult animals, that the body of a rabbit or a guinea-pig, for example, one hour old, was found to contain more than four times as much iron as that of similar animals two and a half months old. Prof. Bunge is of opinion that a long-continued exclusive milk diet for infants is not advantageous, but should be supplemented by the addition of wheat preparations. Strawberries and apples, however, become invested with fresh attractions by the light of these investigations. The writer of the article suggests that reform is required in the administration of iron, and that the immense quantities of iron in the shape of tonics, which custom prescribes for patients, may very possibly, in a large number of cases, only serve to increase the discomfort of the invalid by the disturbance caused in the digestive functions of the body. In conclusion the hope is expressed that Prof. Bunge's valuable results will "set physicians to thinking more of materia alimentari.e, and less of materia medica"!

The *American Naturalist* for July contains a statement of the advantages offered for scientific study by the Missouri Botanical Garden at St. Louis, and by the Hopkins Seaside Laboratory, situated at Pacific Grove on the coast of California, and maintained by the Leland Stanford Junior University.

Quain's "Elements of Anatomy" (Longmans, Green, and Co.) is now in its tenth edition. The second part of the third volume, which has just been published, comprises the descriptive anatomy of the cerebro-spinal and sympathetic nerves, and their ganglia. It is by Prof. G. D. Thane, who, with Prof. Schäfer, edits the edition.

We have received the first part of a new monthly microscopical journal, the *Zeitschrift fur angewandte Mikroskopie*, edited by G. Marpmann, and published by Thost, of Leipzig. It will be especially concerned with technique and methods. The present number contains papers on a new species of *Scenedesmus*, by P. Richter; on modern imbedding materials, by the editor; on the fixing of spores and pollen in glycerin, by H. Reichelt; and a number of reviews and notes.

The Central Meteorological Institute of Finland has just issued vol. xii. (new series) of its observations for the year 1893. This service is one of the oldest, having been established about 1844, and reorganised, under the superintendence of the Society of Sciences of Finland, in 1882. Among its earlier publications there is a series of eye observations taken at twenty minutes interval, from March 1848 to December 1856, before the establishment of self-registering instruments, a labour which is probably without a parallel. The present volume contains hourly observations for Helsingfors, particular attention being paid to the character and motion of clouds, and to atmospherical electricity.

The eighth volume of the late Prof. Cayley's "Collected Mathematical Papers" has just appeared. The volume contains seventy papers, numbered from 486 to 555, published for the most part in the years 1871–73, and runs into 570 pages. In a prefatory note, Dr. A. R. Forsyth, the editor of this and the remaining volumes, says that Prof. Cayley had himself passed the first thirty-eight sheets for press, and prepared one note. The actual manuscript of this note, which was one of the last of Cayley's writings, is reproduced in fac-simile in the present volume, upon the kind of paper which he regularly used during his mathematical investigations. The remaining papers will appear without notes and references. The long biographical notice of Cayley, contributed by Dr. Forsyth to the *Proceedings* of the Royal Society, is reprinted in the volume just published.

The sixth annual report of the Missouri Botanical Garden bears witness that useful work was accomplished during last year.

In addition to the necessary routine work, several researches were carried out, and the results of some of these investigations are embodied in the report. Mr. M. A. Brannon, who occupied the Garden's table at the Wood's Holl Marine Biological Laboratory, has his studies on *Grinnellia* nearly ready for publication. The Director, Mr. W. Trelease, has made a large collection of the flora of the Azores, and is now working at it. The collection fully represents the flora of those islands, and adds somewhat to what is known of the distribution of species through the group. The papers included in the present report are:—"Revision of the North American Species of Sagittaria and Lophotocarpus," by Mr. J. G. Smith, who also describes a few new or little known species; "*Leitneria Floridana*," by Mr. Trelease. "Studies on the Dissemination and Leaf Reflection of *Yucca aloifolia* and other Species," by Mr. H. J. Webber; and "Notes on the Mound Flora of Atchison County, Missouri," by Mr. B. F. Bush. The report is illustrated by sixty excellent plates.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Captain Fitzgerald; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Florence Cowlard; a Serval (*Felis serval*), a White-necked Stork (*Dissura episcopus*), a Vociferous Sea Eagle (*Haliætus vocifer*), an Antarctic Skua (*Stercorarius antarcticus*) from Mozambique, presented by Mr. W. A. Churchill; a Cardinal Grosbeak (*Cardinalis virginianus*) from North America, a Lazuline Finch (*Guiraca parellina*) from Mexico, presented by Miss E. A. Krumbholz; an Orbicular Horned Lizard (*Phrynosoma orbiculare*) from California, presented by Miss Mabel Baker; a Frilled Lizard (*Chlamydosaurus kingi*) from Roebuck Bay, West Australia, presented by Mr. Saville-Kent; an Orang-outang (*Simia satyrus*, ♀) from Borneo, three Pratincoles (*Glareola pratincola*), European, an Eyed Lizard (*Lacerta ocellata*) from North Africa, a Brazilian Tortoise (*Testudo tabulata*), a Black Tortoise (*Testudo carbonaria*) from Brazil, deposited; two Plumed Ground Doves (*Geophaps plumifera*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Terrestrial Helium.—The discovery by Messrs. Runge and Paschen of the duplicity of the bright yellow line seen in the spectrum of the gas obtained from cleveite, and of its apparent non-coincidence with the solar D_3 line, as announced in Nature of June 6, has naturally led to the re-observation of the solar line.

Mr. Lockyer informs us that on June 14, observing in the fourth order spectrum of a grating having 14,438 lines to the inch, he found the D_3 line in the chromosphere to have a considerable breadth with rather uncertain indications of doubling, while in the spectrum of a prominence the line was much better defined, and was distinctly double, the less refrangible component being the fainter, as in the case of the gas from cleveite.

Writing under date June 25 (*Ast. Nach* 3302), Prof. G. E. Hale gives a preliminary account of the observations he has made with the powerful spectroscope of the Kenwood Observatory. To eliminate the effect of the sun's rotation in displacing the lines, observations were made of the chromosphere at the sun's north and south poles.

On June 19 and 20 the chromospheric line was found to be 0·54 tenth metres broad, the wave-length of the middle being determined as 5875·924. In the spectrum of each of two prominences observed on June 20 and 21, an inconspicuous bright line was detected on the less refrangible side of D_3, both lines being narrow and sharp, and the distance between them being 0·357 tenth metres. The absence of metallic lines, other than H and K, indicated that the fainter line was probably not due to the accidental proximity to D_3 of a faint metallic line. Further observations on June 24 showed that the broad line in the chromosphere was also divisible into two parts, and it became evident that the wave-length of the D_3 line determined on June 19 and 20, as well as that determined by Rowland, must be affected by an error on account of the presence of the faint line on the less refrangible side. So far, Prof. Hale has not succeeded in obtaining a measure of the wave-length of the more refrangible and brighter of the solar D_3 lines, considered as a separate line.

The results so far obtained may be stated as follows:—

λ of solar D_3 line (Rowland)	5875·982
,, ,, ,, (Hale)	5875·924
,, brightest component of terrestrial line (Runge and Paschen)	5875·883
Distance apart of components of terrestrial line (Runge and Paschen)	0·323
Distance apart of components of solar D_3 (Hale)	0·357

The wave-length of the brighter component of the solar D_3 line remains to be determined before the question of the identity of the solar and terrestrial gas can be regarded as completely set at rest.

The announcement that the yellow line of the gas from cleveite was double, also led Dr. Huggins to observe the chromospheric line. In his first attempts he failed to see the line double (*Chemical News*, No. 1855), but he now states that he clearly saw the line to be double on July 10, 11, and 13, the less refrangible line being the fainter, and the distance apart of the lines being about the same as that of the lines in the cleveite gas according to Runge and Paschen (*Ast. Nach.* 3302).

It is worth recalling that Belopolsky observed the solar D_3 line to be double in May 1894, and ascribed the appearance to the superposition of a telluric line upon the bright line. Prof. Hale's observations demonstrate very clearly that Belopolsky's explanation cannot possibly account for the doubling of the line as observed by him.

Ephemeris for Barnard's Comet, 1884 II.—The following search ephemeris for the return of this comet is due to Dr. Berberich (*Ast. Nach.* 3301):—

	R.A. h.	m.	s.	Decl. °	′
Aug. 2	2	23	9	+ 12	29
6	2	29	7	13	19·5
10	2	34	30	14	6·8
14	2	39	16	14	50·9
18	2	43	23	15	31·9
22	2	46	48	16	9·7

The positions are for Berlin midnight, and are computed on the assumption that the comet will pass through perihelion on June 3. On June 30, Swift discovered a nebulous object in R.A. 20°, decl. + 2° 55′, which was missing on July 4, and was thought to be a possible return of the comet for which the ephemeris is given above. Dr. Berberich states that the observation by Swift does not fit closely into the orbit.

The August Meteors.—Shooting stars from various radiants appear during the month of August: but the most important shower is that of the Perseids. These are visible for a considerable period, with a maximum on August 10. According to Mr. Denning, the radiant point exhibits an easterly motion among the stars; on the 10th it is situated in R.A. 45°, decl. 57° N.; on August 2 it is in R.A. 36°, decl. 55°, and on August 16 in R.A. 53°, decl. + 58°. The density of the shower varies but little from year to year, the number of meteors seen by one observer on the morning of August 11 being from sixty to eighty. Unfortunately the moon rises about nine o'clock on the 10th, so that this year only the brighter meteors will be visible.

THE SUN'S PLACE IN NATURE.[1]

IX.

In most of the earlier attempts which were made to explain the origin of new stars, the leading idea was that of a single body being suddenly disturbed in some way, with the possible result that the heat of its interior became manifested at the surface. Thus Zöllner, in 1865, suggested that the phenomena might be

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 255).

produced by the bursting of the crust which had just formed on the surface of a star approaching extinction. Again, in connection with the new star in Corona, I pointed out in 1866 that all that seemed necessary to get such an outburst in our own sun was to increase the power of his convection currents, which we know to be ever at work. Dr. Huggins at that time believed that the appearances were due to gaseous eruptions in a single body, and that "possibly chemical actions between the erupted gases and the outer atmosphere of the star may have contributed to its sudden and transient splendour."

Though Zollner's theory was further advocated by Vogel and Lohse in 1877, the idea that such outbursts can be produced in a single body, without external influence, is now almost universally abandoned.

The alternative hypotheses mostly have to do with the possible action between two bodies—an idea first suggested by Newton—and, as I have already pointed out, the evidence that two bodies were engaged in the case of Nova Aurigæ, at least, is conclusive. Even Dr. Huggins has found it necessary to suppose the existence of two bodies, in order to explain the phenomena observed in this case: and Dr. Vogel, who made some most admirable observations during the appearance of this new star, states most distinctly that we can no longer regard the assumption of a single body as sufficient in any explanation of the occurrence.

Notwithstanding the general agreement as to the presence of at least two bodies in the outburst of Nova Aurigæ, there remain considerable differences of opinion as to the nature of the separate bodies, and of the kind of interaction between them.

One explanation which has been suggested ascribes the luminous effects to the development of heat due to the passage of a dark body through a gaseous mass, somewhat after the manner in which meteoric stones produce the appearances of shooting stars in passing through our atmosphere. This kind of action was first suggested by Mr. Monck in 1885, but the possibilities of such actions have been recently more fully discussed by Prof. Seeliger. He points out that the photographic investigations of Dr. Max Wolf and others leave but little doubt that space is filled with more or less extensive aggregations of thinly-scattered matter, which may be called cosmical clouds, thereby accepting my view of a "meteoritic plenum."

If a heavenly body in rapid motion becomes involved in one of these cosmical clouds, its surface will become heated, and the vapourised products will be partly detached and assume the velocity of the cloud; the fluctuations of brilliancy of a new star on this hypothesis are produced by the varying density of the cosmic cloud through which the body is passing.

This hypothesis of Prof. Seeliger's has been strongly combated by Dr. Vogel.

Another explanation depending upon the action of gases has been suggested by Dr. Huggins:

"The phenomena of the new star scarcely permit us to suppose even a partial collision; though if the bodies were very diffuse, or the approach close enough, there may have been possibly some mutual interpenetration and mingling of the rarer gases near their boundaries."

The idea that the phenomena might be produced by the close approach of two bodies, and the consequent disturbances due to tidal action, was first started by Klinkerfues; it has been recently strongly advocated by Dr. Huggins, though I fail to see how it fits in with his previous explanation.

The tidal theory differs from Zollner's only in ascribing the eruptions to the disturbances produced by tidal action when two bodies approach each other. To employ the words used by Dr. Huggins, the tidal action gives rise to "enormous eruptions of the hotter matter from within, immensely greater, but similar in kind, to solar eruptions." This explanation, however, has met with much opposition on physical grounds.

Thus, Prof. Seeliger writes:

"The [illegible] theory of the tides, which is used throughout, is quite incapable of giving a correct representation of the deformations which are doubtless produced by the close passage of the two bodies; for with very eccentric orbits (which it is necessary to assume on other grounds), the continually varying action would last for so short a time that one could scarcely expect to derive a trustworthy conclusion in regard to the actual circumstances from a consideration based on the forms which the bodies could assume in equilibrium."

Again, Vogel [illegible] that:

"Sensible tidal action cannot be assumed to last for any considerable time, as on account of the great relative velocity of the bodies, they would separate at the rate of forty-six millions of miles per day."

These, however, are not the only objections which may be raised to the idea that we have to do with phenomena of the nature of solar prominences, whether produced by tidal action in the case of two bodies, or by a bursting of the crust which is forming in the case of a star approaching the end of its career as a luminous body. In the first place, there is no reason to suppose that the prominences in our own sun are produced by tidal action. The fact that many of the lines seen in the spectrum of Nova Aurigæ during its first appearance were coincident with lines seen in the solar chromosphere, appears, at first sight, to support the idea, but, since the spectra of nebulæ also show chromospheric lines, the same argument might also be applied to prove that nebulæ are manifestations of prominences. I do not imagine that very many will be prepared to believe that nebulæ are prominences, for if they are, they must be prominences of an unseen sun!!

Mr. Maunder and others have pointed out that if the phenomena be due to the formation of solar prominences, the bright lines should be displaced to the more refrangible sides of their normal places, for the reason that only those prominences on the side of the star presented to us would be able to produce visible bright lines, and such prominences would necessarily have their chief movement in a direction towards the earth. We have seen, however, that in Nova Aurigæ, the actual displacement of the bright lines was just the reverse.

Again, the fact that Nova Aurigæ ended by becoming a nebula is difficult to reconcile with the idea that in its earliest stages its luminosity was produced by outbursts of the nature of solar prominences. Nothing seems more remote than the possibility of prominences cooling down and becoming nebulæ. To have so-called "solar prominences" there must be a sun to produce them, and that must remain when the outburst of prominences has ceased; in this case the last stage of the spectrum of the new star should have resembled that of the sun. The fact that it did not indicates how worthless is the prominence suggestion in the light of modern knowledge.

Another very important objection to the solar prominence theory is this: If new stars are real stars capable of exhibiting prominence phenomena, then we have real stars ending as nebulæ, and thus clashing with the idea now accepted even by Dr. Huggins, that nebulæ are "early evolutionary forms" of heavenly bodies. Further, if new stars be real stars, we should have to believe that the last expiring atmospheres of stars consist of hydrogen and unknown gases; but if we take the evidence afforded by the stars themselves, we find that instead of their last atmosphere consisting of hydrogen it indicates carbon or carbon compounds.

It is evident, therefore, that at present there is no agreement among authorities as to which of the special theories I have brought to your notice is to hold the field, each special hypothesis having got no further than a damaging criticism from the authors of the others.

The remaining general hypothesis we have to consider is that advanced by myself. We have everywhere in space, as is now being revealed to us, especially by the photographs of Barnard, Max Wolf, and others, meteoritic aggregations, swarms, and streams, the constituents of which are, comparatively speaking, at rest, or are all moving one way, if they are moving at all, and undisturbed, because they are not being intersected by other streams or swarms at any one time. But supposing any of these bodies cross each other, as unfortunately sometimes excursion trains cross each other, then there is a very considerable difference in the phenomena; there are collisions, and the collisions produce increased light, and we think that a new star is being born. Nothing of the kind. No new star is being born; there is simply a disturbance in a certain part of space, and when the disturbance cools down we shall find that that part of space is still absolutely in the same order. In the case of Nova Aurigæ, and in the case of Nova Cygni after the war was over, nebulæ have been found to lie in the precise positions occupied by the new stars, and the only thing that one has to say about it is that the nebulæ were there before, but that in consequence of our incomplete survey of the heavens they had not been observed.

After the new photographic chart of the heavens has been made, in future times, it will be found that all new stars are not really new, but the lighting up of something which existed there already. The argument for this theory, you will understand, is

simply this. Suppose I light a match, the smaller the match the sooner will it go out, and similarly the larger a fire the longer will it last. So if you are dealing in space with those illuminations which disappear in hours, days, or weeks, you cannot be dealing with any large mass; therefore the collisions in question cannot be between large masses of matter, but it must be a question of collisions amongst the smallest particles of matter we can conceive.

It is interesting to consider one of the possibilities which may explain why small nebulæ may be overlooked in telescopic observations. In the so-called achromatic telescope, all the rays of light are not brought to quite the same focus, so that when ordinary stellar observations are being made, the focus is adjusted for yellow rays which are most luminous to the eye. Now the greater part of the visual light of a planetary nebula is confined to a single line of the spectrum in the green, so that the focus which is best adapted for observations of stars is not suitable for the observation of a small nebula, the nebula being out of focus, and its feeble light thus reduced by the diffusion of the image. This difference is much more marked in large than small telescopes, and Prof. Campbell has pointed out that a small nebula like Nova Aurigæ will in general appear relatively brighter in a small telescope than a large one.

I will next go into some details touching the phenomena of the Novæ in relation to the hypothesis.

First let us see the crucial phenomena we have to explain. We have (1) the sudden bursting out of light and accompanying spectra; (2) the indication of the existence of two bodies revealed by the spectra; (3) the variations and dimming of the light and accompanying spectral changes; and (4) the final stage giving us the spectrum of a nebula.

Since the new era of spectroscopic work has begun, Nova Aurigæ and Nova Normæ have proved to us that the sudden illumination was, to say the least, associated with two bodies, and that these were in different stages of condensation. On the meteoritic hypothesis it was shown that the main differences between bodies giving bright and dark line spectra is one of condensation only: a sparse swarm gives us bright lines because the number of meteorites in unit volume is small and the interspaces are great; a more condensed swarm gives us dark lines because the number of meteorites in unit volume is greater, and the atmospheres of cooler vapour round each meteorite in collision begins to tell because the interspaces are reduced. I am the more justified in insisting upon the importance of this view that two bodies in different stages of condensation are involved, because years after it was formulated Dr. Huggins apparently arrived at it independently—at all events he makes no reference to my prior announcements when he brings it forward as an explanation of the phenomena.

The following quotations will show how this matter stands:—

"If we assume a brightening of the meteor-swarm due to collisions as the cause of the so-called new stars, we have good grounds for supposing that in these bodies the phenomena should be mixed, for the reason that we should have in one part of the swarm a number of collisions probably of close meteorites, while among the outliers the collisions would be few. We shall, in fact, have in one part the conditions represented in Class III*a*, and in the other such a condition as we get in γ Cassiopeiæ."[1]

"The discussion of the observations which have been made of the changes that take place in the spectra of new stars, has already shown that the sequence of phenomena is strikingly similar to that which occurs in cometary spectra after perihelion passage. In general, however, there will be a difference: namely, that in comets there is usually only one swarm to be considered, whereas in new stars, there are two, which may or may not be equally dense. In new stars, we have accordingly the integration of two spectra, and the spectrum we see will depend upon the densities and relative velocities of the two swarms."[2]

"The spectrum of Nova Aurigæ would suggest that a dense swarm is moving towards the earth with a great velocity, and passing through a sparser swarm, which is receding."[3]

"The circumstance that the receding body emitted bright lines, while the one approaching us gave a continuous spectrum with broad absorption lines similar to a white star, may, perhaps, be accounted for by the two bodies being in different evolutionary stages, and consequently differing in diffuseness and temperature."[4]

[1] November, 1887. Lockyer. *Proc.* R.S., vol. xliii. p. 147.
[2] November, 1890. Lockyer. *Phil. Trans.*, 182 A, p. 407.
[3] February 11, 1892. Lockyer. *Proc.* R.S., vol. l. p. 435.
[4] May 16, 1892. Dr. Huggins. *Proc.* R.S., vol. li. p. 494.

Now two sheets or streams of meteorites interpenetrating and thus causing collisions will produce luminosities which will indicate the condensation of each, and the spectra of the two Novæ we are considering thus indicate that the colliding swarms were of different degrees of condensation, and the variations of light observed indicate several such encounters between less dense swarms after the most dense one had somewhat cooled down. The final stage was arrived at and the pure nebula spectrum produced when the most condensed swarm had ceased to indicate any disturbance, after all the others had returned to their pristine quiet and invisibility.

It is important to insist upon the fact that the nebulæ are now almost generally conceded to represent "early evolutionary forms." We have then from the first appearance of a Nova to the last a "backwardation" in the phenomena ending in an "early evolutionary form." Increase of temperature is accompanied by spectral changes in a certain order: if the temperature is reduced the changes occur in reverse order, until finally we reach the "early evolutionary form," which cannot be a mass of gas because its temperature is lower than that of the sun, which it is potentially, and it must contain all the substances eventually to appear in the atmosphere of the sun.

On the hypothesis, then, we imagine a nebula in the position occupied by Nova Aurigæ not chronicled for the reason stated. This nebula is approaching us. It was disturbed by a much sparser stream leaving us, the relative velocity being over 500 miles a second. During the time of impact, the disturbances produced in the two swarms gave rise to bright-line spectra in the sparse swarm, and to dark-line spectra in the more condensed one. The spectrum of the sparse swarm disappears, the spectrum of the dense swarm changes gradually from dark to bright lines, and ultimately it puts on the original nebula spectrum. It is still there, and still approaching us.

We have next to consider the objections which have been urged against this hypothesis. They are of a most trivial nature. An objection made by Vogel is that it is improbable that the velocities could have been so great after collisions. The reply is easy. The light was produced by the disturbed members of the two swarms which escaped end-on collision. On the meteoritic hypothesis we can escape from the difficulties produced by the old idea of collisions *en bloc*. Such objectors would urge that the velocity of a comet as a whole would be retarded by passing through the sun's corona, but we have instances to the contrary.

Another objection has been raised by Dr. Vogel because in relation to the Nova I did not restate all I had previously written concerning the origin of the cause of bright and dark line spectra in stars. It has been difficult for him to understand how one (temporary) star should have bright lines in its spectrum, and another (temporary) star should have dark lines. All I can say is that upon such objectors lies the onus of producing a more simple (and yet sufficient) explanation than that I have suggested.[1]

J. NORMAN LOCKYER.

(*To be continued.*)

THE INTERNATIONAL GEOGRAPHICAL CONGRESS.

THE International Geographical Congress, now a recognised institution, has this year met for the first time on British ground. Originating in a festival organised to celebrate the inauguration of statues of Mercator and Ortelius at Antwerp and Rupelmond, the first Congress was held at Antwerp in August

[1] It has been stated that the meteoritic hypothesis has received a fatal blow from the observations of the Nova (*Astronomy and Astrophysics*, 1892, p. 509). Capable and unprejudiced persons I think will not be of this opinion. I append a quotation from an article by Prof. Campbell, which has appeared since the lectures were delivered.

"As bearing upon any possible theory of Nova Aurigæ, perhaps it will not be out of place to say here what I said last winter in another journal (Pub. A.S P. vi., 52, 133.) The Harvard College Observatory has shown that both Nova Aurigæ and Nova Normæ at discovery possessed substantially identical spectra of bright and dark lines, similarly and equally displaced. Both diminished in brightness, and both assumed the nebular type of spectrum. The new star of 1876 in Cygnus probably had nearly an identical history: passing from a bright star with a spectrum of bright and dark lines, to a faint object with a spectrum consisting of one bright line (undoubtedly the nebular line λ 5010, or the two nebular lines λ 5010 and λ 4960 combined). We may say that only five 'new stars' have been discovered since the application of the spectroscope to astronomical investigations, and that three of these have had substantially identical spectroscopic histories. This is a remarkable fact. We cannot say what the full significance of this fact is. One result, however, is very clear: the *special* theories propounded by various spectroscopists to account for the phenomena observed in Nova Aurigæ must unquestionably give way to the more *general* theories. (*Astrophysical Journal*, Jan. 1895, p. 51.)

1871, under the name of the "Congrès des Sciences géographiques, cosmographiques, et commerciales," and under the influence of the revival of geographical learning subsequent to the Franco-German War, it has met from time to time at different centres, gaining strength and vitality on each occasion. The second Congress assembled at Paris in 1875; the third at Venice in 1881; the fourth at Paris in connection with the Great Exhibition of 1889; and the fifth at Berne in 1891. In each case the representative Geographical Society of the country concerned was responsible for the organisation and arrangement of the meeting, and at Berne it was definitely resolved that in future the Congress should be constituted at intervals of not less than three, nor more than five years, the resolution taking practical shape in the acceptance by the Royal Geographical Society of the responsibilities of a meeting in London in 1895. A proposal, emanating from the Berne Geographical Society, to the effect that the chief officials of each Congress shall retain office until the meeting of the next, is to be submitted this year, and its acceptance would mark a further step towards the establishment of a great permanent organisation for the systematic study and exploration of the globe.

The sixth Congress differs from its predecessors in a characteristically British fashion, inasmuch as it is practically a private enterprise; no State or municipal aid being forthcoming, as on previous occasions. Nevertheless the Royal Geographical Society, aided by grants from a few of the City companies and by private generosity, has been able fully to cope with the demands made on its resources by the immense influx of geographers from all parts of the world. Accommodation has been found in the Imperial Institute, which affords ample room for private and public business meetings, for exhibitions, and for all manner of social functions, as well as opportunity for that private intercourse which goes so far to enhance the value of such meetings. The Congress is under the patronage of the Queen and the Prince of Wales, and the honorary presidency of the King of the Belgians, the Duke of Connaught, the Duke of York, the Crown Prince of Denmark, and the Grand Duke Nicolas Michailovich. The President is, according to the custom of the Congress, the President of the Geographical Society under whose auspices it meets: in this case the President of the Royal Geographical Society, Mr. Clements R. Markham, C.B., F.R.S. A large number of eminent public men and geographers have accepted the position of honorary vice-presidents.

The work of organisation has been carried out by a number of committees, under the chairmanship of Major L. Darwin, R.E.: the general secretaryship is in the hands of Mr. J. Scott Keltie and Dr. H. R. Mill; and the exhibition is under the direction of Mr. E. G. Ravenstein, Mr. John Coles, and Mr. John Thomson.

In devising the general arrangements, it has hitherto been the practice to abstain from formulating any rigorous rules, and to leave the managing Society a pretty free hand. In some cases, notably at Venice, the Congress was somewhat overwhelmed by the exhibition of geographical objects; while in others undue subdivision into sections has tended to defeat one of the most praiseworthy objects of the meeting. Profiting by the experience obtained, the Royal Geographical Society has kept the range of the exhibition within comparatively narrow limits. The Geographical Societies of Paris, Berlin, and St. Petersburg, and various Government departments and private individuals in all parts of the globe have sent representative exhibits of recent work, and the collections have been in many cases arranged entirely by the exhibitors. Another department is devoted to paintings and photographs of geographical interest, including, amongst other things, a series of historical portraits of eminent travellers, cartographers, and geographical writers, many valuable sketches and photographs contributed by explorers, and lantern slides and diagrams adapted to the purposes of geographical education. A third section, due to Mr. E. G. Ravenstein, consists of a loan exhibition, intended to illustrate the development of cartography from the time of Ptolemy to the end of the eighteenth century. Mr. Ravenstein is to be congratulated on the achievement of a remarkable success, for while no important stage of progress is unrepresented, those illustrated by facsimiles only are wonderfully few. The collection includes many priceless examples, such as the Leonardo da Vinci maps belonging to the Queen, the "Henry II." map belonging to the Earl of Crawford and Balcarres, the Mollineux globe from the library of the Middle Temple, the Agas map of London from the Guildhall, the manuscripts of the early Indian surveys by Pitcher and Rennel, Topping, Maclure, and Mackenzie, from the India Office, and extensive contributions from the libraries at Lambeth Palace, the Admiralty, the Ordnance Survey, various Geographical Societies, and the private collections of Mr. S. W. Silver, Mr. H. Yates Thompson, Mr. E. A. Petherick, and many others. It is to be noted that the catalogue of this exhibition, with its appended list of maps, portolani, and atlases in the British Museum, forms an excellent bibliographical outline of the subject.

A similar collection, though on a necessarily smaller scale, has been made by Mr. John Coles, in the department of surveying and meteorological instruments. The exhibits of the Hydrographic Department of the Admiralty and the Ordnance Survey Office are of great historical interest. We could have wished it had been possible to allot a further space to instruments used in deep sea explorations, especially as their modern developments are so well illustrated by Prof. Otto Pettersson and Dr. H. R. Mill.

A final section of the exhibition consists of the most recent equipments for exploration, surveying, mapping, and teaching geography, shown by numerous private firms.

The same leading idea, that of representing general features, has been kept in view in arranging the work of the meetings. While no attempt has been made to present popular programmes, the whole range of geography has been covered, and the chief effort directed towards furthering those larger interests which concern all geographers, rather than to the discussion of more minute technicalities, however important in themselves. Thus general meetings are to be devoted to Polar Exploration, the development of Africa, Exploration, and Cartography; and sectional meetings deal with Geographical Education, Photographic Surveying, Physical Geography, Geodesy, Oceanography, Geographical Orthography and Definitions, and Limnology.

The date of our going to press constrains us to defer a report of most of the work done in all these different departments until next week, except in so far as the earlier meetings are concerned. On Friday evening (July 26) the delegates were presented to H.R.H. the Duke of York by the Ambassador or Chargé d'Affaires of their respective countries. The following were represented, either by Government delegates or by delegates of Geographical Societies:—Austria-Hungary, Belgium, Denmark, France, Germany, Greece, Italy, Netherlands, Norway, Portugal, Roumania, Russia, Spain, Sweden, Switzerland, Turkey, United States, Mexico, Brazil, Japan, Persia, New South Wales, New Zealand, Queensland, South Australia, Tasmania, Victoria, Western Australia, Cape of Good Hope, and the United Kingdom. After the private reception, the Duke of York welcomed the whole Congress in the name of the Queen and the Prince of Wales, and the President made a brief address of welcome on behalf of the Royal Geographical Society, the other British Geographical Societies, and the Geographers of the United Kingdom. The Hon. Chief Justice Daly, of the New York Geographical Society, the oldest President of a Geographical Society living, replied on behalf of the foreign members and delegates, and the meeting adjourned, the remainder of the evening being spent in the gardens of the Institute, where music was discoursed by Strauss' orchestra.

On Saturday (July 27) the Congress assembled at 10 a.m. to hear the President's opening address, which paid a graceful tribute to the geographical work of the nations whose delegates and representatives he cordially welcomed, and gave a forecast of the work about to be undertaken by the Congress. A vote of thanks was proposed by Prince Roland Bonaparte, and seconded by Prof. von den Steinen. At noon two sections were formed. In Section B, which was presided over by Mr. Markham, supported by Chief Justice Daly and Prof. von den Steinen, Prof. Levasseur read a paper on geography in schools and universities, which outlined a system of geographical education extending through primary, secondary, and higher stages. Señor Torres Campos supported the views expressed by Prof. Levasseur, and discussion was continued by M. Ludovic Drapeyron. The importance of a university training for teachers of geography was urged by Dr. R. Lehmann in the second paper, and the needs of geography in secondary education were set forth by Mr. A. J. Herbertson in the third. Thereafter Dr. W. Henkel allowed a paper on geography and history in schools, standing in his name, to be held as read, in order to allow time for discussion. Mr. H. J. Mackinder advocated the establishment of a central school of geography in London, in order to place geographical teaching in this country on a proper footing. Mr.

G. N. Hooper referred to the work done by the London Chamber of Commerce, and the discussion was continued by Messrs. Phillips, Burgess, Batalha Reis, and Yule Oldham. The President proposed that a committee, consisting of Chief Justice Daly (chairman), Prof. Levasseur, Prof. Lehmann, Mr. Mackinder, and Mr. Herbertson, should be appointed to consider a resolution on geographical education, to be submitted to the Congress.

Section C, which met at the same time, concerned itself with photographic surveying. The presidential chair was occupied by Prince Roland Bonaparte and General Walker jointly. In a paper read on his behalf by M. Schrader, Colonel Laussedat considered the application of photography to the rapid determination of points in levelling, and a combined camera and theodolite was exhibited. M. de Déchy, in discussion, insisted that photography must always be merely auxiliary to triangulation, and must not in any way replace it; and Mr. Coles described his work in constructing a map of the Caucasus from photographs alone. Captain E. H. Hills then read a paper on the determination of terrestrial longitudes by means of photography, in which he described improved methods of exposing and measuring plates used in photographing lunar distances, by means of which he had obtained better results than those obtained by Schlichter and Runge. An abstract of a paper by Prof. J. Thoulet, suggesting the extended application of photography to the survey of rapidly shifting sandbanks, was read in his absence. Mr. Coles described and exhibited Colonel Stewart's camera for producing photographs of the whole horizon, and the proceedings closed with an informal communication by M. Janet on the determination of longitudes without instruments of precision.

HELIUM, A CONSTITUENT OF CERTAIN MINERALS.[1]

II.

(II.) *The Properties of Helium.*

FROM what has preceded, it appears that up to now only three minerals are available as sources of helium, unless, indeed, very large quantities of samarskite and yttrotantalite are worked up. These three are clèveite, the uraninite investigated by Hillebrand, and bröggerite. And here we wish to express our indebtedness to Prof. Brögger for his great kindness in placing a large stock of bröggerite at our disposal. It has furnished a large quantity of the helium which we have had in our hands.

Although, so far as we were able to judge by throwing into a two-prism spectroscope of Browning's the spectra of samples of gases obtained from the minerals previously mentioned, all the specimens of helium were identical, still a further proof was desirable. Owing to the small quantities of gas yielded by these minerals, amounting in most cases to a few c.c., it was impossible to ascertain whether these samples were of the same density; but the case was different with the gas from clèveite and from bröggerite. In each case a sufficient quantity was obtained to make it possible to determine the density with fair accuracy. It will be convenient therefore to describe the methods of extracting the gas and the methods determining its density.

In the communication to the Royal Society it was stated that the maximum density of the original gas from cleveite was 3·89. The spectroscope showed the presence of nitrogen in this sample; the bands were very brilliant at high pressure, but on reducing the pressure the yellow line became brilliant, and the nitrogen spectrum disappeared. This always happens when the tube has platinum electrodes and a strong discharge is passed for a considerable time. An attempt was made to remove the nitrogen from this sample of gas by circulating it over red-hot magnesium; but an unfortunate accident caused the admixture of about its own volume of air, carrying with it argon, from which at present there is no known method of separating helium.

It appeared important to decide whether the gas evolved from these minerals is helium, or a compound of hydrogen and helium; for in the preliminary set of experiments the treatment was such that a hydride would have been decomposed either by sparking with oxygen or by passage over copper oxide at a red heat.

[1] A paper by Prof. William Ramsay, F.R.S., Dr. J. Norman Collie, and Mr. Morris Travers, read before the Chemical Society on June 20. (Continued from p. 308.)

The result of experiments directed to this end is to show that no combined hydrogen is present. Gas was extracted from nineteen grams of bröggerite by heating it in a combustion-tube to dull redness; the combustion-tube was connected with a Töppler's pump by means of thick-walled india-rubber tubing, wired carefully. Special experiments showed that the leakage through the india-rubber amounted between Saturday and Monday to less than one small bubble. The bröggerite yielded about 75 c.c. of gas, a large portion of which was absorbed by caustic soda, leaving about 35 c.c. A second charge of 18·3 grams gave 58·5 c.c., and a third, of 22·1 grams, gave 66·0 c.c. The amount of gas evolved depends largely on the temperature. The evolution is rapid at first, but becomes very slow after three hours, and the heating was always stopped before all the gas which might have been extracted had come off. The last portions, as will be seen later, were extracted by fusion with hydrogen potassium sulphate.

This crude product from bröggerite blackened mercury, doubtless owing to the presence of hydrogen sulphide.

The density of this sample was determined: the data are these.

Volume of bulb	33·023 c.c.
Temperarure	22·9
Pressure (corr.)	766·7 mm.
Weight...	0·0327 gram
Density (O = 16)	11·90

The exceedingly small capacity of the bulb calls for some remark, but for no apology. The object here is, not to determine the density with the utmost accuracy, but to secure a guide, sufficient for our purpose, which will indicate the probable molecular weight. Now the hydrogen contained in such a bulb at 0° and 760 mm. weighs approximately 0·0030 gram. A sensitive balance by Oertling, adjusted for the special purpose, could easily be read to 0·00005 gram, without resorting to the the reading of oscillations of the pointer; and this gives an accuracy of 5 parts in 300, or 1·7 per cent. Hence the density of hydrogen, thus determined, might vary between 0·983 and 1·017. It is evident that such an approximation is quite sufficient for our present purpose. The total volume of this gas was 124·5 c.c. A solution of soda was introduced by means of a pipette, and after all absorption had ceased, the residue measured 78·0 c.c. The density was again determined.

Volume of bulb	33·023 c.c.
Temperature	21·6°
Pressure (corr.)	765·4 mm.
Weight	0·0058 gram
Density (O = 16)	2·105

This gas was now left in contact with palladium sponge for a night. The sponge was made by reducing the chloride in a current of hydrogen, at a dull red heat. As it was somewhat porous, it was hammered on a steel anvil before introducing it into the gas, which, of course, was confined over mercury. The contraction amounted to abont 1/30th. The density was again taken.

Volume of bulb	33·023 c.c.
Temperature	19·2°
Pressure (corr.)	760·2 mm.
Weight	0·00630 gram
Density (O = 16)	2·284

This gas had undergone no treatment which was of a kind to remove combined hydrogen, unless, indeed a very improbable assumption—it be supposed that the compound should be decomposed by contact with metallic palladium. The gas was therefore placed in contact with copper oxide, which had previously been heated to redness in a vacuum, and a tube filled with phosphoric anhydride was so interposed as to absorb any water produced. The gain in weight of this tube was 0·0016 gram, indicating the oxidation of about 2 c.c. of hydrogen. In all probability this hydrogen had remained over after treatment with palladium; for it bears no proportion to the total quantity of gas—78 c.c.

The density was again determined.

Volume of bulb	33·023 c.c.
Temperature	16·67°
Pressure (corr.)	754·9 mm.
Weight	0·00720 gram
Density (O = 16)	2·606

We give thus minutely all the determination of density of such samples, because, although they refer to an imperfectly purified sample, yet they show that the density is very low, and they trace, moreover, the gradual change as one ingredient after another is removed.

The bröggerite which had been heated in a vacuum was next fused in successive portions with hydrogen potassium sulphate. A large quantity of gas was evolved, consisting of sulphur dioxide, carbon dioxide, nitrogen, and helium. The sulphur dioxide was removed with chromic mixture, and the carbon dioxide with caustic soda: the yield was 45 c.c. The density was then determined.

Volume of bulb	33·023 c.c.
Temperature	16·18°
Pressure (corr.)	753·3 mm.
Weight	0·01035 gram
Density (O = 16)	3·748

No alteration in volume occurred on passing the gas for several hours over red-hot copper oxide. Hence no hydrogen was present in the free state; and if combined, passage over copper oxide does not decompose the hydride, as was seen before, when the water produced was weighed. It may be remarked that every known hydride would yield its hydrogen on such treatment.

This sample of gas was next circulated over red-hot magnesium for several hours. It is hardly necessary to state that the magnesium was first heated to redness in a vacuum so as to remove hydrogen. In case any should escape removal, however, a red-hot tube of copper oxide formed part of the circuit, as well as a tube filled with phosphoric anhydride. Some caustic soda solution was present in the reservoir above the mercury, which would have absorbed the products of combustion of any hydrocarbon present. The density of this gas was calculated from the data appended.

Volume of bulb	33·023 c.c.
Temperature	14·88
Pressure (corr.)	756·0 mm.
Weight	0·00845 gram
Density (O = 16)	3·037

On examining the magnesium tube, after it had cooled, it was found that on moistening it ammonia was evolved. The gas was, therefore, again circulated over magnesium, at a somewhat higher temperature, so high, indeed, that the gas must have passed repeatedly through magnesium vapour. On pumping out the tubes, an accident led to the loss of a few c.c. of gas; hence the weighing bulb had to be filled at a somewhat reduced pressure. The density is given below.

Volume of bulb	33·023 c.c.
Temperature	18·33
Pressure (corr.)	615·8 mm.
Weight	0·0049 gram
Density (O = 16)	2·187

Again, on moistening the broken magnesium tube, ammonia was evolved; it was recognised by its odour and by its turning red litmus paper blue.

A further experiment was made with bröggerite. 30·8 grams were heated in a vacuum and the gas was collected over mercury, on to the surface of which a few c.c. of caustic soda solution were introduced. The yield of gas was 65 c.c. It was circulated over copper oxide to remove hydrogen, and its density was then determined.

Volume of bulb	33·023 c.c.
Temperature	19·70
Pressure (corr.)	756·7 mm.
Weight	0·0068 gram
Density	2·481

The density of this sample is almost coincident with that of a previous sample, [illegible], obtained in the same way, after it had been purified from hydrogen. This gas was next circulated over very hot magnesium, so as to remove nitrogen. Again, it is certain that for many hours the gas must have been mixed with magnesium vapour, for the magnesium had been completely volatilised out of the hot part of the combustion-tube, and had condensed at the cool end. Again, the product, when moistened showed the reactions of ammonia, proving that nitrogen had been removed. The density of this sample was next taken.

Volume of bulb	33·023 cc.
Temperature	19·17
Pressure (corr.)	756·7 mm.
Weight	0·0056 gram
Density	2·044

The copper oxide tube was omitted during this circulation; hence the density was low, 2·044. The spectrum of this gas showed hydrogen lines and feeble nitrogen bands. A second determination of density, in which the bulb was freshly filled, gave, at the same pressure and at a temperature differing by only 1 from the previous one, an identical weight. Further circulation for a whole day over red-hot magnesium, raised to the highest temperature which the tube could stand, gave a specimen from which hydrogen and nitrogen were absent: at least, the barest trace was visible in a vacuum-tube filled at a fairly high pressure; and care was taken to interpose a red-hot copper oxide tube, and, as usual, a tube containing phosphorus pentoxide. The effect of this circulation was to raise the density.

Volume of bulb	33·023 c.c.
Temperature	17·1
Pressure (corr.)	763·2 mm.
Weight	0·0060 gram
Density (O = 16)	2·152

It is of interest to note that this sample, procured by heating bröggerite in a vacuum, has a density practically identical with that of gas obtained by fusing bröggerite with hydrogen potassium sulphate: that sample had density 2·187.

We next proceeded to extract the gas from 6·96 grams of Swedish clèveite. When heated in a vacuum, the gas was rapidly evolved at first, more quickly than from bröggerite. About 60 c.c. were obtained, and, after treatment with soda, the residue occupied 26·3 c.c. As this was not sufficient for our purpose, and as we had already by density and spectrum proved the identity of gas evolved from bröggerite on heating, and on fusion with acid sulphate, the remaining clèveite was mixed with about five times its weight of fused and dried hydrogen potassium sulphate, placed in a tube, and heated in a vacuum. A further quantity of gas was evolved, which was at once treated with caustic soda solution. Both quantities of gas were mixed. This sample was then circulated over copper oxide for several hours, and the density was then determined with the following result.

Volume of bulb	33·023 c.c.
Temperature..	19·43°
Pressure (corr.)	763·2 mm.
Weight	0·0061 gram
Density	2·205

The spectrum of this gas showed the merest trace of nitrogen, but no hydrogen. The density, it will be seen, is practically coincident with that of the gas from bröggerite. It is noteworthy that the gas from clèveite contains no nitrogen. We are absolutely certain that the presence of nitrogen in the gas from bröggerite is not to be explained by leakage of air, for the tightness of the apparatus was frequently tested during each operation.

We have therefore three determinations of density, and the mean may be taken as approximately correct to within 0·05. They are:

Gas from bröggerite by heating	2·152
Gas from bröggerite with $HKSO_4$ ...	2·187
Gas from clèveite	2·205
Mean	2·181

All these samples of gas were now mixed and passed through the usual absorbents for nitrogen and for hydrogen, namely magnesium, copper oxide, soda-lime, and phosphoric anhydride. The density of this sample was then determined with the larger bulb. The error due to error in weighing cannot in this case amount to more than 0·3 per cent., and is probably less. Of course, the purity of the gas would affect the result. The data are as follows.

Volume of bulb	162·843 c.c.
Temperature...	17·07°
Pressure (corr.)	704·9 mm.
Weight	0·03057 gram
Density (O = 16)	2·218

The wave-length of sound was determined with this sample of gas in a tube 1 metre in length and 9 mm. internal diameter; the vibrating rod was 580 mm. long. We found it exceedingly difficult to procure a tube in which really good sound-waves could be shown with helium; indeed, we were on several occasions nearly despairing of gaining our object. But at last perfect waves, easily read and easily counted, were produced, and measurements were taken with the following results.

Series	I.	II.	III.	IV.	V.	VI.	VII.
Half wave-length	98·6	98·6	97·6	98·3	100·0	98·6	97·9 mm.

Mean of all, 98·8 mm. at 18·9.

In air, a similar series gave the numbers

Series	I.	II.	III.	IV.	V.
Half wave-length	36·00	36·03	36·11	35·89	36·16

Mean, 36·04 mm. at 20·1°

The ratio of the specific heat at constant volume to that at constant pressure for air is 1·408; that for helium is—

$$\frac{(36{\cdot}04)^2 \times (273 + 18{\cdot}9)}{273 + 20{\cdot}1} \times 14{\cdot}479 : (98{\cdot}8)^2 \times 2{\cdot}218 :: 1{\cdot}408 : 1{\cdot}632$$

This sample of gas was again circulated over very hot magnesium and copper oxide for seven hours; the magnesium had no smell of ammonia when breathed on, nor did it turn red litmus paper blue until after long standing. The magnesium was mostly volatilised out of the hot part of the tube.

The *density* of this sample of gas was determined.

Volume of bulb	162·843 c.c.
Temperature	19·8°
Pressure (corr.)	730·0 mm.
Weight	0·0278 gram
Density	2·133

The wave-length of sound was re-determined in the same tube as before. The figures are

Series	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
Half wave-length.	102·7	100·7	101·6	100·7	102·6	101·6	100·9	101·1 mm.

Mean of all, 101·5 mm.

The ratio of the specific heats of helium, calculated from these numbers as before, is 1·652, a sufficiently close approximation to the theoretical number 1·66. In the case of argon, the purest specimen obtained gave for the ratio 1·659; and as remarked (in the *Philosophical Transactions*, 1895, 52), not much dependence can be placed on the accuracy of the last figure.

The result of these experiments goes to prove that the density of the gas named helium is not less than 2·13, and that it has the same claim to be considered a monatomic gas as mercury gas; or if it is a mixture, it must be a mixture of monatomic gases.

As hydrogen was often evolved along with helium from minerals, it occurred to us that if a definite ratio could be found between the helium and the hydrogen evolved by the action of acid, some idea might be gained as to the valency of helium. It would be as if, for example, hydrogen and chlorine were evolved separately from salt by sulphuric acid, instead of in combination; by measuring each, the deduction could be drawn that chlorine was univalent. Experiments made to this end showed, however, that from some minerals no hydrogen is evolved. Gas, from a sample of uraninite sent by Dr. Hillebrand, contained no trace of hydrogen. It is, of course, possible, and, indeed, not unlikely, that all hydrogen is absorbed in reducing the uranic oxide to uranous oxide. The problem then becomes a complicated one; but we hope to solve it by future experiments.

As yet but few experiments have been made with the object of inducing helium to enter into combination. Like argon, it is not attacked by oxygen in presence of caustic soda under the action of the electric discharge; indeed, this forms a good method of removing all impurities other than argon. Again, like argon, it is not affected by red-hot magnesium, and it is not oxidised by copper oxide at a red heat.

As helium is evolved from clèveite and similar minerals at a red heat, an attempt was made to reabsorb it by heating the powdered mineral to redness in contact with the gas, but not to so high a temperature as that which had served to cause it to be evolved. But the attempt was fruitless: no gas was absorbed. When all the gas in the tubes had been pumped out, after they were cold, heating failed to cause the evolution of more gas.

A further experiment was made, in which metallic uranium was heated to bright redness with a blow-pipe in contact with a mixture of helium and oxygen, the latter gas being greatly in excess. But, curiously, the oxidation of the uranium was very slow, and all the helium was recovered, none having been absorbed. The conditions have yet to be discovered under which helium can be made to combine with oxides of uranium, so as to reproduce the natural product.

The Solubility of Helium.

Helium is very sparingly soluble in water. A determination made by the method previously described for argon (*Phil. Trans.* A. 1895, 37) gave 0·0073 as its coefficient at 18·2. The tube contained 162·3 arbitrary divisions, of which 26·0 were occupied by helium and 136·3 by water. After shaking, the volume of the helium was reduced to 25·0 divisions, and that of the water was increased to 137·3. As 137·3 absorb 1·0, 1 volume of water absorbs 0·0073 volume. The whole apparatus was jacketed with running water during this experiment.

This is the lowest solubility hitherto recorded. Generally speaking, the solubility of a gas is related to the temperature at which it condenses to a liquid, and the sparing solubility of helium points to its having a very low boiling point. Prof. Olszewski has kindly undertaken to make experiments on the temperature of liquefaction of helium, and it will be interesting to find whether its boiling point does not lie below, or, at least, as low as that of hydrogen; for their molecular weights are not very different, and helium is a monatomic gas, a condition which appears to lower the boiling point.

Helium is totally insoluble in absolute alcohol and in benzene.

The Spectrum of Helium.

Mr. Crookes is making an exhaustive study of the spectrum of helium, and will shortly publish an account of his work. But, as some of the deductions to be drawn later depend on the lines observed, it is necessary here to add a few words. In general terms, the spectrum has already been described. The particular point to which attention is necessary here is that at least two of the lines in the spectrum of helium, seen with a wide dispersion prism, are coincident with two of the argon lines. These occur in the red, and comprise one of each of the two pairs of characteristic argon lines. This observation has been frequently repeated, using for the purpose spectroscopes of different dispersive power, and throwing into the field both spectra at the same time, with an exceedingly narrow slit: and we may say that if not absolutely identical, the lines are so near that it is not possible with the means at our disposal to recognise any difference in position. But the relative brilliancy is by no means the same. One of the argon lines, rather faint, is coincident with the prominent red of the helium spectrum, and one of the strong red argon lines is coincident with a faint red line in the helium spectrum.

Besides these two, there is a line in the orange-red, which though perhaps not identical, yet is very close. This line is faint in helium, but moderately strong in argon. It is much more easily visible with helium in the "negative glow" than in the capillary tube.

It may also be of interest to state that, according to Runge's observation, the brilliant yellow line of helium is undoubtedly a doublet. This was frequently observed by us with a grating of 14,000 lines to the inch in the spectrum of the third order. But it must also be noted that one of the lines is very faint; the other, more refrangible, is immensely brighter. The distance, judged by eye, appears to be about 1/50th part of that between the lines D_1 and D_2 of sodium. Accurate information on this last point may be looked for from Mr. Crookes, Mr. Lockyer, and from many others who are interested in the probable occurrence of this element in the sun.[1]

III. *General Conclusions.*

It cannot be doubted that a close analogy exists between argon and helium. Both resist sparking with oxygen in presence of caustic soda; both are unattacked by red-hot magnesium; and if we draw the usual inference from the ratio

[1] Prof. Hale and Dr. Huggins have recently observed that the solar line D_3 is also a doublet. (W. R., July 20).

between their specific heats at constant volume and at constant pressure, both are monatomic gases. These properties undoubtedly place them in the same chemical class, and differentiate them from all known elements.

Although opinion is divided on the precise significance of the ratio of specific heats, 1·66, it appears to be most probable that in all cases, as in that of mercury, this ratio points to the monatomicity of the molecule. If we assume this provisionally, it follows that the atomic weight of helium is identical with its molecular weight. The molecular weight is twice the density, for the molecular weights of gases are compared with the atomic weight of hydrogen, taken as unity: hence the atomic weight of helium on this assumption is $2·13 \times 2 = 4·26$. But again we assume, in making this calculation, that helium is a single element, and not a mixture of elements. Before discussing this question, it appears advisable to inquire whether there is any evidence which would corroborate the deduction that it is a monatomic element. This evidence must be sought for in the properties of argon, for those of helium have not as yet been sufficiently investigated.

We know from countless examples among compounds of hydrogen and carbon that increase in molecular weight is accompanied by rise of boiling point; and it may be stated as a proved fact that a polymeride has always a higher boiling point than the simpler molecule of which the polymeride is formed. Among the substances germain to this inquiry, ozone and oxygen may be cited; the complex molecule of ozone is shown by the higher temperature at which it boils. It might be concluded with certainty, therefore, that A_2, could it exist, should have a higher boiling point than A_1.

Next, it is generally the case that the boiling point of an element, provided it has not a complex molecule like that of sulphur and phosphorus, is lower, the lower its molecular weight. There are the well-known instances of chlorine, bromine, and iodine; but if it be objected that these all belong to the same group, we may cite the cases of hydrogen, $-243.5°$; nitrogen, $-194.4°$; and oxygen, $-182·7°$; and we may add chlorine, $-102°$. If argon possessed the atomic weight 20 and the molecular weight 40, it is probable that its boiling point would lie above that of chlorine, instead of, as is actually the fact, at $-187°$—below that of oxygen. But, it may be objected, the boiling point is determined, not by the molecular weight, but by the density. It may be urged that the density of argon is 20, and that its molecules, like those of oxygen and nitrogen, are diatomic, in spite of the argument to the contrary from the ratio of specific heats. The answer to this objection is obvious: if this were so, its boiling point should lie above, and not below that of oxygen.

These considerations cannot, of course, be accepted as evidence, but merely as corroborative of the conclusion as regards the monatomicity of argon. If they apply to argon, they apply with equal force to helium; and if they are accepted, it follows that the atomic weight of helium is 4·26.

It is again necessary to consider the character of argon in attempting to answer the next question: Are argon and helium single elements or mixtures of elements? But before discussing it, let us consider another question: How does argon happen to occur in the air and helium only in minerals? Why is helium not present in air? A satisfactory answer to this question is, we think, contained in a paper by Dr. Johnstone Stoney (*Chem. News*, 1895, lxxi. 67). He there shows that were hydrogen to be present in air (and it might be present, in spite of the oxygen with which it could be mixed, for a small quantity would surely escape combination), it would, in virtue of the velocity of its own proper molecular motion, remove itself from our planet, and emigrate to a celestial body possessing sufficient gravitational attraction to hold it fast. Dr. Stoney suggests this explanation to account for the absence of an atmosphere and of water vapour on the moon, and for the presence of an atmosphere of hydrogen on the sun. It would also account for the absence of helium in our atmosphere, and for the presence of the chromospheric line D_3. Of course, if an element can form compounds, or if it is [illegible], as helium appears to be, it will, like [illegible], be found on the earth.

The inertness of these gases would favour their existence in the free state. Yet argon exists in the atmosphere, precisely because it forms no compounds. Similarly nitrogen is a constituent of air, because in the first place those elements with which it combines directly are comparatively rare, and also because such compounds are mostly decomposed by water; and the excess of nitrogen therefore occurs in the free state. Similarly, the occurrence of free oxygen is due to the fact that some remains over, after all or almost all the readily oxidised substances have already united with oxygen. If there exist gases similar to argon in inertness, they too may be looked for in air.

Now if argon possess the atomic weight 40, there is no place for it in the periodic table of the elements. And up to now there is no exception to this orderly arrangement, if the doubtful case of tellurium be excluded. Rayleigh and Ramsay have shown that the high density of argon can hardly be accounted for by supposing that molecules of A_2 are mixed with molecules of A_1; and excluding as untenable the supposition that argon is a compound, the only remaining suggestion is that it is a mixture. No attempts have as yet been made to test the correctness of this idea; but experiments have already been started which, it is hoped, will throw light on this question.

The density of argon is too high; to fill its place in the periodic table, between chlorine and potassium, its density should be about 19 and its atomic weight 38. We might expect the presence of another element with a density of 41 and an atomic weight of 82, to follow bromine, as argon follows chlorine; and this element would probably also be a gas, since its density would be only a little higher than that of chlorine.

But here we meet with a difficulty. There are certain lines in the spectrum of helium coincident with lines in the argon spectrum. There can be only one explanation, excluding the extremely improbable hypothesis, which is not verified in any instance, that two elements may give spectra containing identical lines. That explanation is, of course, that each contains some common ingredient; and there appears to be a place for one with density 10 and atomic weight 20, to follow fluorine in the periodic table. The density of helium is, however, so low, that there does not appear room for any large quantity of a heavier gas; and to fit the periodic table, the density of argon should be diminished by removal of a heavier admixture, rather than increased by removal of a lighter one.

Such are the problems which now confront us. Until more experiments have thrown further light on the subject, we regard it as labour lost to discuss the relations of these curious elements to others which find their proper place in the periodic table.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

SIR JULIAN GOLDSMID has been elected Vice-Chancellor of the University of London, in succession to Sir James Paget, who has resigned.

THE new *Directory* of the Department of Science and Art, which has just come to hand, contains the regulations for Organised Science Schools, previously referred to in these columns. Among other matter new to the *Directory*, and announcements of changes, we notice that a new method of according the National Scholarships is in contemplation. The change will not take effect until the Session 1896–97, and due intimation of its nature will be given. The syllabus of Practical Plane and Solid Geometry has been recast in the elementary stage, in the direction already noted, and new syllabuses are given for Inorganic Chemistry, theoretical and practical, Geology, and Physiography. It is not clear, however, whether the questions to be set for the examinations next May will be based upon the new or the old syllabuses.

AT the ordinary quarterly meeting of the Royal College of Physicians of London, held on Thursday last, Sir Russell Reynolds, F.R.S., in the chair, the following gentlemen were elected officers of the College:—Censors, Sir William H. Broadbent, Dr. P. H. Pye Smith, Dr. T. Tillyer Whipham, Dr. William Cayley; treasurer, Sir Dyce Duckworth; emeritus registrar, Sir Henry Pitman; registrar, Dr. Edward Liveing; librarian, Dr. William Munk; examiners chemistry and chemical physics, Mr. Charles L. Groves, F.R.S., Mr. W. R. Dunstan, Mr. J. Millar Thomson, Dr. Samuel Rideal, Dr. R. Taylor Plimpton; materia medica and pharmacy, Dr. T. Lauder Brunton, F.R.S., Dr. Daniel J. Leech, Dr. Sidney P. Phillips, Dr. Frederick Willcocks, Dr. Francis G. Penrose;

elementary biology, Mr. F. Gymer Parsons, Mr. P. Chalmers Mitchell; elementary physiology, Dr. H. Lewis Jones; physiology, Dr. Vincent D. Harris, Dr. Thomas Oliver, Dr. Frederick W. Mott; anatomy, Mr. Charles Stonham, Prof. G. Dancer Thane; medical anatomy and principles and practice of medicine, Dr. Philip J. Hensley, Dr. J. Burney Yeo, Dr. G. Vivian Poore, Dr. J. Mitchell Bruce, Dr. Frederick Taylor, Dr. Stephen Mackenzie, Dr. William Ewart, Dr. Seymour J. Sharkey, Dr. J. Kingston Fowler, Dr. Robert Saundby; midwifery, Dr. J. Baptiste Potter, Dr. J. Watt Black, Dr. Peter Horrocks, Dr. Walter S. A. Griffith; surgical anatomy and principles and practice of surgery, Mr. John Langton, Mr. J. N. C. Davies-Colley; public health, Dr. Charles H. Ralfe, Dr. William Pasteur; Murchison Scholarship, Dr. F. Charlwood Turner, Dr. Samuel H. West.

WE gave last week the names of the Research Scholars appointed for 1895, by Her Majesty's Commissioners for the Exhibition of 1851. We are now informed that the following scholars, appointed in 1894, have forwarded satisfactory reports of their work during the first year of their scholarships, which have accordingly been renewed for a second year.

Name of Scholar.	Nominating Institution.	Place of Study.
J. C. Beattie	University of Edinburgh	University of Vienna.
J. R. E. Murray	University of Glasgow ...	University of Glasgow.
W. B. Davidson ...	University of Aberdeen...	University of Würzburg.
R. C. Clinker... .	University College, Bristol	University College, Bristol.
F. Dent . .	Yorkshire College, Leeds	University of Munich.
A. J. Ewart	University College, Liverpool ...	University of Leipzig.
D. K. Morris	University College, London	University College, London.
J. Frith ...	Owens College, Manchester	Owens College.
R. Beattie	Durham College of Science	Durham College of Science.
W. B. Burnie	University College, Nottingham	Central Technical College.
J. A. McClelland	Queen's College, Galway	Owens College.
F. B. Kenrick ...	University of Toronto ..	University of Leipzig.
F. J. A. McKittrick	Dalhousie University, Halifax, Nova Scotia...	Cornell University.

Note.—Such of the above Scholars as remained at the nominating Institution for the first year will now proceed to another Institution in England or abroad.

The following scholars, appointed in 1893, have been selected for exceptional renewal for a third year:—

Name of Scholar.	Nominating Institution.	Place of Study.
H. W. Bolam.	University of Edinburgh	University of Leipzig.
J. W. Walker . .	University of St. Andrews	Universities of Leipzig and St. Andrews.
J. E. Myers	Yorkshire College, Leeds	University of Strassburg
E. C. C. Baly ..	University College, London ...	University College, London.

SCIENTIFIC SERIALS.

American Meteorological Journal, July.—The geographical distribution of the maximum and minimum hourly wind velocities . . . for January and July, for the United States, by Dr. F. Waldo. This discussion is based on the Signal Service and Weather Bureau observations, and the subject is treated in various ways, and illustrated by wind charts. We select from these (1) the hour of maximum wind and (2) the maximum hourly wind, in miles per hour. There is no great regularity in the time of occurrence of the strongest wind; in January it occurs on the Atlantic coast from 2h. to 4h. a.m., and on the North Pacific coast it is retarded to 6h. a.m. On the Gulf of Mexico it takes place about noon, while at inland stations it occurs generally about 2h. p.m. In July, on the Atlantic coast, there is a maximum wind about 2h. p.m. in latitude 45°, but with southward progress it is retarded, until in latitude 30° the hour is changed to 6h. p.m. In the southern part of the Pacific coast, the time of maximum is 1h. p.m., which is much earlier than for the adjacent inland or the northern part of the coast. In general, for the inland north-east the hour is 2h. p.m., and there is a retardation with both western and southern progress. In January the maximum hourly wind reaches a velocity of seventeen miles on the northern parts of the Atlantic and Pacific coasts, decreasing with southward progress, while the inland distribution shows a maximum of ten to thirteen miles per hour over the Great Plains. In July, the maximum hourly wind is eleven to thirteen miles on the Atlantic coast, while on the North Pacific coast there is a very small maximum (eight miles), but this is counterbalanced by the very high velocity of eighteen miles per hour on the central Californian coast. A reference to the wind charts shows the prevailing conditions much better than any verbal description can do.

Bulletin of the American Mathematical Society, No. 9. (June 1895, New York).—Mr. J. de Perott gives a very interesting sketch of Euclidian arithmetic in connection with a notice of the late M. Stieltjes' contribution to the *Annales de la Faculté des Sciences de Toulouse*, vol. iv., entitled "Sur la théorie des nombres." M. Stieltjes had it in contemplation to write an extensive treatise on the theory of numbers, but unhappily his weak health and final untimely death prevented his getting beyond the paper noticed by Mr. de Perott. This paper is devoted to a greatly generalised form of Euclid's work. "It does not insist on the definition of number, nor on the laws which are at the base of the operations we perform on numbers, but passes immediately to the exposition of the chief properties of the least common multiple and the greatest common divisor of numbers. . . . Poinsot was the first, I think, to whom it occurred that the course could be reversed." The results are expressed in a very symmetrical form by the author of the note.—Mr. G. L. Brown writes a short note on Hölder's theorem concerning the constancy of factor-groups, and Prof. F. Morley a like note on the theory of three similar figures. The theory has been recently given in the sixth edition of Casey's "Sequel to Euclid," and also in the second edition of his "Conics." Prof. Morley believes that something is to be said in favour of an appropriate analytic handling of the theory, and gives here some preliminary equations in a convenient form.

Bollettino della Società Sismologica Italiana, I., 1895, No. 3.—Microseismograph for continuous registration, by Prof. G. Vicentini (see p. 178.)—New type of seismic photochronograph and its applications, by A. Cancani. A description of an instrument by which the face of a chronometer is photographed at the moment of the shock or of the arrival of long-period pulsations from a distant earthquake.—Review of the principal eruptive phenomena in Sicily and the adjacent islands during the four months January–April, 1895, by S. Archidiacono.—The Viggianello (Basilicata) earthquake of May 28, 1894, by M. Baratta. An account of an interesting tectonic earthquake. The meizoseismal area, which is elliptical and only about 17 km. long, is restricted to the northern slopes of M. Pollino. This group of mountains represents the northern half of a vast ellipsoid of dolomites and limestones, traversed by great fractures, which, if produced, pass through Rotonda and Viggianello, the towns most damaged by the shock. Notices of Italian earthquakes (February–April, 1895).

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 22.—M. Marey in the chair.—Researches on the composition of grapes from the principal French vines, by MM. Aimé Girard and L. Lindet.—On the osmotic phenomena produced between ether and methyl alcohol across different diaphragms, by M. F. M. Raoult. It is found that with ether and methyl alcohol on the respective sides of a diaphragm of pig's bladder, the methyl alcohol passes by osmosis to the ether side. The bladder membrane appears to be impermeable to ether: even with mixtures the transference is always of methyl alcohol towards the side where it is of less concentration. Exactly the reverse occurs with a vulcanised caoutchouc membrane, which is impermeable to methyl alcohol, but permeable to ether. The experiments show: (1) that osmosis between two determined liquids may not only vary much in energy, but even change its sense with the nature of the diaphragm; (2) that the osmotic movement of substances

across the diaphragm may be absolutely independent of their molecular weights and of their condition as dissolved substance or solvent.—Action of phenyl isocyanate on some acids and ethereal salts, by M. A. Haller. M. Retzius was elected Correspondant of the Anatomy and Zoology Section, in succession to M. Carl Vogt.—Abnormal refractions at the surface of water, by M. Ch. Dufour. Attention is directed to a source of error, due to irregular refraction caused by differences in temperature between water and air immediately above its surface, which may arise in taking the latitude or determining time at sea.—On static or dynamic explosive potentials, by M. R. Swyngedauw. According to the experiments described, the explosive potential between two poles shielded from ultra-violet radiations is not appreciably diminished by very small and very rapid variations of potential. On a phosphorescence phenomenon obtained in tubes containing rarefied nitrogen after the passage of the electric discharge, by M. Gaston Séguy. In presence of vapours of stannic chloride, the author finds the light emitted from a nitrogen tube to be rose-coloured during the discharge, and milky white for some 10 to 80 seconds after interruption of the current. On the electromotive force of the Latimer Clark, Gouy, and Daniell standards, by M. C. Limb. The values found by the author's method for the elements at 0 C. are: Latimer Clark 1·4535 volts (absolute), Gouy 1·3928 volts (abs.), Daniell (Fleming type) 1·0943 volts (abs.).—On Natterer's tubes, by M. Gouy. On anhydrous crystallised manganese sulphide, by M. A. Mourlot. Crystallised sulphide, identical with *alabandine*, has been obtained by means of the electric furnace. Small cubes or transparent derived octahedra of a greenish shade are obtained. They have the density 3·92 and hardness 3·5 to 4.—On some properties of combinations of ferrous chloride and nitric oxide, by M. V. Thomas. The experiments detailed show that the three compounds obtained by the author in the dry way possess no appreciable tension of dissociation at the ordinary temperature, and hence differ from the compounds obtained in solution by M. Gay.—On some alkaline phosphides, by M. C. Hugot.—Specific heats of superfused formic and acetic acids. Modifications applied to Regnault's thermocalorimeter to enable the determination of the specific heats of a large number of superfused liquids, by MM. Massol and Guillot. The specific heats of formic and acetic acids in the solid state are much greater than their specific heats in the liquid state. The specific heat in the liquid state diminishes with the temperature. When superfused, the specific heat is slightly augmented, but remains of the same order as the specific heat in the liquid state. Synthetic formation of nitro-alcohols, by M. Louis Henry.—Oxidation of inactive campholenic acid, by M. A. Behal.—On the constitution of vegetable albumenoid substances, by M. E. Fleurent. Influence of respiration on the volumetric trace of the limbs, by MM. A. Binet and J. Courtier.—Modifications of the heat radiated produced by faradisation, by M. L. Lecercle. An account of the local rise in temperature [illegible] in animals by electric excitation, and its effect on the general temperature.—Aggravation of the effects of certain [illegible] toxines by their passage through the liver, by MM. J. Teissier and L. Guinard.—A contribution to the histology of unicellular glands, by MM. J. Kunstler and A. Gruvel.—On the [illegible] of the magmas of certain amphibole granites, by M. A. Michel Levy.—On the first alcohol thermometer used in [illegible], by M. l'Abbé Maze.

BERLIN.

Physiological Society, June 7.—Prof. Munk, President, in the chair.—Prof. Baginski reported on experiments made, in [illegible] with Dr. Sommerfeld, on bile from 115 children. [illegible] showed that, in comparison with the bile of adults, it [illegible] more water and mucin and less bile-salts. It contained [illegible] ethereal sulphates, and in the case of children who [illegible] hypothermia it was free from bile-salts. Examination [illegible] children suffering from various forms of nephritis [illegible] an abnormally large amount of xanthin [illegible], which could not be accounted for by any breaking [illegible] or blood corpuscles. Dr. Benda de[illegible] in the mucous membrane of the true [illegible], which cause corresponding furrows in the [illegible] layer. They can be readily brought [illegible] by the removal of the epithelium by macerating in dilute [illegible]. The glands are shorter than the vocal cords, and [illegible] karyokinetic cell-division can often be [illegible] in the [illegible] layer.

June 21.—Prof. du Bois Reymond, President, in the chair.—Dr. Schulz spoke on the anatomy of unstriated muscles in vertebrates. He finds that they consist of elongated cells, pointed at each end, whose length is very variable in different animals. Each cell consists of fibrils imbedded in a highly refractive interfibrillar substance, and of granules and a nucleus in the middle of the cell with two nuclear bodies. Two nuclei in one cell were only seen once among thousands of preparations. The fibrils interlace with each other. The separate cells are not held together by any cement-substance, but by protoplasmic threads and branches. The transverse striation described by many observers appears to be due to a wrinkling of the cell resulting from incomplete extension after having been contracted. Nerve fibres are very plentiful. With methylene-blue, gold chloride, or by Golgi's method numerous ganglion-cells can be brought into view, from which short branches are distributed to the muscle cells. In addition to these numerous nerve-fibrils can be seen ending in minute bulbous swellings which are applied to the muscle. The nerves are sensory as well as motor.—Dr. Cohnstein reported experiments on injecting solutions of sugar into the blood-vessels, in support of his views on the formation of lymph in opposition to Heidenhain. The results were the same as on the injection of salt solutions. The amount of sugar in the blood rose and fell very rapidly, whereas it rose and fell very slowly in the lymph. The maximum of sugar observed in the lymph was equal to the maximum met with at an earlier stage of the experiment in the blood. The solid constituents of the blood became less after the injection, and then increased slowly to the normal; in the lymph, on the other hand, they increased at first and then became less. After the injection of sugar the blood capillaries of a frog's web were considerably dilated and the circulation quickened. Dr. Cohnstein interpreted these results as indicating an initial passage of water from the intercellular spaces into the blood-vessels, followed at a later stage by a return filtration into the lymph. He had also observed a diminution in the secretion of bile after the injection of sugar, and attributed this to compression of the bile capillaries resulting from dilatation of the blood capillaries.

CONTENTS.

THURSDAY, AUGUST 8, 1895.

THE STUDY OF INSECTS.

A Manual for the Study of Insects. By Prof. John Henry Comstock and Anna Botsford Comstock. Pp. 701. (Ithaca, N.Y.: Comstock, 1895.)

THE present work is very much on the same lines as Dr. Packard's well-known "Guide to the Study of Insects," though somewhat more popular, and dealing still more exclusively with North American entomology, of which, on the whole, it furnishes an admirable compendium. It is got up in a very attractive form, and is crowded with illustrations, the woodcuts being chiefly from engravings from nature by Mrs. Comstock.

The first chapter is devoted to a brief explanation of the principles of zoological classification and nomenclature, in the course of which we meet with a system of trinomial nomenclature for sub-species, or constant varieties, which has not hitherto been much patronised by entomologists. Thus, with reference to a common American swallow-tail, Prof. Comstock writes:

"This name, *Jasoniades glaucus*, is used when reference is made to the species as a whole. But if one wishes to refer to the black form alone, it is distinguished as *Jasoniades glaucus glaucus*; while the yellow form is distinguished as *Jasoniades glaucus turnus*."

Surely this is too complicated and clumsy a system for ordinary use!

The second chapter deals with "Insects and their near relatives," and includes a brief definition of the branch (or, as it is more commonly called in England, sub-kingdom) Arthropoda, and a table of the four classes *Crustacea*, *Arachnida*, *Myriapoda*, and *Hexapoda*, or insects. The *Crustacea* and *Myriapoda* are very briefly noticed, though a few typical forms of each are figured; but the *Arachnida* receive more attention, the orders and principal families, especially of the *Araneida*, being briefly discussed, with notices of their chief peculiarities and habits. As an illustration of the author's style in the more popular parts of his book, as well as embodying a curious phase of cannibalism, we may quote the following passage from p. 24:

"Fig. 23 represents the large egg-sac of one of the cobweavers. This is made in the autumn, and contains at that season a large number of eggs—five hundred or more. These eggs hatch early in the winter; but no spiders emerge from the egg-sac until the following spring. If egg-sacs of this kind be opened at different times during the winter, as was done by Dr. Wilder, the spiders will be found to increase in size, but diminish in number as the season advances. In fact, a strange tragedy goes on within these egg-sacs; the stronger spiders calmly devour their weaker brethren, and in the spring, those which survive emerge sufficiently nourished to fight their battles in the outside world."

The remaining chapters are taken up with a sketch of the seventeen orders of insects admitted by Prof. Comstock, with special, and indeed almost exclusive, reference to the North American species. These chapters differ very much in length and importance, the space allotted to some of the smaller orders being barely a couple of pages, while the chapter on *Lepidoptera* alone occupies nearly a third of the volume.

The interest of the book is much enhanced by the illustrations; and in speaking of the *Membracidæ*, one of the families of *Homoptera*, Dr. Comstock observes: "Nature must have been in a joking mood when tree-hoppers were developed"; and the row of "odd fellows" at the foot of p. 154, where this observation occurs, fully bears out the remark.

But it must not be supposed that this book is too popular to appeal to serious students; far from it. Some of the smaller orders of insects are, indeed, passed over with but slight notice; but in the larger ones, we meet with elaborate descriptions of structure, and dichotomous tables of the principal families, which are afterwards discussed in greater detail, and in most cases one or more of the representative American species are figured, frequently with transformations.

Although, as a rule, America suffers more from insect pests than Europe, yet there seem to be exceptions which we should hardly anticipate. Thus Prof. Comstock informs us (p. 103) that "The earwigs are rare in the North-Eastern United States, but are more often found in the South and on the Pacific coast," and the native American cockroaches also are regarded by him (p. 106) as harmless, the destructive species, as in England, being all imported insects. Among these, he mentions the "Croton Bug," as he calls *Phyllodromia germanica*, as infesting "the vicinity of the pipes of the water-systems of many of our cities." In England, this species is particularly numerous in bakeries.

Under the *Fulgoridæ* (Lantern-flies), Prof. Comstock refers to "the fact that they are phosphorescent," apparently being unaware that the statement is very greatly doubted, though it is perhaps premature to say that it has been actually disproved.

A great many figures of neuration of *Lepidoptera* and other insects are given, all numbered according to a uniform system which Prof. Comstock has adopted from Redtenbacher, with modifications of his own, but which is unfortunately not fully explained in the work before us.

English names are given to most of the insects noticed, some of them being rather grotesque. Thus, at p. 274, we find a figure of "The Firstborn Geometer" (*Brephos infans*), with the explanation on the following page: "As this is probably the most primitive geometer occurring in our fauna, we suggest the popular name Firstborn for it." This is not the first occasion on which we have had occasion to animadvert on the introduction of crude speculations on the course of evolution, as if they were established or probable facts.

It is perhaps worth noticing that Prof. Comstock places the *Lepidoptera* between the *Myrmeleonidæ* and the *Diptera*. He has a peculiar classification of his own, which we have not space to indicate in detail; but he makes the *Hepialidæ* and *Micropterygidæ* a separate sub-order under the name of *Jugatæ*, and after it he places the *Frenatæ*, in which he includes all the remaining families, commencing with the *Megalopygidæ*, *Psychidæ*, *Cossidæ*, &c., and ending with the "super family" *Saturniina*, the "families" *Lacosomidæ* and *Lasiocampidæ* (apparently not referred to any "super-family"), and the butterflies, including the "super-families" *Hesperiina* and *Papilionina*, in a reversed order, terminat-

ing with the Nymphalidæ, sub-family *Satyrinæ*. In the butterflies, Dr. Scudder has been chiefly followed.

The family *Papilionidæ* supplies us with an illustration that the book is only written primarily for American students; for the *Papilioninæ* are distinguished by the black ground-colour, the tail, and the five-branched radius of the fore-wings; and the *Parnassiinæ* by the white tail-less wings and four-branched radius, characters not universally exact, though amply sufficient to distinguish the North American forms.

A curious fact is noticed by Prof. Comstock with reference to the Garden Whites. He tells us that the native American species *Pieris oleracea* and *Pontia protodice* have both become greatly lessened in numbers by the increase of the imported European *Pieris rapæ*.

Another curious fact noticed by Prof. Comstock is that the dog-flea is the common flea of the United States, the true *Pulex irritans* being comparatively rare; while the importance of counter-checks in agricultural entomology is illustrated by the author's remark: "Nothing more wonderful has been accomplished in economic entomology than the subduing in California of the cottony-cushion scale by the introduction from Australia of a lady-bug, *Vedalia*, which feeds upon it."

We cordially commend Prof. Comstock's book to European, and especially to British, entomologists; for, although it is written mainly for American students, it contains much which entomologists of other nations will find both useful and instructive. W. F. K.

AGRICULTURE AND HORTICULTURE.

Agriculture, Practical and Scientific. By James Muir, M.R.A.C. Pp. 350. (London: Macmillan, 1895.)

Agriculture. By R. Hedger Wallace. (London and Edinburgh: W. and R. Chambers, 1895.)

The Horticulturist's Rule-Book. By L. H. Bailey. Third edition. (London and New York: Macmillan and Co., 1895.)

PROF. MUIR'S neat and presentable volume is the latest claimant upon the indulgence of the agricultural public, the number of readers—and what is more to the point, the number of students—amongst whom is undoubtedly steadily increasing. Commencing with a discussion of the plant, the author speedily falls back upon the soil as the staple of his discourse, though parenthetically he introduces a chapter on plant food in the soil. Then we get the inevitable section on the British geological formations, which has about as much relation to the living art of agriculture as a list of our kings and queens has to a true understanding of English history. Drainage, irrigation, and other processes for ameliorating the soil are next discussed, and then half a dozen chapters are devoted to the important subject of manures. Implements and machines are next briefly glanced at, and the remainder of the book is occupied by chapters on the chief crops of British agriculture. We believe that, well-worn as the theme is, there is still room for novelty in the treatment of agriculture as a book subject, but Prof. Muir does not appear to have hit upon it.

Live stock constitute the backbone—the sheet-anchor of British agriculture, and to omit all reference to this indispensable section of our greatest national industry in a book bearing the comprehensive title of the volume under notice, is a blemish upon the work. No one would ever infer from its name that the volume is silent upon the great subject of sheep husbandry, which has become so inextricably—and we may add so advantageously—interwoven with the arable farming of this country. Nor would any one expect, in a book on "Agriculture, Practical and Scientific," to find no allusion to the milk-pail and the cows that fill it, and no mention of the butter and cheese industries. The author recognises that agriculture embraces "the breeding, feeding, and management of all kinds of farm live-stock," but it is not till the reader begins perusing its pages, that he learns that the work "will not attempt to deal with" this part of the subject. In this matter, the author had nobody but himself to please, and all we venture to say is that the title of the volume should have fitted its contents. A work on "agriculture" that ignores live-stock might fairly be compared to a treatise on chemistry that made no mention of carbon.

The part of the work that is best done is that relating to crops, and had Prof. Muir chosen to confine himself to this branch of farming, he would not have acted unwisely. His skilful treatment of this section of the subject serves to revive the recollection of John Wilson's admirable work in the middle of the century. But the most important cropping of all—that of grass land—is inadequately treated, though it is abundantly evident, from the few pages allotted to this subject, that the author might usefully have given more space to it at the expense of one or two perfunctory chapters which would not have been missed. The processes of hay-making and ensilage are well described, yet here again the idea arises that the author felt he was approaching his limits, and the result is that he appears to exercise a restraint which we feel sure has operated to the disadvantage of the reader. A feature of the work that will be much appreciated is that it reproduces in a handy form many of the tabular statements that have from time to time been published in the *Journal* of the Royal Agricultural Society of England. Three dozen illustrations accompany the text, and those of seeds are particularly noteworthy for their fidelity.

Commending the book, then, for its trustworthy treatment of farm crops, we may notice one or two features that seem to call for criticism. The index is sometimes relied upon for the introduction of terms not given in the text. Thus, "nitrification" is indexed as dealt with at page 25, turning to which the reader finds the process described, but no name given to it, unless perchance the term "oxidation" is inadvertently used instead. Other similar cases occur. A highly important subject to farmers, the temperature of germination, is surely awarded scant treatment when it is dismissed in the brief paragraph: "The temperature most favourable to germination varies in the seeds of different plants." Such frequent recourse is made by the author to the work of Lawes and Gilbert, that it is regrettable he did not imitate the consistency with which they employ the term "nodules" to denote the outgrowths on the roots of papilionaceous plants. The repeated use of the word

"tubercle" can only lead to confusion, especially now that, in connection with bovine and other tuberculosis, it is so frequently heard at agricultural gatherings. Several peculiarities in spelling, adhered to throughout the work, might in a new edition be brought into conformity with general usage: examples are afforded in *Telletia*, *Cecydomyia*, *Centorhynchus*, *Siton i*, *Chonopodium*, *Claviceps purpura*.

It is difficult to understand why the second of the volumes of which the titles head this notice has been prepared, unless it be to find favour with candidates in a certain specified examination, the syllabus of which, however, the author tells us, "has not been slavishly followed." The really valuable parts of the book have apparently been culled from the writings of five living agricultural authors whose names are mentioned in the preface, and who, if they turn over the pages of this compilation, can hardly fail to alight upon much that they have seen before. It is regrettable that the author did not cling to his guides throughout. He would not in that case have said of sainfoin: "In appearance the leaves resemble those of vetches, but the blossom is more like that of red clover." Apart from the worthlessness of such a statement as this, it cannot fail to raise a doubt as to whether the author has ever seen a field of sainfoin. Again, with reference to lucerne, we read: "Like sainfoin, it produces good crops for about ten years." Where, we would ask, is the district in which sainfoin stands for anything like this period? What is meant by the statement that "sainfoin is much harder than lucerne"? The germination of a seed is described as "the period parallel to the sucking of a young mammal"; and elsewhere we read, "nitrification goes on or acts more quickly under circumstances favourable for rapid growth, and in this respect is parallel to germination." Nothing, perhaps, indicates the character of the book more thoroughly than the page of illustrations entitled "Various Specimens of ——s Grass Seeds." We omit the name of the seedsman, who probably would be sorry to claim that a seed of rye-grass, for example, sold by him is different from all other rye-grass seed.

The 350 pages of the book are divided into no fewer than 70 chapters. *Inter alia* a treatise on chemistry is introduced, with figures of a spirit-lamp and test-tube. From a chapter on "Blossoms and their functions," we cull the following specimen of literary grace: "We are apt to look upon them merely as objects created to feast man's eye with their beauty, or his nose with their sweet scent." The language of the book is of an irritating style, which is constantly in evidence from the grammatical blunder at the close of the preface down to the final chapter, in which reference is made to what "the plant needs to live healthy." It is, however, only fair to add that, at the outset, the author writes: "It has been my endeavour to avoid errors."

The sub-title of Mr. L. H. Bailey's book "A compendium of useful information for fruit-growers, truck-gardeners, florists, and others" indicates its scope. In a score of chapters such subjects are dealt with as injurious insects, insecticides, plant diseases, fungicides, lawns, grafting, seeding, storing of fruits and vegetables, the weather, and many other matters of practical interest. It is stated in the preface: "The contents of the volume have been gleaned from many sources; and, whilst the compiler cannot assume the responsibility of the value of the many recipes and recommendations, he has exercised every care to select only those which he considers to be reliable." The result is a most valuable book, and though intended primarily for American readers, it will none the less constitute a useful reference manual for horticulturists in this country. We notice, with regard to potato disease, that it is recommended to spray the plants with Bordeaux mixture "upon the first indication of the blight." It would probably be better to follow the advice, recently published by the Irish Land Commission, to spray before the appearance of disease, and thus employ the application as a preventive rather than a remedial measure. It is when the reader meets with such a remark as the "marsh-marigold or so-called cowslip," that he must bear in mind the American origin of the book. There is probably no better work of its kind.

OUR BOOK SHELF.

Electrical Laboratory Notes and Forms. Arranged and prepared by Dr. J. A. Fleming, F.R.S. (London: The *Electrician* Printing and Publishing Co.)

It is now generally recognised that the best way to teach the rudiments of science is by the natural or kindergarten method, which aims at leading the young student to observe facts and phenomena for himself, and come to conclusions concerning them. The method is applied easily enough to very elementary practical work, and with the best results. In the case of elementary work in physics, all the student requires to be told is what to do, and he may be left to find the teaching of his results. For instance, it is only necessary to instruct him to find the weights of equal bulks of different liquids and solids, and the results of his experiments show him at once what relative density means. This principle of letting the results of experiments suggest conclusions is undoubtedly the right one for introductory courses of practical physics and chemistry; indeed, almost the only information that need be given to the students in the laboratory is how to set up their simple apparatus and what to do with it; nothing ought to be said about what they are going to prove, or the experiments lose their value of developing the faculties of acute observation and intelligent induction from the observed facts.

Advanced work in physics and chemistry offers difficulties to the extension of the scientific method of observation and induction. The time spent in the laboratories is far too short to enable students to rediscover the more intricate laws and relationships for themselves, however admirable the mental training of such researches may be; and if the instruments are all arranged so that it is only necessary to press a knob to make them act, and obtain a result, the value of the mechanical observations then made cannot be very great. The difficulty of applying the scientific method to physical laboratory work is brought out by the volume before us. The volume contains twenty elementary and twenty advanced exercises in electrical measurement. Each exercise consists of a six-page sheet, two pages of which are occupied with a condensed account of the theoretical and practical instructions for performing the particular experiment, while the remaining pages are ruled up in lettered columns, to be filled in by the student with the results of his observations. What the student does

is really to test the accuracy of formulæ, mostly arrived at by theoretical considerations; the work is therefore purely deductive, and not inductive. Yet it is difficult to see how to make the work covered by these notes anything but deductive; certainly no better system of teaching practically the elements of electrical engineering has so far been developed.

By means of Dr. Fleming's notes and a little oral assistance now and then, the student will be able to perform instructive experiments, and will be taught to observe closely, and to record his results neatly. The method followed facilitates the work of the demonstrator and the student, and enables a large amount of practical work to be carried out in a comparatively short time.

Microbes and Disease Demons. By Dr. Berdoe. Pp. 93. (Swan Sonnenschein and Co., 1895.)

Under the above sensational title the writer discusses, or rather attacks, the anti-toxin treatment of diphtheria. It is difficult to understand what has prompted the production of so prejudiced and, we regret to say, unscientific comment upon this subject. We most emphatically take exception to such expressions as "scientific quackery," and others of a similar character, being applied to investigations of which, although the therapeutic value may be as yet a question of opinion, undoubtedly mark a new step forward in our endeavour to unravel the problems surrounding disease.

We have no intention of discussing Dr. Berdoe's views in detail, but we feel ourselves called upon to refer to one statement, because the writer has used it as a vantage ground for his most savage attack upon this method of treating diphtheria. We refer to the death in Brooklyn alleged to have resulted from the injection of some of the anti-toxin. Several pages are devoted to a detailed account of the incidents of the case, and Dr. Berdoe does not hesitate to designate it as "sudden death from anti-toxin." This, however, is not the view of the Brooklyn Health Department, or of authorities in the Bacteriological Laboratory of the New York City Board of Health, in both of which institutions the anti-toxin used was submitted to a very careful and exhaustive examination, and the official opinion given that it was not responsible for the death of the patient.

The case for or against the anti-toxin treatment of diphtheria is not one which should be approached from a party point of view, and such prejudiced, vaporous effusions as Dr. Berdoe has permitted himself to indulge in, will never take any part in deciding the question of its efficiency. To arrive at any such positive conclusion is of necessity a matter upon which time and experience can alone give the final verdict, and its discussion should only be entrusted to those who are capable of approaching the subject in a scientific and judicial spirit.

Men gu-yu-mu-tsi: or, Memoirs of the Mongol Encampments. Translated from the Chinese by P. S. Popov, Russian General Consul at Peking. 580 pp. *Memoirs of the Russian Geographical Society*, vol. xxiv.: (Russian.) (St. Petersburg, 1895.)

This is the work of two Chinese men of science, Chjan-mu, or Shi-chjou, author of a history of Jinghiz khan's conquests, and Khe-tsyu tao, author of several geographical works, of which the description of the northern borderland is best known. It was published in China in 1867, and consists of two parts: a description of the different tribes and confederations into which the Mongols are divided, with short notes on the extent of the territories they occupy, and short historical notices—the whole covering only about 160 pages of the Russian edition—and a great number of most interesting footnotes, which cover more than two thirds of the volume, and contain a great variety of miscellaneous geographical and historical information.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

University of London Election.

I have read the letters which Mr. Bennett, Mr. Thiselton-Dyer, and Prof. Ray Lankester have addressed you on the subject of the University of London, and much regret that my friends, whose opinion I value so much, take exception to one paragraph in my letter to Prof. Foster. I do not wish to seem to treat their views with any want of respect, and perhaps, therefore, you will allow me to send a few lines in reply.

They all criticise the sentence in which I state that I should endeavour to maintain the right of Convocation given in the Charter, which expressly provides that no alteration should be made in the constitution of the University without the assent of Convocation.

Prof. Ray Lankester says that "Sir John Lubbock has adopted and made himself the leader of this extraordinary and fantastic policy." Whether it is extraordinary and fantastic or not, is of course a matter of opinion, but, at any rate, it is the law at present.

I am satisfied that my constituents highly value this right, and I fail to understand how Mr. Thiselton-Dyer has been able to persuade himself that in endeavouring to maintain it I am taking a line "not courteous to Convocation," or have given "Convocation the severest slap in the face it has ever received."

Prof. Ray Lankester also says that I "have shown an unfavourable estimate of the intelligence" of my constituents. This is such an extraordinary version (not to say perversion) of what I said, that I trust you will allow me to quote my own words. What I said was—

"Feeling that Convocation ought to be consulted on a matter so vitally affecting the University, I should strongly urge, and would do my best to secure, that the scheme when arranged should be submitted to Convocation for their approval, to be signified as at a senatorial election, and would oppose the Bill unless this were conceded."

Why should this proposal appear to my friends as being, in Mr. Bennett's words, fatal to "all hopes of bringing our University into line with the requirements of the age"? The Commissioners will either propound a wise scheme or an unwise one. My critics believe that it will be wise. Why, then, should they assume that Convocation will reject it? At any rate it is an extraordinary reason for attacking me as a Member of Parliament, that I have faith in the good sense and sound judgment of my constituents.

John Lubbock.

High Elms, July 30.

Metrical Relations of Plane Spaces of n Manifoldness.

Plane spaces of n manifoldness are assumed to have the following properties:

(1) Given a S_{n-1} (a plane space of $n-1$ manifoldness) and a point P outside the same, then a certain S_n will exist which contains both the S_{n-1} and P.

It follows therefore that a S_n is determined by $n+1$ of its points, unless these points have that special situation to each other by virtue of which they are contained in a plane space of minor manifoldness.

(2) If a plane space S_m contains $n+1$ points, which have not the special situation to each other above mentioned, then it will contain the plane space S_n, determined by these points.

It therefore appears that $n+1$ points determine a S_n uniquely.

Given a straight line L and any point P upon the same: through L any number of planes can be constructed, each of which contains a certain line L′ through P perpendicular to L. The aggregate of such lines L′, in a space S_n form a S_{n-1}, which has that special position towards L by virtue of which it is called perpendicular to L in P.

To prove this theorem, which certainly holds if $n=2$ or 3, let us assume that it is true when $n=k$; then it will also be true when $n=k+1$. Through P, in a space S_k which contains L and is contained by S_n, construct the S_{k-1} perpendicular to L. Any point not contained in the S_{k-1} and L determines a plane,

which contains the perpendicular PQ to L. PQ and the S_{k-1} determine a space S_k, and the proposition is that any line through P in this S_k is perpendicular to L. Through PQ construct a plane space Σ_{k-1} in S_k perpendicular to L. It must exist, according to hypothesis. S_{k-1} cuts the S_k into two parts, because every straight line in S_k (as easily follows from the assumptions) has one point in common with the S_{k-1}, we therefore have no means of passing from one point of such straight line to its other points without passing the S_{k-1}. S_{k-1} and Σ_{k-1} cut the S_k therefore into four different parts, which have the cut of Σ_{k-1} and S_{k-1}, that is a certain S_{k-2}, in common. Let the four departments, into which the S_k is cut, be called A, B, C, D. A straight line through P, not contained by the S_{k-2}, will be situated (as it passes P, that is a point of the S_{k-2}) in two different departments; and if we change the situation of this line continuously, without passing either the S_{k-1} or the Σ_{k-1}, it will remain in the same two departments. The departments are therefore arranged by two. If a straight line through P, belonging to A, also belongs to B, then A and B shall be called opposite to each other. Let A, B and C, D be opposite to each other. We have no means whatever of distinguishing two opposite departments, unless we assume at the very least another arbitrary point, because every plane configuration through the S_{k-2}, extending into one department, equally extends into the opposite one. Whatever is true for the one department must therefore be true also for the opposite one.

Now construct any line L′ through P in the S_k. Let L′ belong to A and B. If L and L′ are not perpendicular, then the angle LL′ contained in A must be larger or smaller than the corresponding angle LL′ contained in B. Let L′ change its position continuously; if the angle LL′ contained in A would be always larger than the corresponding angle in B, this would amount to a permanent property of A distinguishing it from B, which it cannot possess. Therefore, whichever evolution L′ may perform from the S_{k-1} to the Σ_{k-1} in A (and B), it must have at least one intermediate situation in which L and L′ are perpendicular. The aggregate of such situations form a surface in A and B. Let L', . . . L'_{k-1} be $k-1$ lines contained in that surface; then the plane space of $k-1$ manifoldness containing these $k-1$ lines, must, according to the hypothesis, be perpendicular to L. The surface must therefore contain this plane space. If now we replace one of the two S_{k-1} or Σ_{k-1} by this space, the argument will still hold. However, near the two borderings plane spaces will finally approach, there will always be at least one intermediate plane perpendicular space, all of which are contained in the S_k. It is therefore nothing left but to concede that the S_k in question has the property established in the proposition.

Through any point P only one line L will pass, which is perpendicular to a space S. Assume indeed two such lines, which may have with S respectively Q and R in common. Then PQR would form a triangle, of which ∠ PQR as well as ∠ PRQ, according to the foregoing, will be = a right angle. This, however, is impossible, unless Q and R coincide.

A point and a plane space therefore determine a certain line, the perpendicular to that space through the point, a certain point—the one in which the line above mentioned cuts the space—and a magnitude, the distance of the two points above mentioned. This is always true, unless the point belongs to the space. Let the point approach the space. If the two points in question coincide, then the point will belong to the space. The conditions, therefore, that a point and a space are united, is (distance of point and space) = o.

Let P move continuously so that its distance from a plane space S remains unaltered; P and S may determine a space Σ; then the aggregate of such points in Σ is another plane space. Let P and Q be two situations of P. Then all points of the line PQ have the same distance from S, as is easily seen to rest on Euclid's parallel axiom by means of parallelograms. The general proposition can, from this, be established by considerations analogous to the proof of our first theorem, independent of any new assumption. Two such spaces Σ and S are called parallel, and determine a certain magnitude, whose disappearance is the condition of coincidence of Σ and S.

Let Σ and S be parallel. Through any point A outside the same draw two lines, which cut both Σ and S, in B, C and B′, C′ respectively, then the lines ABB′ and ACC′ have a point in common, they are therefore in the same plane, BC and B′C′ must therefore either have a point in common, or be parallel. A point in common they have not, as they are contained in Σ and S, and these two have no point in common. It follows that

$$AB : AB' = AC : AC'.$$

We now add to our assumptions another one. $n+1$ points determine, as already stated, a plane space S_n, and besides a certain pyramid of n dimensions; of which we assume that it shall possess magnitude. Let the $n+1$ points be A_1 . . . A_{n+1}. A_2 . . . A_{n+1} determine a certain space S_{n-1}. Draw any line through A_1. It cuts S_{n-1} in a point B. Choose A'_1 on this line so that $A_1B = BA'_1$. Then the two points A_1, A'_1 have an exactly symmetrical position to S_{n-1}. No property can be valid for the one which is not valid for the other (as long as no elements are introduced to disturb the symmetry). We cannot therefore assume that one of the two pyramids, determined respectively by A_1 and the A_2 . . . A_{n+1} or A'_1 and A_2 . . . A_{n+1}, should be larger than the other. Now the *locus* of points A'_1 is, according to the foregoing, a parallel Σ_{n-1} to S_{n-1}. It follows: The magnitude of the pyramid is dependent (1) on n of its points (2) and the distance of the $n+1^{st}$ from the plane space determined by these n points.

What we have in mind, when we speak of the magnitude of a pyramid, will come out clearer when we give a theorem of addition. Let X be any point collinear with and intermediate between A_2 and A_3. Then we say:

The pyramid determined by A_2X and any other points + the pyramid determined by XA_3 and those other points = to the pyramid formed by A_2A_3 and the rest of the points.

This explanation, combined with the above, shows that the magnitude of a pyramid is equal to some constant multiple (say $\frac{1}{n}$) of the product of the magnitude of the pyramid A_2 . . . A_{n+1}, and the distance of A_1 from the space fixed by the other points. We shall write this number $(A_1A_2 \ldots A_{n+1})$. (A_1A_2) is simply the distance of the two points, and according to a convention necessitated by considerations of continuity, we assume

$$(A_1A_2) + (A_2A_1) = o.$$

Generally, if we transpose any two letters, the magnitude designated changes sign.

If A, B, C are three collinear points, and if we designate by the single letters A, B, C the distances from these points of any fixed point O on that line, then we have identically

$$(AB)C + (BC)A + (CA)B = o.$$

This is an algebraical identity easily established. The same holds also when the single letters A, B, C are made to denote the distance of these points from any space Σ, which either is parallel to line ABC, or has with it a point in common, as is easily established by proportions.

If between three points of a line such an equation exists, this must be true also for $n+2$ points in a S_n. The proof of this by induction is perfectly easy Let for instance A, B, C, D, be four points in a plane, and let Σ be any space, that has with it a line in common. Join CD; it may meet AB in E. Then we have some linear identity

$$aA + bB + cE = o$$

where a, b, c denote constants independent from Σ, and also

$$dC + eD + fE = o.$$

Eliminating E, we obtain some linear identity between

A, B, C, D.

In order to determine the constants, let us assume the space Σ (which is permitted) to be parallel to the plane ABCD; then we have if

$$aA + bB + cC + dD = o$$
$$a + b + c + d = o.$$

If we place Σ so that it cuts ABCD in CD, and if then we make $a =$ (BCD), c follows = (CDA). We therefore obtain

$$(BCD)A + (CDA)B + (DAB)C + (ABC)D = o$$

and just so in the general case

$$(BCD \ldots L)A + (CD \ldots LA)B + (D \ldots LAB)C + \ldots = o.$$

The use of the distances of points from variable plane spaces enables us to do away with fixed coordinate systems. The proof of projective theorems becomes perfectly lucid, while at each stage of the proceedings we are always able to give the geometrical significance of the constants employed. To give a

few instances: Let $A_1 \dots A_{n+1}$ be $n+1$ points in a plane space S_n. Let P be any other point. We then have one linear relation

$$a_1A_1 + a_2A_2 + \dots + a_{n+1}A_{n+1} + pP = 0.$$

Assume outside the space any point Q. Construct the plane spaces $QA_1 \dots A_n, QA_2 \dots A_{n+1}, \dots$ $n+1$ in all, and cut them by some line joining the residual point $A_{n+1}, A_1 \dots$ respectively with a point R on the line QP. We thus obtain $n+1$ new points $A'_{n+1}, A'_1, \dots$, which joined give a plane space Σ_n, that cuts S_n always in one and the same plane cut S_{n-1}, however we may choose Q and R, which is related to P and the configuration of the A in a peculiar manner.

To follow the different steps indicated, let us assume

$$pP = qQ + rR$$

(the three points are collinear); therefore

$$a_1A_1 + a_2A_2 + \dots + qQ + rR = 0.$$

Joining R with A_1, we obtain a line that contains the point $\frac{a_1A_1 + rR}{a_1 + r}$, which as

$$a_1 + a_2 + \dots + q + r = 0$$

is also $= -\frac{a_2A_2 + \dots + qQ}{a_2 + \dots + q}$, that is contained in the plane space $A_2A_3 \dots Q$.

$$A'_1 \text{ is therefore } = \frac{a_1A_1 + rR}{a_1 + r}.$$

Just so

$$A'_2 = \frac{a_2A_2 + rR}{a_2 + R}.$$

The line $A'_1A'_2$ contains the point

$$\frac{(a_1 + r)A'_1 - (a_2 + r)A'_2}{a_1 - a_2};$$

that is

$$\frac{a_1A_1 - a_2A_2}{a_1 - a_2},$$

which is collinear with A_1, A_2. The plane space S_{n-1} contains therefore all the $\frac{n \cdot n+1}{1 \cdot 2}$ points thus formed, and the proposition follows at once.

In a similar way it may be proved that, if two $(n+1)$ pyramids in a S_n are in perspective, the intersection of corresponding sides, $\frac{n+1 \cdot n}{1 \cdot 2}$ in all, are all contained in a S_{n-1}. We prove this simply for $n = 2$, which is sufficient to exhibit the general way of proceeding. Let ABC, A'B'C' be two triangles in perspective; let AA', BB', CC' have point P in common. Then we must have

$$\begin{aligned} P &= aA + a'A' \\ &= bB + b'B' \\ &= cC + c'C'. \end{aligned}$$

Join AB, A'B'. Their intersection, from

$$aA + a'A' = bB + b'B'$$

follows

$$\frac{aA - bB}{a - b} = \frac{a'A' - b'B'}{a' - b'}.$$

Now $\frac{aA - bB}{a - b}, \frac{bB - cC}{b - c}, \frac{cC - aA}{c - a}$, are obviously collinear.

Two plane spaces in general do not determine one magnitude only. Take, for instance, two lines in space. They have a distance, and form an angle. If their distance or the sine of their angle is = 0, they will be coplanar. If both are = 0, they will coincide. We have two magnitudes, because the system of two lines in space has two degrees of degeneration (coplanarity and coincidence). This is also generally the case, because geometrical magnitudes are nothing but the most suitable invariants, whose evanescence is the necessary and sufficient condition for the degeneration of the system to which they belong.

If two plane spaces A, B determine only *one* magnitude, we designate the same by (AB). Let A be a straight line, for instance, and B a plane space which has one point in common with A. From any point of A, say P, draw the perpendicular to B. Join B with point Q, common to B and A. Then the sine of (∠Q) is the magnitude denoted by (AB). Let A be a plane, having in common with B a line. From any point P of the plane draw the perpendicular on B, say PB, and from this point B the perpendicular on the common line BQ. Then again sin (∠Q) = (AB), and thus generally. We determine the sign of the magnitude according to the rule

$$(AB) + (BA) = 0.$$

Let us now add another plane space C to the system A, B, such that both CA and CB determine only one magnitude. Then the whole system may determine an additional one, whose evanescence would signify that C belongs to the plane space fixed by A and B in conjunction, and is united with the space that A, B have in common. It is in fact the product of (AB) and the magnitude formed by C and the space AB, and will be written

$$(ABC)$$

In this way we proceed, obtaining the definition of a magnitude, which has the property that its evanescence is the necessary and sufficient condition for the degeneration of the system to which it belongs.

The magnitude in question may be formed in various ways, but the system being such that it can possess only one such magnitude, the different formations must always lead to one and the same result, with the exception of a constant factor. This factor must either be + 1, or else − 1, on account of the symmetrical way in which the magnitude is formed. If the system is one of straight lines through a point P, the magnitude in question has a special significance. Two triangles which have an angle in common, are in proportion as the product of the sides including this angle. Three lines in space which have a point in common and are not coplanar, form a corner. Cut a corner by two different planes. The two different pyramids are in proportion as the product of the three sides forming the corner. And so in general, as can be easily proved by induction. Therefore, if we have such a corner of n lines in a space S_n, and cut it by a space S_{n-1} the pyramid formed is = the product of the n sides extending from the vertex of the corner multiplied with a factor which is specific for the corner; and this latter factor is exactly the magnitude formed according to the rule given.

(It may happen that the formation of the magnitude, as given, leads to zero without giving a significant result. This is an indication that somewhere during the process one of the conditions of degeneration is fulfilled—for instance, when C belongs to the space AB. Then the process is the reciprocal one. We determine the magnitude formed by C and the space common to A and B. If that also is zero, then A, B, C belong to what is called a pencil. The simplest case of this kind is the system of three lines in a plane.)

Let A B C be three plane spaces belonging to a pencil: that is, let (ABC) = 0. Let D be any other plane space, which has an element with the pencil in common. Then we have again

$$(AB)C + (BC)A + (CA)B = 0,$$

where the single letters A, B, C in this identity denote the magnitude formed between each of these three spaces and the auxiliary one.

It will suffice to prove this for the case of three lines through a point P. Let Σ cut the pencil in a line S. Let A, B, C form with S the angles α, β, γ respectively, then the proposition amounts to

$$\sin(\alpha - \beta)\sin\gamma + \sin(\beta - \gamma)\sin\alpha + \sin(\gamma - \alpha)\sin\beta = 0,$$

which is nothing but the Ptolemaus theorem about four points in a circle.

Now again we may proceed to show, that between $n+2$ elements A_i, for which, to be short $(A_1A_2 \dots A_{n+2}) = 0$, a linear relation must exist $\Sigma a_i A_i = 0$, where the a_i are certain constants. Of course, if not also some of the minors are zero, such as $(A_1A_2 \dots A_{n+1})$, this will be the *only* relation that can thus exist. We can therefore determine the a_i, by giving Σ exceptional positions. The result is again

$$(A_1A_2 \dots A_{n+1})A_{n+2} + (A_2A_3 \dots A_{n+1}A_{n+2})A_1 + (A_3 \dots A_{n+2}A_1)A_2 + \dots = 0.$$

Let $A_3 \dots A_{n+2}$ form the space S, and the magnitude (S) then, making Σ identical with S, we obtain

$$(A_2S) \cdot (S)A_1 + (SA_1) \cdot (S)A_2 = 0.$$

But $(A_2S) = -A_2$ for this special position of Σ, and $(SA_1) = A_1$, therefore the test applies, and the theorem must be correct.

For such systems A_r as we have considered, all projective properties will be corresponding to each other, and all metrical properties at least as far as they are dependent upon the interpretation of the constants employed. EMANUEL LASKER.

Ilkley, July 9.

P.S.—The same holds true, with slight modifications, for the only curved space that contains no exceptional elements, that is the surface of a globe of n manifoldness.—E. L.

The Feigning of Death.

THE discussion, a few months since, of the feigning of death in reptiles (vols. li. pp. 107, 128, 223, and lii. p. 148), induced me to experiment on the Currant Moth, whose powers of "shamming" are so familiar. The moth was first seized by one wing, and it at once feigned death; thereupon I cut off its head with a pair of scissors, and the *animal continued to feign death*. I use the expression advisedly, for absolute immobility was maintained for some seconds, and then violent fluttering ensued, causing the animal to rush wildly about the table, but failing to lift it into the air. In this condition any impulse, such as touching or pinching, induced a repetition of "shamming." After a strong stimulus the shamming was prolonged, and indeed a direct connection was obvious between the strength of stimulus and the length of period of quiescence. This power of response to stimulus was maintained for two days, and then weak fluttering set in for some hours, followed by death. Our entire ignorance of the physiology of the nervous system of insects renders it difficult to draw complete conclusions from these phenomena; nevertheless, it is difficult to conceive that volition can persist for forty-eight hours in a decapitated animal. We are forced then to conclude that here, at any rate, death-feigning is a purely reflex phenomenon, and that the sensory stimulus received by the surface of the body causes inhibitory impulses to arise reflexly from the ganglia of the central nerve chain, and prevent all movement of the locomotor muscles. In confirmation of this, it may be mentioned that denuding the wing of its scales over any area caused a marked diminution of sensitiveness over the area so treated. Since all stages between sensory hairs and ordinary scales occur in Lepidoptera, it is not unreasonable to assume that the scales still function as tactile end-organs, in spite of their modification subserving decorative purposes. OSWALD H. LATTER.

Charterhouse, Godalming, July 31.

Halley's Chart of Magnetic Declinations.

IN NATURE for May 23 and 30, 1895, are interesting communications from Dr. Bauer and Mr. Ward in reference to Halley's old chart of magnetic declinations.

I have a copy of this chart not referred to by either of these gentlemen.

It is bound in vol. i. of "Miscellanea Curiosa." This work was edited by Halley; it consists of three volumes, containing, in the main, reprints of papers read before the Royal Society. Vol. i. was published in 1705, and was printed by J. B., for Jeffery Wale and John Senex.

The chart is $7\frac{1}{2}$ inches high and 13 inches long, and embraces just the circumference of the earth.

The title in the upper left-hand corner reads: "A new and correct Sea Chart of the Whole World, showing the Variations of ye compass as they were found Año 1700 with a view of the General and Coasting Trade Winds and Monsoons or shifting Trade Winds by the Direction of Capt. Edm. Halley."

In the lower left-hand corner is the note: "Capt. Halley's map of the World in two large sheets is sold by R. Mount and T. Page on Great Tower Hill, London."

The name "I. Harris, delin. & scu." is in the lower right-hand corner of the chart. CHARLES L. CLARKE.

New York, July 27.

THE ERUPTION OF VESUVIUS, JULY 3, 1895.

THIS recent disturbance at Vesuvius is interesting in several ways, and at one time had all the appearance of developing into as grand a display as that of 1872.

The last eruptive cycle of Vesuvius commenced on June 7, 1891, when I had the good fortune to be but a few hundred yards distant at the time the main bursting of the rift took place. The details of that eruption, with illustrations, can be referred to in my articles and reports.[1] We may briefly state that cycle as follows: the splitting of the whole of the great cone of Vesuvius by a radial rift which extended beyond the base for some distance across the Atrio del Cavallo. At the first moment a little lava issued from the upper part of the rift, but after a few hours all came from its lowest extremity in the Atrio, and continued to flow with practically no interruption for a period of nearly three years, or, more correctly, from June 7, 1891, to February 7, 1894. During that period no great quantity was given forth at any one time, so that no stream could attain much length before cooling. Though the amount emitted during that period is enormous, and if vesicularised into pumice and scoria would, I think, quite equal Monte Nuovo in volume. The consequence of this is, that a great and pure lava cone was built up in the Atrio, of low inclination (14°), and adding much to obliterate that interesting and characteristic feature of the volcano. Coincident with the formation of the rift, the central cone rapidly crumbled in, until a deep crater was formed which eventually attained over 150 m.

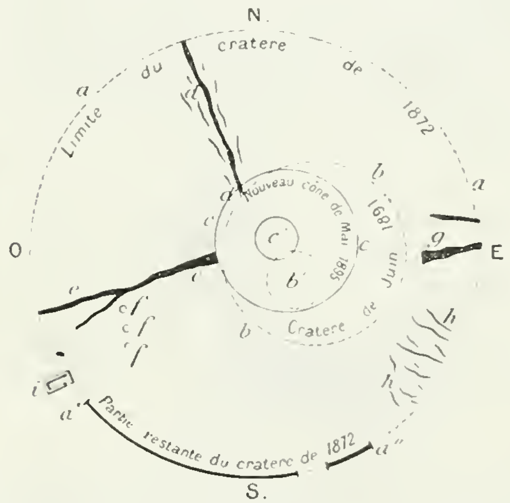


FIG. 1.—Diagram showing the actual state of Vesuvius, from a drawing by M. A. Bourdariat, after an earlier plan of mine (*La Nature*, June 8, 1895). (*a*) Limit of the crater edge of 1872; the part represented by a dotted line is that covered by more recent lavas of different dates. The parts *a*′ and *a*″ are still uncovered. (*b*) Crater of June 1891. (*b*′) Active vent of the 1891 crater. (*c*) New cone in process of formation (May 1895). (*c*′) Active vent of the cone on May 12, 1895. (*d*) Rift and vapour mouth of June 7, 1891. (*e*) Fissure emitting acid vapours on crater plain formed in the 1872 crater. (*f*) Very old hot-air passages and fumaroles. (*g*) Fissure of May, 1889. (*h*) Numerous fissures on the south-east edge of the crater plain. (*i*) Guides' shelter.

in depth and diameter. It was at its greatest dimensions in February 1894, when the lava stopped issuing by the lateral outlet, and therefore commenced to rise in the chimney. The immediate result of that stoppage was that the formation of a cone was soon commenced at the bottom of the crater by the ejection of lava cakes. The growth of this new cone of eruption was so rapid that, when I visited and photographed the interior of the 1891 crater in November last, this was not above 60 or 70 m. deep, and the cone of eruption was rapidly increasing in height within it.

My friend M. Alex. Bourdariat has carefully observed the

[1] "Il Vesuvio," *Corriere di Napoli*, June 10, 1891. "L'Eruption du Vésuve," *L'Italie*, Rome, June 13, 1891; *Le Figaro*, Paris, June 17, 1891. "The Eruption of Vesuvius," *Mediterranean Naturalist*, Malta, July 1 and August 1, 1891. "Lettre sur l'Eruption du Vesuve," *L'Italie*, Rome, July 18, 1891. "L'Eruption du Vesuve, visites d'exploration au Volcan," *La Nature*, August 8, 1891 (illustrated). "The Eruption of Vesuvius," NATURE, vol. xliv. pp. 160-161, 320-322, and 362 (illustrated). "Report British Association," 1891-92-93-94. "L'Eruzione del Vesuvio," *Rassegna delle Scienze Geologiche*, vol. i. Rome, 1891 (illustrated).

phenomena of the volcano during the early months of the present year, and has recorded the changes in *La Nature*, June 8 (Fig. 1). It appears from his interesting description that in January of this year the apex of the cone of eruption overtopped the edge of the 1891 crater. Lava even flowed out in the crescentic depression between eruptive cone and crater ring. This was followed by a little repose of some days, to be succeeded by powerful ejections of lava cakes to a considerable height (80 to 100 m.), which rapidly added to the growth of the eruptive cone. In May, this new cone was from 15 to 20 m. above the 1891 crater, and at the commencement of July was considerably more, as is shown by Fig. 2, taken from San Giorgio a Cremano, as the others (and also notes) by Mrs. T. R. Guppy.[1] This sketch shows that on the day preceding the eruption, central activity with cone-forming stage was very active, attaining the fifth degree on my scale.

M. Bourdariat's plan of the summit of the great cone, constructed on one of mine of earlier date, shows the axis of the new eruptive cone is not concentric, but to the north-west of the 1891 crater. This he attributed to the wind, no doubt one of the causes at work, but I had seen such displacement to be the case in November last, when from the depth of the cone top within the enclosing crater walls these sheltered the falling cakes from the wind. There was evidently even then the radial fissure directed to the north-west in process of formation, which has now been the point of issue of this new eruption.

FIG. 2.—Vesuvius as seen from San Georgio a Cremano before the eruption (commencement of July).

The first indication of the final splitting of the great cone was at midnight, when the crater became quiet. In half an hour (that is, on July 3 at 12.30 o'clock) when the guardian of the upper railway station of Mr. G. M. Cook's railroad, which is but a very short distance from the rift, was awakened by a strong shock of earthquake that produced some slight cracks in the masonry foundation of the building. The shocks, though slighter, continued during the night. At eight the stronger shocks were again repeated, and the activity, which had recommenced at the chimney, had again ceased. This was due to the filling of the fissure as it extended outwards by the lava, the level of the surface of which naturally sunk. When this takes place, support is removed from the inner sides of the chimney in the cone, which crumbles in and chokes the vent. The whole top of the mountain had by this time become fissured, in consequence of which, at nine o'clock, seven or eight large blocks of rock, besides a quantity of small ones, were detached from the top of the cone, crashed and ploughed down its side, leaving a scar described as looking like a mud stream, and marked by a number of pits at equal distances, due to the bounding of some of these boulders. This scar is seen in Fig. 3, close by the side of the right of the new lava stream. Mr. Treiber, Mr. Cook's engineer, calculates one of these blocks to be at least 20 c.m. The point of detachment and the resulting scar was by the side of the upper part of the new fissure, but a little to the south-west, and the traces left by the rolling masses are parallel to it.

At 10.18, the radial dyke reached the surface of the great cone and formed an eruptive mouth on a level with and to the north of the upper railway station, from which a copious outflow of lava took place, running down the cone, as seen in the figure below.

At 10.30, about 70 m. lower down, a fresh eruptive mouth was opened, and is well seen in Fig. 3, having an oblique

FIG. 3.—Vesuvius as seen on July 3, at 10 a.m.

crateriform appearance, as in the case of the upper one, and on other similar occasions a jet of steam, that constitutes the excavating agent, was converted into a blackish column by the lapilli, sand, and dust dislodged and carried up with it from the side of the mountain. There is certainly some discrepancy in Mr. Treiber's report, for Mrs. Guppy's sketch, made at ten o'clock, shows this lower *bocca* already in existence. Her sketch likewise exhibits the progress of truncation of the central eruptive cone by the formation within it of a crater. Such a crater is entirely due to the crumbling in of the edges and their fall down the chimney, as no explosions were going on by the top part of the main chimney. Lava continued to pour forth from the lower end of the lower crateret, and probably from a part of the radial fissure that reached the surface below it, but which of course is hidden by the flowing lava. The stream reached the bottom of the great cone at the junction of the Atrio del Cavallo and the Piano di Genista, and then extended towards the upper end of the ridge of the Lion's Paw, or I Canteroni, where was once the old Crocelle. Here it soon formed a fine stream 60 m. in breadth. Besides the two main craterets, already

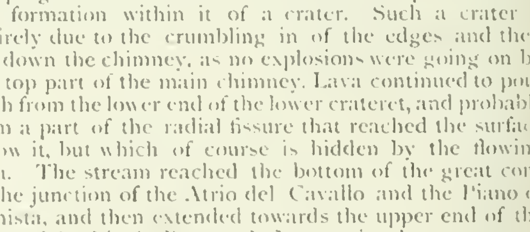


FIG. 4.—Vesuvius as seen on July 5, at 10 a.m.

described, two minor ones also were formed on the same line of rift.

On July 4, the craterets quieted down, little lava flowed, so that during twenty-four hours the face of the stream only advanced 12 m. This corresponded with a slight return of activity at the main chimney, so as to relieve the accumulating vapour tension of the lava below, which the mountain will not resist for long.

The ejections were, of course, of the *accessory* type (that is, not *essential* to the eruption, but simply the remnants of the crumbled-in portion of the eruptive cone. Each

[1] I have, moreover, to thank Mrs. Guppy for notes and sketches that, at considerable trouble, she has placed at my disposal. They are of much value, as she has observed and knows all the changes of the mountain during the many years she has resided in Naples.

puff had its characteristic black colour, due to the quantity of *accessory* sand and dust.

At 22 o'clock, the upper crateret gave out a little vapour and a little lava, but again became quiet. At 23 o'clock, the lower crateret showed new cracks around about it, with the escape of vapour.

During the night, between the 4th and 5th, the lava again increased, so that it is reported the next morning to be advancing at the rate of 25 m. per hour. It had turned to the west, and flowed down on the south side of the Lion's Paw, or the Observatory ridge, and had divided into two main streams, which subsequently subdivided into minor ones that radiated in different directions.

On July 5, the explosions at the central crater were powerful, so as to form from time to time pine-shaped vapour plumes over the volcano. At others, the vapour was bent over the Atrio by the sirocco wind, so as to spread a shower of dust and sand right across that depression. One of these is well indicated in Fig. 4.

So far no damage has been done except to a private carriage road that crosses the Piano di Ginista to the lower railway station. No cultivated land has been reached. The lava is, however, on a steep slope, and is flowing in the direction of the valley called the Cupa Pallarino, over the edge of which a magnificent cascade of incandescent rock was formed in 1872.

The eruption is quite identical in all its details with the usual antecedent ones, resulting from the formation and extension outwards of radial dykes. Many of such eruptions I have described in these pages and elsewhere, and fully explained their mechanism, production, growth and closure.

Three results may happen: (1) The radial sheet of rock may cool and seal the rift so that the volcano will soon return to the cone-forming stage, as seems to be indicated by the appearance of pasty lava cakes amongst the ejecta on July 5. (2) The fissure may enlarge and extend downward with the outflow of lava, as in 1872, with the formation of a much larger central crater. (3) It may follow the more usual course, as its immediate predecessor, and give issue to a small but almost continuous outflow of lava during months or years.

H. J. JOHNSTON-LAVIS.

P. L. CHEBYSHEV (TCHEBICHEFF).

THE death of Prof. Chebyshev has hardly been noticed in the English papers; and even in Russia, except for a short sketch in the University *Bulletin*, and in a speech of Prof. Markoff's with reference to him, which is reported in the *Bulletin de l'Académie impériale des Sciences de St. Pétersbourg*, no biographical notice has appeared of this celebrated mathematician.

Paphnyty Levovitch Chebyshev was born on May 14, 1821, at Akatovo, in the government of Kaluga; and after being educated privately, entered Moscow University; he completed the usual courses, and took his Bachelor degree. In 1846 he received his Master's degree at the same university for his "Essay on the elementary analysis of the theory of probability," and in the following year commenced a series of lectures as assistant lecturer in Petersburg University. He received the Doctor's degree in 1849 for his well-known "Theory of Comparison," which contained a model exposition of the formation of the theory of numbers, and clearly proved the strength of his mathematical genius. In 1852 Chebyshev was promoted to an extra professorship, and in 1860 to a regular professorship. During 1853-59 he was elected successively assistant, extra, and ordinary tutor in the Academy of Sciences. He remained a professor, doing active work of the most valuable kind, thirty-five years, during the course of which, at various times, he lectured on every branch of pure mathematics, and during one period—in 1849-51—on practical mechanics.

In his numerous writings Chebyshev left a very great deal to the reader's imagination, often giving deductions simply without proofs, but in his lectures he never left a point without the fullest explanation; and his lectures are distinguished not only for elegance and accuracy, but for their extraordinary simpleness; the already-mentioned "Theory of Comparison" may serve as a good example, as well as his proof of Bernoulli's theorem, which is now given in all works on the theory of probability.

The professorial services of Chebyshev had a great significance to the Petersburg University. He placed the teaching of mathematics on a firm basis, and formed an independent school of thought. All the present staff of mathematical teachers in the Petersburg University, except a very few of quite the youngest, are his pupils and follow in his footsteps. His moral influence did not, therefore, cease when he resigned his professorship in 1882. The Council of the University elected him an honorary member, and his pupils kept up the habit of going to him on certain days to have lively discussions on various scientific subjects, in which his indomitable energy acted on his hearers in the most animating manner. He was always to be found engaged either on some complicated calculation or on models of mechanism he had invented.

Everything Chebyshev did bore the impress of genius; he invented new methods for the solution of difficult problems, which had appeared and had remained unsolved; he suggested himself a series of most important problems, and worked at them till the end of his life. His very first writings on the theory of numbers, devoted to the problem of the inter-dependence of the prime numbers, and on limits, gave him a European reputation, and his succeeding investigations on irrational differentials, and maximal and minimal quantities, assured his position as the most original mathematician of the nineteenth century.

He died November 26, 1894; his works will shortly be republished by the Petersburg University.

NOTES.

As already briefly announced in these columns, the Institute of France will celebrate its centenary next October. The programme of the *fêtes* which have been organised in connection with that event has just been sent to the Members and Correspondants of the Institute, the intention being that the centenary shall be marked by a reunion of all the men of light and leading who belong to the Institute. On the afternoon of October 23, there will be a reception in the Palais de l'Institut of the Foreign Associates and Correspondants and of French Correspondants, and in the evening the Minister of Public Instruction will hold a reception. On October 24, a meeting will be held in the Great Hall of the Sorbonne, at which the President of the Republic will attend. Discourses will be delivered by the President of the Institute, the Minister of Public Instruction, and M. Jules Simon. A banquet, to which all the Associates and Correspondants are invited, will take place on the evening of the same day. On October 25, there will be a special performance at the Comédie Française, and a reception will be held by the French President. The celebration will be concluded on October 26, by a visit to the Château de Chantilly. It will be seen from this that the hundredth anniversary of the foundation of the Institute of France will be celebrated in a manner worthy of the high position which the Institute holds among the world's societies of science, art, and literature.

THE seventh session of the Australasian Association for the Advancement of Science will be held in Sydney, from January 3 to 10, 1897, under the presidency of Prof. A. Liversidge, F.R.S. The Presidents and Secretaries of the Sections are

as follows:—Astronomy, Mathematics, and Physics: President, Mr. R. L. J. Ellery, C.M.G., F.R.S.; Secretaries, Prof. R. Threlfall and Mr. J. Arthur Pollock. Chemistry: President, Mr. T. C. Cloud; Secretary, Mr. W. M. Hamlet. Geology and Mineralogy: President, Captain F. W. Hutton, F.R.S.; Secretaries, Prof. T. W. E. David and Mr. E. F. Pittman. Biology: President, Prof. T. J. Parker, F.R.S.; Secretaries, Prof. W. A. Haswell and Mr. J. H. Maiden. Geography: Secretary, Mr. H. S. W. Crummer. Ethnology and Anthropology: President, Mr. A. W. Howitt; Secretary, Dr. John Fraser. Economic Science and Agriculture: President, Mr. R. M. Johnston; Secretaries, Prof. Walter Scott and Mr. F. B. Guthrie. Engineering and Architecture: President, Mr. H. C. Stanley; Secretary, J. W. Grimshaw. Sanitary Science and Hygiene: President, Hon. Allan Campbell; Secretary, Dr. J. Ashburton Thompson. Mental Science and Education: President, Mr. John Shirley; Secretary, Prof. Francis Anderson. Communications and papers for the meeting, or inquiries, may be addressed to the Permanent Hon. Secretary, The Chemical Laboratory, The University, Sydney, N.S.W.

It is announced that the Hodgkins prize of ten thousand dollars has been awarded by the Smithsonian Institution, in equal proportions, to Lord Rayleigh and Prof. Ramsay, in recognition of their discovery of argon.

We regret to notice the death of Mr. Joseph Thomson, whose explorations in Africa have added so much to our knowledge of that continent. He was only thirty-six years of age.

Science announces the following appointments:—Prof. William J. Hussey, of Illinois, to succeed Prof. Barnard as Astronomer at the Lick Observatory; Dr. J. Allen Gilbert to be Assistant Professor of Psychology at the University of Iowa; Mr. J. H. Tyrrell to be Professor of Geology and Mineralogy in the University of Toronto.

Reuter's correspondent at Newfoundland, writing under date of July 23, says:—The steamer *Kite*, having on board the members of the Peary Relief Expedition, took her departure a few days ago for Bowdoin Bay, Inglefield Gulf. Her return can hardly be looked for before October 1.

Mr. Cecil H. Smith, of the Department of Greek and Roman Antiquities in the British Museum, has been appointed director of the British School at Athens for the next two years, in succession to Mr. Ernest Gardner, who has held the office since 1887. The Trustees of the British Museum have, with the concurrence of the Treasury, given Mr. Smith special leave of absence for the purpose.

The annual meeting of the Society of Chemical Industry was held at Yorkshire College, Leeds, last week. In his presidential address, Dr. T. E. Thorpe, F.R.S., described some of the important advances made in technological chemistry during recent years, and especially dwelt upon the methods used for the enrichment of coal gas; the manufacture of glycerine from waste soap lyes; the manufacture of edible fats; the improvements on the chemical side of photography; and the chemistry of textiles. The following new officers were elected:—President, Mr. Tyrer; Vice-Presidents, Mr. T. Fairley, Mr. Boverton Redwood, Sir H. E. Roscoe, Dr. T. E. Thorpe. Members of Council, Prof. Le Neve Foster, Mr. Douglas Herman, Mr. C. C. Hutchinson, Mr. Ivan Levinstein, Mr. J. S. McArthur, Sir Robert Pullar. Treasurer, Mr. E. Rider Cook. Foreign Secretary, Dr. Ludwig Mond. It was decided to hold the next annual meeting of the Society in London.

Bedford College (for Women) has taken what appears to us to be an important and commendable step in establishing a separate and scientific course of instruction in hygiene. This subject, which is becoming every day of more consideration, has generally been taught in a somewhat disconnected manner, as an adjunct to be attached anywhere, rather than as a distinct study; at Bedford College it is now to take its place as a special subject. Students will be required to devote themselves for a session or more solely to this and allied branches of science, namely, physiology, bacteriology, chemistry, and physics, practically as well as theoretically, and thus they will have the opportunity, by following a connected system of teaching, of really understanding the meaning and practical bearings of the subject. Many appointments as sanitary inspectors, health mistresses in schools, and teachers of hygiene, being now open to women, the subject seems to offer considerable inducement to those who have an aptitude and liking for scientific work, to devote themselves to this study.

Men of science often have occasion to regret that they do not live in the glorious age when tidal evolution shall have so reduced the spin of this world of ours that there will be forty-eight hours in a day. To be able to devote twice the present amount of time to observation would indeed be a boon to the busy investigator, and the man who shows how to do it, places his fellow workers under a deep obligation to him. Yet that is what Dr. Gowers, F.R.S., did in an inaugural address delivered before a general meeting of the Society of Medical Phonographers last week. Here is his argument: "Science rests on observation, which without immediate record is of little value; not only is memory inadequate, but record at once reveals unsuspected imperfections in observation. Compared with long hand, shorthand permits, in a given time, twice the amount of record, while leaving twice the time for observation." Shorthand requires no better recommendation than this to the notice of students of science, and we are glad to know that the Society of which Dr. Gowers is president, though only started last December, has now 165 members. In the daily work of the practitioner, which is peculiar in being a form of personal science, record is very important. For most practitioners, however, record is practically impossible in longhand, while shorthand offers them the desired means. But this is not only the case with medical men; it is always important that observations, however trivial or strange, should be committed to writing. We are, therefore, a little surprised that the Society should, so far as the name is concerned, be only one of Medical Phonographers. Its objects appear to be broad enough to justify the name being changed to the Society of Scientific Phonographers, and a further argument for the more comprehensive designation is that many scientific workers outside the ranks of the medical profession have already become members.

An interesting point in connection with the sand filtration of water has been recently brought to light by Dr. Kurth, of Bremen. It has frequently been pointed out that the thickness of the layer of fine sand in filtering beds cannot be reduced beyond certain limits without endangering the bacterial quality of the filtrate. Making more detailed examinations of the particular bacteria present in the effluent from a filter in which the depth of filtering material had been interfered with, Dr. Kurth found that the rise in the number of bacteria was almost entirely due to the presence in large quantity of one particular microbe, of which, however, no trace could be found in the raw water with which the filter was being fed. On one occasion there were as many as 900 in 1 c.c. present of this special microbe, whilst all the bacteria together in the raw water did not amount to more than 700 in 1 c.c. In this instance, therefore, the objectionable rise in the number of bacteria present in the filtrate did not necessarily indicate that the efficiency of the filter in dealing with the raw water was in fault, but rather that the disturbance

of the sand had dislodged certain microbes present in the filtering material. It would appear, therefore, of interest to obtain in cases where the filtrate is unsatisfactory some particulars of the microbes present in the effluent, and determine in what relation they stand to the raw water microbes.

The question of the audibility of fog-horn signals at sea seems destined to occupy a great deal of attention in naval circles. Some time ago we gave a description of the American experiments, which went to prove that round each siren there is a zone, about $1\frac{1}{2}$ nautical miles broad, within which fog-signals cannot be heard, although they are distinctly heard outside that zone. These observations cannot now be treated with the incredulity they at first met with, since other experiments have confirmed them. A series of such experiments are described in *Hansa*. In one of these, the vessel steamed with the wind straight towards the light-ship from a distance of $4\frac{1}{2}$ nautical miles. At a distance of $2\frac{3}{4}$ miles the sound became faintly audible, and suddenly increased in loudness at $2\frac{1}{2}$ miles, retaining the same intensity up to two miles distance. From $1\frac{3}{4}$ to $1\frac{1}{2}$ miles the note was scarcely audible, but then it immediately increased to such an extent that it appeared to originate in the immediate neighbourhood of the vessel. The steamer at this point reversed its course, and the fluctuation over this part of the course was found to be the same, except that it was even more strongly marked. Reversing again, the vessel steamed over this distance a third time, and again the sound disappeared at $1\frac{1}{2}$ miles and reappeared again, so loud that it sounded as if the fog-horn was only two cables' lengths off. Then, at half a mile, the sound disappeared entirely, to reappear at quarter of a mile from the light-ship, after which it gradually and steadily increased in intensity until the latter was reached. It is time that this question, which is of great practical importance, should be systematically investigated.

The second annual report of the Iowa Geological Survey, dealing with the work done during 1893, has just come to hand. The Survey was organised just three years ago, and it has carried out some very valuable investigations during its comparatively short existence. The coal deposits of Iowa have received special attention since the organisation of the Survey, and one volume descriptive of them was issued last year. But these deposits are far too extensive to be discussed in a single volume. We have it on the authority of Dr. C. R. Keyes, the Assistant State Geologist, that the area of the coal measures in Iowa is somewhat over twenty thousand square miles, and that isolated carboniferous outliers, and the region bordering the productive coal measures, which must be gone over in tracing the limits of the formation, occupy fully five thousand square miles or more. With reference to the beds of gypsum at Fort Dodge, Dr. Keyes says the area covered by the gypsum contains, approximately, twenty-seven square miles, and that, at the lowest estimate, the mass of gypsum which is found available in the region is not less than sixty millions of tons. Much valuable data with reference to these deposits are given in the report, and also information in regard to the building stones, clays, and other useful mineral substances in Iowa. Though the Survey has primarily a utilitarian point of view, it is clear from the report that the more scientific side of geology is not neglected. Prof. W. H. Norton contributes to the report a paper on the thickness of the Palæozoic strata in North-Western Iowa, based upon records of a number of borings for artesian and other deep wells. He also gives the results of a study of Devonian and Carboniferous outliers in Eastern Iowa. The report is illustrated by thirty-four figures in the text, and thirty-six plates; the most striking of the latter belong to a paper by Dr. Keyes, on glacial scorings in Iowa. Two new localities showing exceptionally fine effects of glacial action were found near the city of Burlington in 1893. One of them is near Kingston, on the top of a bluff overlooking the Mississippi river, and judging from the reproduction of a photograph, it furnishes a very remarkable example of a glaciated surface. Prof. Calvin, the State Geologist, is to be congratulated upon the work carried on under his direction. The Survey has lately lost Dr. Keyes, who has become State Geologist of Missouri, his place being filled by Mr. H. F. Bain.

The fifty-sixth annual meeting of the Royal Botanic Society will be held in the Gardens, Regent's Park, on Saturday afternoon next, the 10th inst., at one o'clock.

A dainty catalogue, in which many rare and valuable geographical works are described, has been issued by Mr. Bernard Quaritch. The catalogue should be seen by all interested in geographical literature.

We learn from the *Journal of Botany* that the herbarium of the British Museum has recently acquired a very fine collection of Hepaticæ made by Herr F. Stephani. It numbers about 10,000 specimens, and includes types of 1100 new species described by Herr Stephani.

The *Proceedings* of the Liverpool Naturalists' Field Club for 1894 contain a record of a large amount of scientific work done in the way of botanical excursions in Lancashire, Cheshire, and North Wales; a list of carboniferous fossils found within twenty miles of Liverpool; and reports of papers read at the evening meetings. The total number of animals and plants that has been recorded as occurring in the district, both living and extinct, is given as 5735.

The August number of the *Quarterly Journal of the Geological Society* contains a paper, by Dr. J. W. Gregory, on the Palæontology and Physical Geology of the West Indies. Among the other papers we notice the following:—Prof. J. B. Harrison and Mr. A. J. Jukes-Brown, on the chemical composition of oceanic deposits; Mr. H. M. Bernard, on the systematic position of the Trilobites; Prof. W. J. Sollas, on the mode of flow of a viscous fluid; Dr. C. S. Du Riche Preller, on fluvio-glacial and inter-glacial deposits in Switzerland; and Mr. E. T. Newton, on fossil human remains from Palæolithic gravels at Galley Hill, Kent.

The Royal College of Belen, Havana, has just published its magnetical and meteorological observations for the year 1890. This institution has regularly issued reports since 1862, and the continuous instrumental curves, which accompany the tables, have furnished valuable information for the investigation of West India hurricanes. Since 1872, one of the late Padre Secchi's well-known and expensive meteorographs has been regularly at work at Havana, and is said to give very satisfactory results. We note that an attempt is made each month to connect the magnetical with the atmospherical disturbances.

We have received from the Jesuit College of Oña, province of Burgos, a pamphlet containing meteorological observations made twice daily, with monthly and yearly results for the years 1883–1894. The Observatory is 1900 feet above sea-level, and is rather sheltered; but the summary of the climate of that part of Spain by Prof. Valladares, and the observations of cirrus clouds and their connection with atmospheric disturbances, are valuable contributions to meteorological science. During the twelve years in question, the extreme shade temperatures varied from 1°·3 to 100°, the annual mean being 51°·8, and the average yearly rainfall was 22 inches.

M. Casimir de Candolle contributes to the *Archives des Sciences Physiques et Naturelles* an important paper on the latent life of seeds. From a series of experiments, chiefly on seeds of wheat, oat, and fennel, he concludes that dormant seeds pass through a period of completely suspended animation.

in which all the functions of the protoplasm are quiescent, but from which they revive when again placed in conditions suitable for germination. The immunity from injury appears to depend on the protoplasm of the seed passing into a completely inert state, in which it is incapable of either respiring or assimilating, before exposure to the unfavourable conditions. The period of suspended animation may extend over an indefinite time, probably through a long series of years, and the seeds may during this period be subjected to very low temperatures without destroying their vitality. Those above mentioned were exposed, in a refrigerator, as many as 118 times in succession, to a sudden cooling to temperatures varying between −30 and −53° C., without injurious effects. On the other hand, seeds of the sensitive plant and of *Lobelia Erinus* succumbed, for the most part, to similar treatment. These statements have an important bearing on the question of the retention of their vitality by buried seeds.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Herman Schlesenger; a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss Folhurst; a Macaque Monkey (*Macacus cynomolgus*) from India, three Slow Lorises (*Nycticebus tardigradus*) from Sumatra, presented by Mr. Stanley S. Flower; a Geoffroy's Marmoset (*Midas geoffroii*) from Panama, presented by Miss Mina Sangiorgi; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mdlle. Eugénie Grobel; a Barbary Ape (*Macacus innuus*) from North Africa, presented by Mr. Edwin Fletcher; two Crested Porcupines (*Hystrix cristata*), two Cape Zorillas (*Ictonyx zorilla*) from South Africa, presented by Mr. J. E. Matcham; a Ducorp's Cockatoo (*Cacatua ducorpsi*) from the Solomon Islands, presented by Mrs. Dexter; a Nightjar (*Caprimulgus europaeus*), European, presented by Mr. T. West Carnie; two Robben Island Snakes (*Coronella phocarum*) from South Africa, presented by Mr. Barry McMillam; —— Chameleon (*Chamaeleon basiliscus*) from Egypt, presented by Mr. J. Buchanan; a Brown Capuchin (*Cebus fatuellus*) from Guiana, a Black-backed Jackal (*Canis mesomelas*) from South Africa, six Ring-tailed Coatis (*Nasua rufa*) from South America, deposited; a Red River Hog (*Potamochoerus penicillatus*) from West Africa, a Sooty Phalanger (*Phalangista fuliginosa*) from Australia; a De Filippi's Meadow Starling (*Sturnella defilippi*) from La Plata, purchased; two Mandarin Ducks (*Aex galericulata*), seven Summer Ducks (*Aex sponsa*), three Chilian Pintails (*Dafila spinicauda*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Rotation of Venus.—Notwithstanding the persistence with which the planet Venus has been telescopically observed, the period of rotation is still undetermined with anything like certainty. Schroter believed the time of rotation to be 23h. 21m.; and this period, or thereabouts, was pretty generally adopted until the announcement by Schiaparelli, in 1890, that the time of rotation was probably equal to that of the planet's revolution round the sun, that is, about 225 days. This conclusion was based on the rigidity of the markings at different hours of the day and for weeks together. Observations by M. Perrotin and Dr. Terby tend to strengthen the conclusion arrived at by Schiaparelli. On the other hand, M. Niesten observed the planet between 1881 and 1890, and found that a period of 23 hours satisfied his observations; while M. Trouvelot, from nearly twenty years' work, concludes that the rotation period was about 24 hours. In this divided state of opinion, therefore, it is evident that much remains to be done before any satisfactory conclusion can be drawn.

During the present year, Mr. Brenner, of the Manora Observatory, has observed the planet as frequently as possible since April 17 (*Ast. Nach.* 3300). His first observations of a bright and a dark spot near the north pole led him to agree with Schiaparelli, but further observations have changed his opinion, and he now believes the period to be about 24 hours. On July 2 he announced that a marking near the southern cusp had been visible since June 9, but became invisible about 4 p.m. each day, while a well-marked streak appeared about 8 p.m. Other marks also appeared and disappeared in a manner inconsistent with a rotation period of more than 24 hours. One of the most important of the markings, though noted quite independently, appears to be identical with one observed by Mr. Stanley Williams eleven years ago: in a communication to Mr. Brenner, Mr. Williams states: "In 1884 I managed to secure about one hundred sketches of the markings on Venus. These mostly favour a rotation of about 24 hours; but there was one strongly-marked indentation near the southern horn, which remained visible continuously for about a month. It was prolonged on the disc by a narrow and unusually dark and definite streak (for Venus)." Mr. Brenner has since claimed to have proved with certainty that Venus rotates in about 24 hours: some of the markings return regularly at the same hour of the day, and are invisible at other times, when the definition is equally good; and it is even possible to observe the appearance and advancing of the most conspicuous streak.

Geodetical Observations.—Dr. Geelmuyden, of Christiania, has recently published the results of a comparison between the astronomical and geodetical determinations made in the course of a triangulation of Norway. The stations selected for observation lie between 59 and 64° lat., and the astronomical work connected with the investigation was conducted under the direction of the late Prof. Fearnley, extending as far back as 1868. The observations refer to measurements made at eleven stations, of which nine have both the azimuth and latitude determined, and two the difference of longitude.

As origin for the geodetical survey, the geographical coordinates of Dragonkollen, a station on the Swedish border, have been chosen, partly because its position is particularly well determined, but principally on the ground that its situation points to the existence of a very small local attraction. Assuming that for this station a vertical line coincides with the normal of Bessel's ellipsoid, Dr. Geelmuyden has computed, with the data already collected in the course of the geodetic survey, the deviations of the plumb-line for the other stations, in which both the azimuth and the latitude have been determined. The results are shown in the following table:

Station.	Difference of azimuth.	Difference of latitude.	Deviation of vertical.
Jonsknuden	+ 8·55	− 1·31	5·17
Gausta	− 6·23	—	—
Husbergöen	− 0·72	+ 0·54	0·68
Christiania	− 3·87	+ 1·79	2·87
Högevarde	− 13·00		—
Hostbjörkampen	+ 6·40	+ 4·68	5·88
Næverfjeld	+ 4·49	+ 6·62	7·06
Gien	− 10·72	− 2·65	6·20
Graakallen	− 7·71	− 6·98	7·98
Norberghaug	− 6·70	+ 0·67	3·36

The deviations of the plumb-line here shown, agree on the whole with what might be expected from the conformation of the surface and the contiguity of neighbouring mountains. For example, the westerly deviation of Gien can be explained by the attraction of Dovrefjeld. An exception is, however, met in the case of Norberghaug, where an easterly rather than a westerly deviation would have been expected. A map is attached, in which is shown both the position of the several stations and the direction of the deviation of the plumb-line.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE annual summer meeting of the Institution of Mechanical Engineers was held in Glasgow last week, under the chairmanship of the President of the Institution, Prof. Alexander B. W. Kennedy, F.R.S. A strong local committee had been organised under the chairmanship of Sir Renny Watson, Prof.

Archibald Barr being Secretary, and very complete arrangements had been made for the instruction and entertainment of members taking part in the meeting. In a great engineering centre there can be no lack of objects of interest to afford excursions for a meeting of this Institution, and the organising committee had taken full advantage of the facilities put at their disposal by owners of works who had liberally thrown them open to members.

The meeting commenced on Tuesday, July 30, and was brought to a conclusion on the Friday following. The mornings of the two first days were devoted to the reading of papers, of which the following is a list:—

Hydraulic stoking machinery and labour-saving appliances in modern gas works, by Andrew S. Biggart.

Notes on modern steel-work machinery, by James Riley.

Recent engineering improvements of the Clyde Navigation, by James Deas, Engineer of the Clyde Navigation.

Notes on hydraulic power supply in towns: Glasgow, Manchester, Buenos Ayres, &c., by Edward B. Ellington.

Papers on telemeters and range-finders for naval and other purposes, by Profs. Barr and Stroud, and on the electric lighting of Edinburgh, by Henry R. J. Burstall, were also on the agenda, but had to be adjourned until the next meeting.

On members assembling in the Institute of Fine Arts, they were welcomed by the Lord Provost of Glasgow, Sir James Bell, and the usual formal business having been disposed of, the first paper was taken, namely, that of Mr. Biggart, on gas works machinery. In this the author described an extensive hydraulic plant which has recently been laid down at the Dawsholm Gas Works in Glasgow. The apparatus is designed to supersede hand labour in the charging of retorts, and clearing them of the residual coke when the gas has been abstracted from the fuel. The usual method of performing these operations by hand must be known to most people. The coal having been broken to suitable size by hand, is placed in the retort by means of a long half-round scoop or trough. This is pushed into the retort and then turned over, the coal then being spilled and spread evenly throughout the length of the retort. This is very laborious work, and moreover the smoke and dust accompanying it are very injurious. It is, however, less trying than the discharging of the retorts, an operation which consists of raking out a mass of coke almost at a white heat. It will be easily understood, even by those not personally acquainted with gas works, that labour of this nature does not tend to the advancement of the labourer, for though good wages are paid they are apt to be spent in ways not all that could be desired. The introduction of machinery to supersede this somewhat demoralising work is therefore a distinct boon to the workman as well as the proprietors of gas works, and thus, indirectly, the users of gas; in fact, it is the oft-told tale of intelligent work being required to produce machines which take the place of the unthinking labourer. That is very nearly the whole history of the elevation of the working classes. In the machinery described by Mr. Biggart, and illustrated by wall-cartoons displayed at the meeting, the coal is broken by a machine having rolls with powerful steel claws which draw in the coal and break it to pieces of the required size. The coal is conveyed by means of buckets travelling on chains; these scoop it up and take it to the machine or to the required spot after it is broken. The charging machine consists first of a steel frame mounted on a carriage which runs on rails laid on the platform in front of the battery of retorts. Attached to the frame is a hopper, and from this a given quantity of coal is allowed to fall in front of a "pusher-plate." The function of the latter is to thrust the coal into the retort, the necessary forward motion being obtained by means of a hydraulic ram. A second ram is used to withdraw the pusher. About six or seven pushes are required to place the coal in a retort, the quantity that has to be placed at the far end naturally going in first. The arrangement of the mechanism is such that the coal is practically level in the retort, a fact which the gas manager looks on as important. There are many very ingenious devices incorporated in the design of this machine, which we have described in so elementary a manner, but to make them clear we should require somewhat elaborate illustrations. All charging operations are performed by means of a single lever. Having charged one retort, the machine is run along the lines of rail to the next retort, and so on through the whole range.

Having described the main outline of the charging machine, the action of the drawing machine hardly needs explanation, the two being so like in principle. In both the mechanism for raising the pusher or rake, respectively from the coal or coke so as to clear them, is very ingeniously devised, compared to hand labour. The saving in time and labour is considerable, as the machine will charge forty-eight retorts in an hour under favourable conditions. Not half the number of men are required in the retort-house; and it is said that the saving which this represents, averages about a shilling per ton of coal carbonised. As, roughly, about 8,000,000 tons of coal are annually used for gas-making in this country, it will be seen that the universal use of these machines should lead to a saving of £400,000 every year, to say nothing of relieving the working classes of exhausting and by no means elevating labour. It is, however, worthy of note, as indicative of the spirit of the age, that it was strikes, or the fear of strikes, that led to the more general introduction of these labour-saving appliances.

In the long discussion that followed the reading of the paper, the most notable point was the testimony of experienced persons as to the success of these machines.

Mr. James Riley's paper, on modern steel works machinery, was a valuable contribution to the published knowledge on this subject. Mr. Riley has taken a prominent position in the manufacture of mild steel from the time the material was introduced commercially, and he therefore speaks with authority. He was connected with the now almost classic Landore Works under Sir William Siemens, but it was as head of the Steel Company of Scotland that he made his name most widely known: indeed, there is no one to whom naval architects and ship constructors owe more than to the author of the paper for what has been done in the development of the steel-plate industry. Mr. Riley has recently found a new field for his energies, and it was largely in the description of the plant which he has been fitting up, that his paper dealt.

Some of the most impressive examples of the mechanical engineer's art are to be found in the modern steel works of this country. Massive cogging-mills, which will roll down an ingot of ten tons of steel, almost at a white heat, into slabs: hydraulic shears which crop off the ends of these slabs, cutting through a thickness of 12 inches and a width of 5 feet of glowing steel; the enormously powerful hydraulic forging presses—the casting for the cylinder alone, in an instance mentioned by Mr. Riley, weighing 64 tons; the plate mills, rail mills, hot saws, the live rollers and hydraulic turning gear, which deal with many ton ingots of steel as if they were but playthings; all these form an exemplification of artificial force hardly surpassed. The paper in question gave descriptions in detail of the most recent examples of these machines, which it would be of interest to repeat; but the difficulty of making the forms of construction clear without the diagrams shown on the walls, will compel us again to confine ourselves to mere outline. In a cogging mill described and illustrated, slabs up to 60 inches wide could be produced, and these are rolled on their edges by vertical rolls, the ordinary horizontal rolls being used for rolling on the flat. Ingots and slabs are taken to and from the mill by special carriages actuated by hydraulic rams. Hydraulic slab shears, described in the paper, have a centre cylinder of 31 inches in diameter, and two side ones 22 inches each; the work being held down by hydraulic power whilst being sheared. The accumulator pressure is one ton per square inch. The table has two hydraulic cylinders, by which it is raised or lowered. Steam slab cutting shears and plate mills are also described. The author advocates the use of three-high plate mills in place of the more usual reversing mill. A three-high mill runs continuously, the work being passed forward between the bottom and middle roll, and back between the top and middle roll. The frequent reversing of the engine driving the ro thus done away with, is naturally a source of loss. Hydraulic power has also been adopted for working plate shears, the mechanism employed for actuating the blades being of the nature of a toggle arm worked from a crank shaft by levers.

A long discussion followed the reading of this paper, in which the desirability of rolling plates from the ingot, without previous cogging, was considered very fully. In America this practice is largely, indeed all but universally, followed; but the general opinion of the high authorities who spoke, appeared to be that in England, owing to the diversity of sizes of plates required, cogging into slabs was a necessary part of plate rolling. It is possible, however, that by properly apportioning mills to the description of work required, the intermediate process may in time become less universal in this

country. That, however, remains to be seen, and one must remember how difficult it is to shake trade customs, however much they may stand in the way of advancement in manufacturing processes.

Mr. Deas' paper on Clyde navigation improvements was another excellent contribution to the proceedings of the Institution, although perhaps rather of the nature of a civil than a mechanical engineering paper. We use the term "civil engineering" in its restricted but more generally accepted sense. The Clyde is probably the most artificial tidal river in the world. What man has done for the Clyde, and what the Clyde has done for Glasgow, every one has heard. Mr. Deas carries the details of the narrative a step further, showing how he built up good and enduring quay walls where the nature of the ground rendered the task one of the greatest difficulty. The most striking feature was the series of hollow concrete cylinders, sunk into the natural sand or gravel to form a foundation for the quay walls. The method of sinking was ingenious, and to those interested in these matters a perusal of the paper will be of great interest, both in regard to this and many other points.

Mr. Ellington's paper was one of great interest, as, indeed, were all the memoirs read at this meeting. The author has taken the foremost position in the introduction of the distribution of hydraulic power from a central station. The first example on a large scale was the installation at Hull, which was laid down in 1877. This was followed, after an interval of seven years, by the London scheme, which has now reached large dimensions, not far from ten million gallons of water being pumped per week at a pressure of 750 lbs. to the square inch: the mains extending over the most important parts of the metropolis. Since then the system has been applied in Liverpool, Melbourne, Birmingham, Sydney, and Antwerp; the latter city using over three million gallons per week. The latest examples are Manchester and Glasgow, where the pressure has been increased to 1120 lbs. to the square inch. It was the Glasgow scheme that Mr. Ellington chiefly described. These works have been carried out under the supervision of Mr. Corbet Woodall, acting for the Corporation. The engine-house is laid out to contain six sets of triple compound engines of 200-horse power each. There are two accumulators having rams 18 inches in diameter, and 23 feet stroke; each is loaded to 127 tons. The capacity is 57,500 gallons per hour at the standard pressure of 1120 lbs. to the square inch. The water supply is taken from the Corporation mains; in London Thames water is used. The mains are 7 inches in diameter, there being gutta-percha packing rings at the joints.

Speaking of the efficiency of the system, the author founded his remarks chiefly on his experience in London, and it was found that the average for ten years was 0·9243. The efficiency is determined by the fraction representing the ratio of the quantity of water registered by consumers' meters to the quantity pumped at the central stations. In Liverpool a still better coefficient is obtained, the efficiency being 0·9555. A Parkinson meter is used by the author; this is very like a gas meter. The Kent positive low-pressure meter is largely used in London.

Perhaps the most interesting part of Mr. Ellington's paper was that in which he compared the cost of hydraulic power supply and electric supply. The results were largely in favour of the water system, and were certainly somewhat surprising to many. In making this comparison data were taken from the records of the London Hydraulic Power Company and of the Westminster Electric Supply Corporation. In making the comparison 1000 gallons of water at 750 lbs. per square inch is taken as equivalent to 6·518 Board of Trade units of electricity. The analysis showed that the station cost of hydraulic power is 5·172*d.* per thousand gallons pumped at a pressure of 750 lbs. per square inch. The corresponding cost of an equivalent amount of electric energy, reduced to the same hydraulic standard, is 9·014*d.* per thousand gallons; on an electrical standard of Board of Trade units of 0·703*d.* and 1·383*d.* for hydraulic and electrical energy respectively. It was a curious coincidence that, in making this comparison, the capital outlay, output, quantity sold, and average price obtained were nearly the same; it was only in cost of production that the divergence was remarkable. A further point that came out in the discussion was that the dividends paid by the two companies respectively were not greatly different. The author could come to no other conclusion on the figures than that, from some cause not hitherto explained, hydraulic power is much less costly to produce than electricity. Prof. Kennedy, who occupied the chair, and who is so largely responsible for the distribution of electrical energy, could find no fault with Mr. Ellington's figures; but we believe the matter is likely to become the subject of further investigation.

We do not propose dealing with the many excursions that were made, and which included visits to a large number of shipyards, engine works, iron and steel works, as well as the large Corporation undertakings, such as the gas and water works. To describe these at all adequately would require a volume rather than an article. It will suffice to say here that these excursions were well attended, and the meeting was highly successful generally.

THE INTERNATIONAL GEOGRAPHICAL CONGRESS.

THE closing meeting of the International Geographical Congress took place on Saturday morning (August 3), and there seemed to be no dissentients from the opinion that in all its departments the Congress has been a great success. In particular, the meeting is to be congratulated on accomplishing much important work, and combining therewith a large amount of entertainment and social intercourse, without unduly taxing the energies of its members. While there was no reason to expect, in a scientific body like the Congress, any serious complication of interests, it is specially satisfactory to recognise the spirit which showed itself in all the sittings from day to day, and found its most definite expression in the graceful and courteous speech in which General Greely seconded the proposal that the Congress accept the invitation of the German delegates to hold the next meeting in Berlin. The Congress has not as yet met in Germany, and it was felt that a large number of members would have great difficulty in attending a meeting at Washington, although a visit to the United States offered many inducements to accept the cordial invitation which came from that country.

At the close of its proceedings the Congress gave deliverance on a number of important questions which we may take as representing the general views of geographical experts on matters of special moment in that branch of science. With regard to Africa it was agreed that it is desirable to bring to the notice of the Geographical Societies interested in Africa the advantages to be gained:

(1) By the execution of accurate topographical surveys, based on a sufficient triangulation, of the districts in Africa suitable for colonisation by Europeans.

(2) By encouraging travellers to sketch areas rather than mere routes.

(3) By the formation and publication of a list of all the places in unsurveyed Africa, which have been accurately determined by astronomical observations, with explanations of the methods employed.

(4) By the accurate determination of the position of many of the most important places in unsurveyed Africa, for which operation the lines of telegraph already erected, or in course of erection, afford so great facilities.

Resolutions were passed as to the collection and cataloguing of cartographic materials, and urging that all maps should bear the date of their publication, and the report of an influential commission appointed at Berne to consider a proposed map of the world on a scale of 1 : 1,000,000 was adopted in a form embodying a resolution that:

(1) The Commission has received the Report of the Berne Committee, and feels grateful for the work done by it.

(2) The Commission declares that the production of a map of the earth to be exceedingly desirable.

(3) A scale of 1 : 1,000,000 is recommended as being more especially suited for that purpose.

(4) The Commission recommends that each sheet of the map be bounded by arcs of parallels and of meridians. A poly-conical projection is the only one which is deserving of consideration. Each sheet of the map is to embrace 4 degrees of latitude and 6 degrees of longitude, up to 60 degrees north, and 12 degrees of longitude beyond that parallel.

(5) The Commission recommends unanimously that the meridian of Greenwich and the metre be accepted for this map.

(6) The Commission recommends governments, institutions, and societies, who may publish maps, to accept the scale recommended.

(7) The Commission lays down its mandate, and recommends

that the Executive Committee of the Congress be charged with the duty of carrying on its work, and be authorised to co-opt for this purpose scientific men representing various countries.

Support was given to the proposal for further international surveys in the North Atlantic, the North Sea, and the Baltic, by the adoption of a resolution, drawn up by a special Committee—"That the Congress recognises the scientific and economic importance of the results of recent research in the Baltic, the North Sea, and the North Atlantic, especially with regard to fishing interests, and records its opinion that the survey of these areas should be continued and extended by the co-operation of the different nationalities concerned on the lines of the scheme presented to the Congress by Prof. Pettersson."

The recommendation of the Education Committee was adopted, to the effect that—"The attention of this International Congress having been drawn by the British members to the educational efforts being made by the British Geographical Societies, the Congress desires to express its hearty sympathy with such efforts, and to place on record its opinion that in every country provision should be made for higher education in geography, either in the universities or otherwise."

Other resolutions were also carried, expressing the approval of the principle of State printed registration of literature, as the true foundation of national and international bibliography, urging the need of some agreement as to the writing of place-names, and acknowledging the scientific necessity of an international system of stations for the observation of earthquakes.

Besides the above, a number of resolutions were adopted in the course of the daily deliberations, of which the following is, perhaps, the most important of all the decisions of the Congress.

The resolution refers to the Exploration of the Antarctic regions, concerning which the Congress recorded its opinion that this is the greatest piece of geographical exploration still to be undertaken, and in view of the additions to knowledge in almost every branch of science which would result from such a scientific exploration, the Congress recommended that the several scientific societies throughout the world should urge in whatever way seemed to them most effective, that this work shall be undertaken before the close of the century.

The following is a summary of the proceedings of the Congress during the week. Previous meetings were reported in our last issue.

The general session on Monday (July 29) opened with a paper on Antarctic Exploration by Geheimrath Prof. Dr. G. Neumayer, and a discussion followed, in which the President, Sir Joseph Hooker—the only survivor of Sir James Clark Ross's Antarctic Expedition of 1843—Dr. John Murray, Sir George Baden-Powell, Mr. Arundell, M. de Lapparent, General Greely, and Prof. Guido Cora took part; and a committee was appointed to draft the resolution already quoted. The Congress then turned its attention to the Arctic regions, papers being presented by Admiral A. H. Markham, General Greely, Herr S. A. Andrée, and M. E. Payart. Herr Andrée's project for reaching the North Pole by means of balloons was somewhat severely criticised, but the author was confident of being able to meet all the difficulties suggested, and announced that he had already obtained the funds necessary for his expedition. A paper on Russian researches on a sea route to Siberia was afterwards read by Lieut.-Colonel de Shokalsky.

In the afternoon, General Annenkoff and Mr. J. Y. Buchanan presided over Section B, which dealt with papers relating to physical geography. M. le Comte de Bizemont presented a paper by M. G. Lennier on the modifications of the coasts of Normandy, and Prince Roland Bonaparte gave an account of researches on the periodic variations in French glaciers. After these were discussed, papers on the decimal division of time and angles, on the centesimal division of the right angle, on standard time, and on a system of symbolic hour zones, were read by M. le Dr. J. de Rey Pailhade, M. Louis Fabry (presented by M. Jacques Léotard), M. Bouthillier de Beaumont (presented by M. le Comte de Bizemont), and Prof. d'Italo Frassi, and a further discussion followed.

Section C, presided over by M. le Colonel Bassot and Colonel Sir Henry Thuillier, concerned itself with geodesy, and important papers were read on the geodetic operations of the Indian Survey, by General J. T. Walker, C.B., F.R.S., late Surveyor-General of India; the desirability of a geodetic connection between the surveys of Russia and India, by Colonel T. H. Holdich, C.B. (read by Colonel Sir John Ardagh); the general levelling of France, by M. Charles Lallemand, Directeur du Service du nivellement général; the rise and progress of cartography in the Colony of the Cape of Good Hope, by A. de Smidt, late Surveyor-General of that colony; and on the geodetic survey of South Africa, by Dr. David Gill, F.R.S., Astronomer-General for Cape of Good Hope (communicated by Mr. A. de Smidt).

In the course of discussion the need of surveys of the Nile Valley in connection with the South African triangulation was emphasised.

On Tuesday, July 30, the general meeting was chiefly occupied with reports, and the discussion of resolutions already referred to. Section B was devoted to oceanography, under the presidency of Dr. John Murray. Mr. J. Y. Buchanan gave a retrospect of oceanography during the last twenty years, and read a paper, by the Prince of Monaco, on the work of the yacht *Princess Alice*. A paper on ocean currents and the methods of their observation, by Captain A. S. Thomson, was laid on the table; and Prof. W. Libbey, of Princeton, gave an account of some valuable researches on the relations of the Gulf Stream and the Labrador current. Prof. Libbey's investigations have afforded some remarkable results bearing on the migrations of fish on the eastern seaboard of the United States, and they form an interesting contribution to the study of certain problems in marine zoology. A paper by Prof. J. Thoulet, suggesting that geographical societies in towns situated near the coast should interest themselves in the oceanography of neighbouring seas, was laid on the table.

Section C, presided over by Prof. H. Cordier and Prof. J. J. Rein, discussed geographical orthography and definitions. Papers were read on the orthography of place-names by Mr. G. G. Chisholm; on geographical place-names in Europe and the East, by Dr. James Burgess; and on the transliteration and pronunciation of place-names, by Dr. Giuseppe Ricchieri.

Popular interest in the Congress probably reached its highest point at the general meeting on Wednesday (July 31), when the proceedings related exclusively to Africa and its development. Sir John Kirk read a paper on the suitability of tropical Africa for development by white races or under their superintendence, dealing with the possibilities of colonisation proper, the establishment of European settlements in places permitting of temporary residence, and the means whereby the native races may themselves be taught to aid in the development of the country. Count von Pfeil laid down the conditions of success in colonising tropical Africa, which he said were chiefly a thorough knowledge of the character of the country it was proposed to colonise, of tropical hygiene, and of the art of making the native take an active share in the work. Mr. Silva White's paper dealt with the problem from various points of view, the author concluding that tropical Africa is on the whole unsuitable for European colonisation, and that it is capable of only a limited degree of development as compared with other and still undeveloped regions of the world. Mr. H. M. Stanley, Mr. E. G. Ravenstein, M. Lionel Décle, and Slatin Pasha also presented communications to the meeting, and a discussion followed. General Chapman read a paper on the mapping of Africa, and a proposal was referred to a committee whose report includes the resolution given above. A paper on a crestographic map of Africa was read by Mr. Silva White, and another by M. Victor de Ternant, on French Africa, was laid on the table.

Only one of the sections met (Section C). The Presidents were Dr. A. Gregoriev and Prof. Libbey. Oceanographical papers were communicated by Prof. Otto Pettersson and Mr. H. N. Dickson, dealing with recent research in the North Sea. Prof. Pettersson submitted a scheme for an extension of the same work, and a committee was appointed to draw up the resolution afterwards adopted by the Congress. A paper on limnology as a branch of geography was then read by Prof. Forel, and after remarks by Prof. Anuchin, Prof. Halbfass, Prof. Penck, Prof. Libbey, and M. de Krapotkine, Dr. H. R. Mill asked that his paper on "Limnology in the British Islands" be held as read. Señor F. A. Pezet gave an account of the counter-current "El Niño" on the coast of Northern Peru.

The general meeting of Thursday (August 1) opened with a return to the subject of Antarctic exploration. Mr. C. E. Borchgrevink, who had been unable to reach London in time for the meeting on Monday, read a paper on his voyage in the *Antarctic* to Victoria Land. Prof. C. M.

Kan read a paper on Western New Guinea, and future exploration in Australia was discussed by Mr. David Lindsay. A memoir on the Niger lakes, by M. Paul Vuillot, was laid on the table, and one on explorations in Madagascar, by M. E. F. Gautier, was communicated in abstract. In the absence of M. Maistre, who was to have read a paper on the hydrographic system of the Shari and Logone, Señor Don Torres Campos gave an account of the climatology of the Portuguese and Spanish colonies on the west coast of Africa.

Section B—Presidents, M. Levasseur and Mr. Ravenstein—received the following papers:—On the construction of a terrestrial globe on the scale of 1 : 100,000, by Prof. E. Reclus; on the construction of globes, by Signor Césare Pomba; the life and geographical works of Cassini de Thury, by M. Ludovic Drapeyron; an ethnographical map of Europe, by Herr A. von Haardt.

Prof. de Lapparent, Dr. John Murray, and Prof. Penck presided over Section C, where Prof. Palacky read a paper on the geographical element in evolution; Dr. E. Naumann, one on the fundamental lines of Anatolia and Central Asia; Dr. S. Passarge, a third on laterite and red earth in Africa and India; and Mr. Henry G. Bryant, a fourth on the most northern Eskimos. The last paper described observations made in North and South Greenland during the Peary Relief Expeditions.

On Friday (August 2) the President communicated a paper to the general meeting, by Baron A. E. Nordenskiold, on ancient charts and sailing directions. Prof. Hermann Wagner read a paper on the origin of the mediæval Italian nautical charts, which gave some interesting results as to the length of the mediæval nautical mile. Mr. Yule Oldham dealt with the place of mediæval manuscript maps in the study of the history of geographical discovery, and, in the course of remarks on this paper, Mr. Batalha-Reis announced the discovery of an authentic fifteenth century portrait of Prince Henry the Navigator, at Lisbon. The Congress received a number of presentations, and discussed various proposals and resolutions.

Section B—Presidents, Señor Don Torres Campos and M. le Prof. Levasseur—dealt with spæliology (or the science of caverns) and mountain structure. A paper on the method of investigating caverns, by M. E. A. Martel, was read; M. F. Schrader described new instruments and methods used in surveying the Pyrenees; and Prof. Rein gave an account of observations in the Spanish Sierra Nevada.

Dr. F. Naumann occupied the chair in Section C, in which Prof. Penck read an important paper on the morphology and terminology of land forms, and communications were received from Mr. Batalha-Reis on the definition of geography, and Prof. Gerland on earthquake observations.

On Saturday only a general meeting was held. General Annenkoff read a paper on the importance of geography in connection with the present agricultural and economical crisis, and the rest of the time was occupied with resolutions and reports. The President dissolved the Congress in a short concluding address, and bid the foreign visitors a hearty farewell.

After such well-filled days the Congress wisely devoted most of its evenings to recreation. Only two exceptions were made. On Monday night Prof. Libbey showed by the lantern a large number of photographs made in the north of Greenland; and on Thursday Dr. H. R. Mill gave a demonstration in the form of a lecture on the English lakes.

THE BRITISH MEDICAL ASSOCIATION.

THE sixty third annual meeting of the British Medical Association, held in London last week, was the largest in the history of the Association, and one of the greatest assemblies of medical men ever known. Twenty-two years ago the Association held its annual meeting in London, but whereas at that time the membership was only 1500, the number now exceeds 16,000. A large number of foreign medical men were present at the meeting, among them being Prof. Stokvis, Dr. W. W. Keen, Dr. Apostoli, Prof. Mosso, Dr. Fraenckel, Dr. Farkas, Prof. Pozzi, Dr. Ottolinghi, Prof. Lazarewitch, Prof. von Ranke, Prof. Baginsky, Dr. Hermann Biggs, Dr. Ball, Dr. Koster, Prof. Gayet, Dr. Meyer, Prof. Panas, Prof. Fuchs, Prof. Bowditch, Dr. L. A. Nékám, Prof. Baumler, Prof. Martin, Dr Cushing, Prof. Cordes, Prof. Hamburgher, Prof. Marinesco, and Prof. Geikie. Sir T. Russell Reynolds therefore presided over an assembly international in its main aims, and representing an Association as remarkable in its growth as it is high in its standing. It is only possible here to give a few extracts from some of the addresses and refer briefly to a part of the general work of the sections. For these reports we are indebted to the *British Medical Journal*, the organ of the Association. Sir T. Russell Reynolds took for the object of his address "the most striking fact of modern physiological, pathological, and therapeutical research, viz. the power of living things for both good and evil in the conservation of health and in the prevention or cure of disease." In the course of his remarks he said:—"The most important fact with regard to recent microbiological research is the gradually-increasing appreciation of the fact that these lower forms of life exert, not necessarily mischievous, but, indeed, benignant influences on the human body, and that although the mode of their operation is not fully explained they take part in healthy processes, assisting normal functions, nay, indeed, it would seem sometimes producing them and warding off the malign effects of other influences to which we are habitually exposed. These bodies, to which we are indebted for this aid, operate partly by their chemic action and partly by what we must call a vital process, and by their cultivation outside the human body and their modification by passing through other organisms, can be made to exert a malign or a beneficial agency on man. It seems even in the range of possibility that at some time not very distant some other than 'the ancient mariner' may apply to them the far-reaching words of Coleridge, and exclaim

> O happy living things! no tongue
> Their beauty might declare;
>
> * * * *
>
> Sure my kind saint took pity on me
> And I blessed them unaware.

"The third great revelation of the last twenty years is the wonderful protective and curative power of these living products. This, in a very wide sense, is not new. Of all the most powerful agents of destruction, the most violent have been derived from 'living' things; they are to be found in the animal and vegetable worlds, not in the mineral. In their most terrible malignity—such as in snake-bite, glanders, or hydrophobia—these need no human skill for their development: they are prepared in the laboratory of nature, and, alas! are only too ready to our hand. Next to these come the poisons of stinging things, and, after them, the more slowly operating and less deadly animal infections; some with indeed beneficial influence, as 'vaccinia'; others with local effects on the skin, but not often great disturbance of the general health.

"The vegetable kingdom can produce potent poisons, such as belladonna berries, aconite root and leaves, poppy juice, and the ignatian bean; but in order to render these more deadly the hand of man has to come in and prepare nicotine, strychnine, morphine, and the like; just as it may produce, from the mineral or quasi-mineral world, such potent agents as hydrocyanic acid, concentrated acids, and other dealers of destruction.

"The interest in these facts lies in the modern mode for their utilisation. The great potency of living products has led to very fanciful notions in therapeutics; and there have been those who, to cure diseases of organs, have given portions of the same but healthy organs of animals or of man or other animals. Again, the idea has been pronounced that even excreta were useful drugs, and that the diseased organs of man might effect a cure of those supposed to be afflicted in like manner.

"Curious as some of these details are, they are of real interest to us only as they lead up, through inoculation for small-pox, to our own Edward Jenner's discovery of vaccination, and then, through the researches of Pasteur, Lister, and Brown-Séquard, to our present state and plane of knowledge. It would seem now that there is scarcely any limit to what may be expected in the cure or prevention of disease; and the most striking of all phenomena is, to my mind, the probability of rendering an animal immune by the introduction into its organism of a healthy constituent of the body of another. This, if fully confirmed, will be the greatest veritable triumph of therapeutic and preventive medicine, instituted and guided by extended inquiry into comparative anatomy, physiology, and pathology. As in the human race or species there exist, as is well known, what may be termed 'idiosyncrasies'—by which is simply meant that as a matter of fact some people, and some people's families, escape epidemic diseases, whereas they are especially prone to take others to

which they may be exposed—so in the great economy of Nature certain groups of animals have been shown to exhibit no capacity for 'taking,' or for even being 'inoculated' with the poisons to which others are exposed, and from which they suffer, and that severely. It would seem, therefore, that use may be made of these animals, more or less naturally immune from certain maladies, and that their immunity may be partially conferred on man.

"Quite recently a communication of the greatest importance has been made on the rendering of animals immune against the venom of the cobra and other snakes, and on the antidotal properties of blood serum of immunised animals. This subject has occupied attention during the last six years, and we must all look forward with expectancy and hope to the possible and probable diminution of a great national and imperial calamity.

"The outcome of what I have been saying is this: that the scattered fragments of knowledge and 'guesses at truth' of many years have been gathered into a focus during the past twenty-five years; that the vegetable life, extracting from the mineral world the materials it needs for growth and production of powerful agencies for good in the form of food and medicines, and for evil in the form of poisons, has given itself up to the growth of animal life, with its much more complex organs, and for cure of ills once thought beyond the reach of human aid; but that, thanks to man's scientific ardour and industry, it has again shown itself to be our servant, our helper, and our protector.

"These are not dreams of the study, they are facts of the laboratory and of daily life; and in using that word 'life' again, I must endeavour to emphasise still more forcibly upon you my urgent belief that it is to living agencies and their employment that we must look for help in the care of infancy, the conduct of education—moral, mental, and physical—the training up of character as well as of limbs; that it is the guidance of living functions, in the choice of living occupations, be they either of hard work or of amusement. It is to these we must appeal if we would see the *mens sana in corpore sano*; and then it will be to these that we may confidently look for help when the inroads of age or of disease are at hand, often to cure us of our trouble; or, if not, to give us rest and peace.

"It would be absurd in me, now and here, to attempt to say in what this potency of life exists. It is enough for us to recognise its existence, rejoice in its marvellous energy, and anticipate still more from our investigations of its modes of action, but I cannot help feeling that, however far we go in our research into the arcana of nature, one of our ablest neurologists, who has gone very far, is right when he says: 'Search while you may with eyes, however aided and however earnest, that which we call "life," eludes our search and resists our efforts. We must be content with what knowledge we can gain, secure or insecure, and while using it as best we may, should realise in all humility how much there is we cannot know, and yet we cannot doubt.'"

An address in medicine was delivered by Sir William Broadbent, who traced the growth of the art and science of medicine. He pointed out that of the infancy of medicine properly speaking nothing is known.

Individual acts of healing are related in the Old Testament, and the treatment of wounds is described by Homer; the Chinese from remote antiquity had a system of medicine, and medicine has a place in the *Vedas*; but in the works of Hippocrates, who was born about 460 B.C., the earliest medical literature which has been handed down, the theory and practice of the art of healing is shown in a considerably advanced stage of development. The development of medicine from that time was sketched by Sir W. Broadbent in an admirable address, and the great advances made during the present century in the many departments of his subject were touched upon. In one of the sections, the excellence and defects of modern therapeutics were passed in review as follows:—

"We have still to ask, What is the bearing of all these advances of knowledge on therapeutics, which, after all, is the object of our lives?

"Until the last few years it has not been easy to answer this question by instances of any very extensive applications of physiology to the treatment of disease, and morbid anatomy was at one time a stumbling-block in the way of therapeutic effort. The pathologist, pointing to an excavated lung or cirrhosed liver, would ask the physician what he could expect to do with drugs against such conditions. But that stage has passed away, and I will not mock your intelligence by other illustrations beyond those just given of therapeutic applications of physiological and pathological knowledge, or by arguing that all knowledge of normal processes aids in the comprehension of morbid processes, and that we are in a better position to combat disease when we thoroughly understand its causation and initiation, and follow mentally its development, course, and tendencies.

"Given the faculty of observation, the insight which penetrates the meaning of the phenomena, the analytical and synthetical powers by which a diagnosis is constructed, the ready adaptation of means to a well-defined end, and the firmness of character required to deal with the frailties of human nature, and the best physiologist will make the best pathologist and the best pathologist the best physician.

"As regards the remedies at our command, they are only too numerous. Recourse to a great variety of drugs is fatal to exact knowledge of their effects and to precision in their use, but new ones are added every day for the benefit chiefly of those who do not know how to employ the old ones. There have, however, been recent acquisitions of extreme value, heavily discounted, unfortunately, in the case of some by the mischief done through their indiscriminate use: the antiseptic group, the chloral sulphonal group, the salicylates and salicine, the phenacetins and antipyrin class, coca and cocaine. What makes some of these, moreover, far more important and interesting is the fact that their physiological action has been inferred from their chemical constitution.

"A fact which brings practical therapeutics into near relation with physiology and pathology is that the active principles of all drugs are isolated, their chemical composition is ascertained, and their physiological action investigated. Pharmacology, in effect, has become a branch of experimental physiology, and the immediate effect of remedies is known with a completeness and accuracy heretofore undreamt of. All this is working towards a more intelligent employment of drugs, and leads towards the goal of all the efforts to bring therapeutics within the circle of the sciences. This goal is that we should know not only the effects of remedies, but how these effects are produced. This is in the last resort a question of chemistry. As I have said before, all vital actions are attended with molecular or chemical changes; are, from one point of view, chemical action, and come under the laws of the correlation of force and conservation of energy; so, therefore, are the physiological and therapeutical action of drugs, and obviously the key to the latter is to be found in the chemistry of vital processes. Therapeutics, to become scientific, is only waiting for answers to the questions which she puts to chemistry. Why are sodium salts so much more abundant than potassium salts in the blood, and why are the former almost confined to the liquor sanguinis, and the latter to the corpuscles? We must assume that albuminoid proteids have an affinity for sodium, and the globulins for potassium. With the answer to this is bound up the secret of the necessity of sodium, potassium, and calcium salts to anabolic and catabolic operations, in which they take no traceable part, and of the presence of iron in the blood corpuscles.

"Why, again, in the case of substances apparently so similar as potassium and sodium salts will the former, if injected into a vein, even in small quantity, paralyse the heart and destroy life, while we see pints of normal saline solution thrown into the circulation with none but good results? How does prussic acid—the simplest in composition and constitution of all organic substances—prove fatal with such fearful promptitude by its presence in infinitesimal proportion in the blood? How again does morphine suspend the activity of the nerve centres? Chemists must admit that the poisonous effects of prussic acid and morphine can only be due to some molecular change in these substances; they know that if the deadly cyanogen is so tied up that its component atoms cannot fly apart it is innocuous, and that a very slight change in the chemical constitution of the morphine molecule entirely alters its effect; it is an almost irresistible inference from the doctrine of conservation of energy that the change in the molecule, say of the morphine, must be equal and opposite to the molecular change in the nerve cells which it arrests. It seems to me, therefore, that we have in the chemical constitution of the morphine molecule a clue to the character of the chemical change by which nerve action takes place and to the quantivalence of nerve energy.

"What then is our position to-day in respect of the three points which we have been following—the recognition of disease, the

knowledge of remedies, and the ideas which govern the employment of remedies in the treatment of disease?

"The basis of therapeutics is diagnosis, the grasp of the actual condition underlying the symptoms or phenomena, and the greater our command of powerful remedies and the more precise our knowledge of their effects and of the mode in which these effects are produced, the more important does accuracy in diagnosis become.

"A diagnosis, to be real, implies not only the recognition of the disease which may be present and an accurate appreciation of the morbid changes which may have taken place in various organs. It embraces a knowledge of the nature and intensity of the pathological processes which have been and are in operation, and of the causes which set them going, and also of the results to which they tend. A further element, moreover, enters into the consideration; an estimate, by the aspect of the patient, by the pulse and temperature, and by other subjective and objective indications, of the impression made on the system, and of the resistance which it is capable of to the lethal tendencies of the disease.

"Year by year we see improvement in this respect; not only that hospital physicians and teachers endeavour to carry diagnosis to a greater pitch of accuracy and a higher point of refinement than ever before, but that the entire body of medical men are trained by improved education and systematic clinical teaching to appreciate and to practise careful diagnosis in their daily work.

"Diagnosis, we may say, has reached an extraordinary degree of advancement. There are, no doubt, still new fields to conquer, but in the recognition of diseases, local and general, there is not much which seriously concerns the human race which remains to be done. The same degree of knowledge, however, does not extend to morbid processes. Our comprehension of the significance and essential character of inflammation is by no means complete and satisfactory. The part which fever plays and the place which it holds among the phenomena of disease is far from being fully understood. It cannot have been intended by nature for the destruction of the subject, and we can see distinctly that in some cases it forms part of the defensive operations; possibly, indeed, its general tendency is defensive, by promoting the production of phagocytes, or possibly a certain elevation of the temperature may be fatal to maleficent organisms which have taken possession of the blood or tissues. We are not certain, indeed, whether in pyrexia the heat-producing oxidation in the structures receives its stimulus from, or takes place at the bidding of, the nervous centres, or, on the other hand, is due to enfeeblement of the restraint which they normally exercise over it, or whether it defies control by the thermo-taxic nervous centres."

An address in surgery was delivered by Mr. Jonathan Hutchinson, who gave a brief retrospect of the surgery of the past, interspersed with a few comments as to what may be hoped for the future.

Prof. Schafer delivered an address in Physiology, taking for his subject "Internal Secretions." After describing various glands and secretions and their method of interaction, he said: The general results to which we are led point very strongly in favour of the notion that internal secretions are yielded both by the ductless glands and by what are usually known as the true secreting glands, and it is obvious that such internal secretions may be of no less importance than the better-recognised functions of the external secreting glands. That a failure of one or other of these internal secretions has to be definitely reckoned with by the physician there can be no doubt whatever, while at the same time the therapeutist will be able to avail himself of the active principles which the internally secreting organs afford, and in certain cases to use their extracts in place of the hitherto more [illegible] employed vegetable medicaments.

The work of the different sections covered a wide range, and [illegible] related purely to medical practice. It will be [illegible], therefore, for us to indicate by the following summary [illegible] of a few of the more important papers [illegible] reported in the *British Medical Journal*.

Section of Medicine.

The President, Dr. Pavy, opened the proceedings in this Section with an address in which he described the progress in [illegible] discovery of the casual relationship existing between [illegible] and certain diseases, enlarging upon the [illegible] effect that this had had upon the question of treatment, and upon the control that could be exercised upon the spread of infectious diseases. He briefly touched upon the serum treatment of diphtheria. Dr. Sidney Martin then introduced the discussion on diphtheria and its treatment by the antitoxin. Dr. Martin commenced by stating that there had always been two schools of therapeutists with regard to the treatment of diphtheria, the one trying to discover some local application which would loosen or remove membrane in the throat, and the other to provide a remedy that would act upon the general symptoms of the disease. The want of success in the past made it essential, in his opinion, to examine most carefully into any new method of treatment suggested, and to submit it to a most rigid scientific inquiry before accepting it. The antitoxin treatment, he stated, had been studied with the greatest care, and its recommendation was based upon the results of a consideration of the pathology of the disease.

Prof. von Ranke (Munich) stated that whilst in 1892 he had in his hospital a mortality of 56·2 per cent., in 1893 of 46 per cent., and in 1894 up to September 24, when he had commenced the serum treatment, one of 57 per cent., since that time his death-rate had been reduced to 17·7 per cent. He further considered that not only was the reduced death-rate due to the injection of antitoxin, but that the course of the disease was favourably influenced in the most striking manner. Prof. Baginsky, of the Empress Frederick Hospital, Berlin, though not speaking with the high enthusiasm of Dr. Ranke, yet gave equally startling figures, stating that whilst the mortality in the four years previous to 1895 had been on the average 41 per cent. under the old system of treatment, during the last year, under the serum treatment, it had been reduced to 15·6 per cent. Dr. Sims Woodhead spoke briefly upon the importance of using large doses of serum, and concluded by quoting some Paris statistics which were highly favourable. Dr. Hermann Biggs (New York) then gave a most interesting account of the immunising effect of the serum, quoting figures to show that in almost all cases the immunising power of the serum extends to a period of thirty days. He further stated that out of 800 healthy children who had received injections, he had not seen a single case in which any harm had resulted from the treatment.

Section of Surgery.

Sir William MacCormac, President of the Section of Surgery, took for the subject of his address "Some Points of Interest in Connection with the Surgery of War." He came to the following conclusion:—

"It would appear probable that in a future war many of the wounds produced by the new projectile will be surgically less severe, and prove amenable to effective surgical treatment. Probably also the number of severe injuries will be very great when we consider the enormous range of the new weapon and the penetrating power of the projectile, which enables it to traverse the bodies of two or three individuals in line, including bones, and to inflict serious or fatal wounds at a distance of 3000 or 4000 yards. It is impossible to say what the proportion between these two is likely to be. At near ranges the explosive effects will be much the same as before; but at long range the narrow bullet track, the small external wounds, which often approach the subcutaneous in character, and the moderate degree of comminution and fissuring of the bone will be surgically advantageous. These will form the bulk of the gunshot injuries of the future, for it would seem impossible with magazine quick-firing rifles to maintain a contest at close quarters without speedy mutual annihilation.

"We may take it for granted that the number of wounded, in proportion to the numbers engaged and actually under fire, will be greater than before. The supply of ammunition will be larger, the facility for its discharge greater, and smokeless powder will increase accuracy of aim.

"I think we are justified in believing, although there is high authority for a contrary opinion, that the next great war will be more destructive to human life, 'bloodier,' in fact, than any of its predecessors; and that the number of injuries, and in many cases the severity of the injury, will be largely increased. But very many cases will remain less severe in character, more capable of successful treatment, and less likely to entail future disablement, while improved sanitation and antiseptic methods will enormously increase the proportion of recoveries.

"It is the unceasing effort of modern surgery to provide antiseptic protection in an effective form in time of war; and I may be permitted to recall that the medical organisation during our

last war in Egypt was so complete in this respect that not a single case of infective wound disease occurred during the whole campaign.

"As a temporary dressing, some form of antiseptic occlusion will prove most generally applicable. The small wounds of entrance and exit render this plan comparatively easy of application, and the chances of septic infection will be diminished by the less frequent necessity for probing or searching for a lodged projectile, and, indeed, the ascertained presence of the bullet is no sufficient indication *per se* to attempt its removal. The eye, rather than the hand, is the best thing to employ at a first dressing station, as Fischer has well said.

"If only asepticity can be ensured—and this is the great difficulty—we may expect a large measure of success to follow the treatment of wounds of the soft parts, many forms of fracture—notably also wounds of the joints, and very especially wounds of the lung."

Section of Public Medicine.

The proceedings in this Section were opened by Mr. Ernest Hart, who delivered an address on "Public Health Legislation and the Needs of India." Mr. Hart strongly criticised the whole system of the sanitary service and the medical service of India, and held that it needs to be overhauled and reconstituted.

"What is urgently needed," he said, "is a Royal Commission or strong Departmental Committee to inquire into the whole matter, and to institute a radical change. For at present India is decimated by preventable diseases; the health of our troops is ruined by the same causes. With us lies the reproach of nursing and fostering cholera in what is called its endemic home—a purely ignorant and silly phrase. Until some great change is made in the whole system of the present administration, the great sanitary needs of India will never be met."

Section of Pharmacology and Therapeutics.

In this Section, under the presidency of Sir William Roberts, there was a discussion on Sero-Therapeutics, embracing the application of serum treatment, not only to the acute infective disorders, but also to the cure of bites from venomous serpents. In his introductory remarks the President drew attention to a hitherto much neglected alkaloid of opium, generally known as "narcotine," but more properly termed "anarcotine," from the complete absence of narcotic properties. A large amount of evidence was available which seemed to show that this alkaloid has very valuable antiperiodic powers, which, should further investigation corroborate, will render it a valuable remedy in certain cases of malaria in which quinine entirely fails. The discussion on Sero-therapeutics was opened by Dr. Klein in a paper on the nature of Antitoxin. He drew attention particularly to the differences in action between a protective serum obtained from animals immunised by injections of filtered diphtheria toxin, and by those treated with living cultures of the diphtheria bacillus. He had found that while the first had an extremely high neutralising power on the chemical poison separated from the bacilli, it had not nearly so marked an immunising power. On the other hand, an antitoxin prepared with the aid of living cultures, while it was less active than the other in neutralising toxins, was far more efficacious as an immunising agent. He also gave brief hints on the advantage of using a dried serum in place of the usual liquid form, and stated that the use of the former was far less likely to be followed by the appearance of rashes and other complications.

Other Sections.

Dr. Mickle, President of the Section of Psychology, delivered an address on the abnormalities occurring in the form and arrangement of brain convolutions. The Section of Physiology was opened by Dr. Ferrier with an address on the relations of physiology and medicine. In the Section of Anatomy and Histology, Mr. Henry Morris, in his presidential address, gave a brief history of the rise of artistic illustration in its relation to anatomical teaching.

The presidential address in the Section of Pathology and Bacteriology was delivered by Dr. Samuel Wilks, F.R.S. In the course of his remarks he drew attention to the fact that every pathological process is accompanied by a corresponding reparative process, and lamented that sufficient regard had not been paid to the distinction between these constructive and destructive processes. To study these for the sake of discovering the several influences exerted in the production of each is of great practical import; and a consideration of them also shows that pathology is governed by the same laws as those which exist in every other department of nature, and therefore must take its place on an equivalent footing with the other sciences.

Mr. H. Power, the President of the Section of Ophthalmology, remarked on the work that had been done by the founders of ophthalmology in the past, and the gradual formation of a scientific branch of medicine, of which the methods of diagnosis and treatment were fortunate in being founded on pure science. Owing to its intimate relations with the other branches of medicine and surgery there was no danger of its separating from the parent stem and becoming barren; at the same time he advocated a sounder education in the sciences on which ophthalmology was established, such as mathematics and physics, being required of all candidates for ophthalmic posts in hospitals.

Bacteriological Exhibits.

A collection of exhibits brought together to illustrate points of general pathological interest was on view during the meeting. Bacteriological exhibits made up one of the departments of the temporary museum thus formed. Dr. Cautley exhibited cultures and coverglass preparations of an organism found in seven out of eight cases of the affection usually termed influenza cold. It was of special interest and importance as showing, first, that the disease in question is microbial in origin, thus explaining the frequency with which such colds affect all the members of a household; secondly, that it possesses a certain relationship to epidemic influenza. The biological characteristics indicated that the organism is allied to the organism of epidemic influenza. Morphologically the organism presented a further point of interest, many club-shaped forms, similar to those of the diphtheria bacillus, appearing in the specimens. Some excellent photographs of the specimens accompanied the exhibit, and were taken by Mr. E. C. Bousfield.

The cultivations from the laboratories of the Conjoint Board of the Royal College of Physicians, London, and of the Royal College of Surgeons, England, were permanently fixed by formic aldehyde. This substance arrests the growth almost at once, and after the lapse of two or three days kills the bacilli. Various organisms in culture illustrated this method, and showed its applicability to museum and other specimens.

Drs. MacFadyen and Hewlett exhibited from the Bacteriological Department of the British Institute of Preventive Medicine a complete series of cultures of the most important micro-organisms, and Mr. Joseph Lunt exhibited living cultures of various water organisms isolated from drinking water, sewage, air, &c., together with some interesting instances of enzymes filtered from both cultures of various organisms, possessing liquefying and other properties similar to those possessed by the parent organisms.

Dr. Klein showed a large number of photographic lantern slides representing nearly all known pathogenic bacteria, and, amongst others, duplicates of Mr. Bousfield's work for the influenza and cholera reports, the latter especially showing vibrios with their flagella with wonderful clearness.

SCIENCE IN THE MAGAZINES.

FOUR short papers on Huxley appear in the *Fortnightly Review*. The Hon. G. C. Brodrick, Warden of Merton College, Oxford, records some personal reminiscences of the man whose loss is so keenly felt. It appears that about thirty-seven years ago, when a Linacre Professorship of Physiology, coupled with Human and Comparative Anatomy, was founded, Huxley meditated becoming a candidate for the chair. Before the election took place, however, he made up his mind not to seek the office, which was awarded to the late Prof. Rolleston. The reason he assigned was that his opinions were too little in harmony with those prevalent at Oxford. This opinion he again gave, but with diminished emphasis, when he was asked, twenty years later, to accept the chair, upon the death of Prof. Rolleston. His work for the advancement of anthropology forms the subject of a note by Prof. E. B. Tylor. "Close upon the end of his life," says Prof. Tylor, "Huxley did his best to promote the scheme to make anthropology at Oxford an examination subject for an Honours degree in Natural Science. Writing to me, he said, "If I know anything about the matter, anthro-

pology is good as knowledge, and good as discipline.' But Convocation thought he did not, 'know anything about the matter,' and threw out the proposed statute." Huxley's career as biologist is sketched by "A Student of Science." The following is worth quoting from that contribution. "It was characteristic of the Professor's general mental attitude that mere novelty never affrighted him. When Ramsay propounded his theory of the excavation of lake basins by glacial action, Huxley supported it, even against the opposition of Lyell and Falconer. Suppose St. Paul's Cathedral removed from its present site to any part of the North Sea, the English Channel, or the Irish Sea, and the whole dome would be clear out of water. Place it, on the other hand, on the flow of Loch Lomond, and the largest ship in the British Navy might float safely over the golden ball, for the Loch has a maximum depth of 630 feet. Sir Andrew Ramsay's theory explains a striking fact like this, and affords undoubtedly a rational explanation of many similar phenomena." The fourth of the papers treats of Huxley as philosopher, and is by Mr. W. L. Courtney, the editor of the *Fortnightly*. Under the title "The Spectroscope in Recent Chemistry," Mr. R. A. Gregory contributes to the same review a brief history of the discovery of argon and helium, and discusses the many interesting points raised by the advent of those two new terrestrial elements, especially with reference to their spectra. It is worthy of contemplation that, so far as instrumental possibilities go, both argon and helium could have been discovered spectroscopically many years ago, and Lord Rayleigh would have been saved his years of tantalising experimentation. And yet there are some who think that the spectroscope will not help much more in the extension of natural knowledge!

The evolution of the orator and poet, actor and dramatist, is traced by Mr. Herbert Spencer in his fourth paper on "Professional Institutions," which appears in the *Contemporary*. First in his story of development comes the orator, who proclaimed the great deeds of a victorious chief during the triumphal reception; then was evolved, through natural selection, the poet, who, with picturesque phrases and figures of speech, gave rhythm to the laudatory speeches. Gradually the orator or poet joined with his speeches mimetic representations of the achievements of the living or the apotheosised ruler, or else they were simultaneously given by some other celebrant. So the actor was produced, and as more complex incidents came to be illustrated by speech and action, it was necessary for one to arrange the parts to be played, and thus the dramatist was developed. In support of this very natural sequence, Mr. Spencer adduces a variety of evidence supplied by uncivilised races and by early civilised races. Another paper in the *Contemporary* consists of extracts from Mr. E. A. Fitzgerald's journal of his ascents of virgin peaks in the New Zealand Alps. Five new peaks were ascended, namely, Sealy, Silberhorn, Tasman, Haidinger, and Sefton, the Matterhorn of the range. He also discovered a pass which has received his name, and across which the range has now been traversed to the west coast. Several attempts had previously been made to find such a route, but unsuccessfully. Mr. Fitzgerald's paper will therefore not only be read with interest by lovers of Alpine adventure, but will also be valued by the geographer.

The story of Antarctic exploration is told in *Macmillan's Magazine*, and the movement for further researches in those higher southern latitudes is given support. It will be remembered that the efforts made by the Royal Geographical Society, in connection with a committee of the Royal Society, to induce the Government to fit out an expedition for exploring in the Antarctic Ocean, were not successful. Notwithstanding this, the writer of the article expresses the general opinion when he says: "When it is undertaken at all it is desirable that the next Antarctic expedition should be a national one. Private enterprise, which has been splendidly active of late in the way of Arctic discovery, would scarcely be equal to all the demands of extensive and thorough Antarctic exploration."

A passing notice must suffice for the remaining articles of more or less scientific interest in the magazines and reviews received. A brief sketch of the characteristics of Sonya Kovalevsky is given in the *Century*, and one of the concluding sentences read: "Notwithstanding her solid contributions to applied mathematics, she originated nothing: she merely developed the ideas of her teachers." A number of elementary facts with reference to the transporting power of water and the deposit of sediment, are stated by Mr. W. H. Wheeler in *Longman's Magazine*. The *National* contains an article, by Mr. J. L. Macdonald, on fruit-farming in California, which is worth the attention of agriculturists. In the *Quarterly Review*, roses and rose cultivation are surveyed, though more from an historical than a scientific point of view. An *Edinburgh Reviewer* discusses organic variation and animal coloration, basing his remarks upon Mr. Bateson's "Materials for the Study of Variation" and Mr. F. E. Beddard's "Animal Coloration." In *Good Words* we find an illustrated article by Dr. Bowdler Sharpe, on curious nests of birds, and a paper on the Earl of Rosse and his great telescope, by Sir Robert Ball. *Chambers's Journal* contains, among other instructive articles, one on the U.S. North Atlantic Pilot Chart, and another on "Taka Joli," a new substitute for yeast. Finally we have to acknowledge the receipt of *Scribner's Magazine*, the *Sunday Magazine*, and the *Humanitarian*.

PHOTOMETRIC STANDARDS.

THE following Report of the Committee appointed by the Board of Trade, in December 1891, "to inquire into and report to them upon the subject of the standards to be used for testing the illuminating power of coal gas," has just been published as a Parliamentary paper.

"(1) It was intimated to us, by a letter from the Secretary to the Board, that the method at present in use for measuring the illuminative value of coal gas has been objected to, alike by the Metropolitan Gas Referees and the London County Council, as being of an unsatisfactory nature: that the London Gas Companies are alive to the defects in the present system; and that legislation is admittedly necessary for the purpose of substituting a more trustworthy standard for that now in existence; but that, in view of the difference in opinion as to what the substituted standard should be, the President of the Board deemed it advisable that, before his support was given to any legislation, the whole question should be considered by a Committee that would command the confidence of the various interests affected.

"(2) The method at present in use for measuring the illuminative value of coal gas consists in comparing the light of the gas, when burning from a particular burner at a specified rate, with the light of a sperm candle burning also at a specified rate, which last is taken as a standard. We have satisfied ourselves, from considerations set forth in the Appendix to this Report, that the flame of a sperm candle does not furnish a satisfactory standard, by reason of the amount of light which it affords varying over a wide range, under conditions as to the manufacture of the candle, as to its mode of use, and as to adventitious circumstances attending its use, which, as a whole, it is not possible to regulate and define.

"(3) Though recognising, however, that the sperm candle flame does not furnish a satisfactory standard, we nevertheless consider it advisable that, in official documents and reports, the quantity of light yielded by coal gas burned under specified conditions should continue to be expressed as heretofore, in terms of candle-light, the actual comparison, however, being made between the gas-light and some well-defined and constant light ascertained to be equal in quantity to, or a definite multiple of, the average light given by the standard sperm candle.

"(4) We have further come to the conclusion that, in the present state of experience and knowledge, the source of the light to be used as a standard by gas-testers generally must be produced by the process of combustion, and be in the nature of a flame.

"(5) We find that the one-candle-light flame proposed by Mr. A. Vernon Harcourt as giving a standard light, and commonly known as the 'Harcourt pentane air-gas flame,' when used under the conditions defined, does constitute a very exact standard, capable of being reproduced at any time without variation of illuminative value.

"(6) We have satisfied ourselves that the light given by Mr. Harcourt's above-mentioned pentane air-gas flame as defined, in respect to the conditions of its production, in the Appendix, is a true representative of the average light furnished by the sperm candle flame constituting the present standard. Since 1879, when the pentane air-gas flame was first introduced, many series of experiments have been made by different observers, in which the light of the proposed standard has been compared with the light of the standard sperm candle

flame, with the result that in those series of experiments in which the height of the pentane air-gas flame was adjusted strictly according to the directions given in the Appendix, the light afforded by this flame was found to agree exactly with the mean result afforded by the standard candle flame. In other series of experiments, indeed, in which a slight variation was made in the mode of adjusting the height of the pentane air-gas flame, some discrepancies in the direct results furnished by the comparison of its light with that of the standard candle flame were observed; but in these several series of experiments also, when the necessary correction, called for by the difference in the mode of adjustment resorted to, was made, the light of the pentane air-gas flame was found to accord closely with the mean result afforded by the standard candle flame.

"(7) Inasmuch, however, as there is a practical advantage in comparing directly the light of such a coal-gas flame as is usually tested (being, that is, of about a sixteen-candle-light value), with a light approximating somewhat in value thereto, we have further submitted to careful examination the flame of the ten-candle-light pentane argand proposed as a standard by Mr. W. J. Dibdin in 1886. This flame is produced by burning a mixture of air and pentane vapour from a suitable argand burner, provided with an opaque screen by which the light from the upper portion of the flame is cut off. The screen being set at a definite height, it was found by Mr. Dibdin that, owing to a compensating action affecting the lower or exposed portion of the flame, the luminosity of this portion of the flame remains constant even under considerable variations, whether in the total height of the flame or in the proportion of pentane vapour to air in the mixture burnt. With a view to simplify the construction of the argand burner furnishing a cut-off flame of this constant luminosity, we have tried various changes in the form of the cone and in the division of the air supply to the flame, but in every case have found the original burner, as supplied by Mr. Sugg for the purpose, to give more satisfactory results than the modified forms.

"(8) The amount of light emitted by the portion of the Dibdin argand pentane-air flame that is used in photometry, being dependent on the distance above the steatite ring of a screen by which the upper part of the flame is cut off, we have come to the conclusion that when the bottom of the screen is fixed at a height of 2·15 inches (54·6 mm.) above the top of the steatite ring, the amount of light emitted by the lower portion of the flame is substantially equal to ten times the average light of a standard sperm candle flame, or to ten times the light of Mr. Harcourt's one-candle-light pentane air-gas flame.

"(9) We have further satisfied ourselves that any number of Dibdin argand burners may be produced, having the form and dimensions set forth in the Appendix; and that these several burners, when used in the manner there defined, may be depended on to furnish a flame giving, when duly screened on the top, ten times the average amount of light given by a standard sperm candle.

"(10) We therefore recommend that the pentane-air flame furnished by a Dibdin argand burner, having the form and dimensions set forth in the Appendix, and used in the manner there defined, be accepted as giving the light of ten standard candles, and that this flame be authorised and prescribed for official use in testing the illuminating power of the gas supplied by the London Gas Companies.

"(11) We further recommend that sealed specimens of the burner, the carburetter, and the pentane for use therewith, duly certified by the Gas Referees, be deposited with the Board of Trade, and also in such places and in the care of such persons as the Board may direct, to be available for the purpose of comparison, in the event of any question arising as to whether the pentane-air flame of some particular burner does or does not afford the same amount of light as that now proposed for adoption as a standard.

"(12) With a view to making some provision for future possible improvements and requirements, we further recommend that the Gas Referees be authorised, should they at any time see fit, to approve and certify for use in gas-testing any other flame based upon the 10-candle standard defined above, which they may consider suitable for the purpose, whether produced in a like or unlike way, and whether having the same or a different multiple value; such other flame, however, not to be used for gas-testing unless approved by the Board of Trade, and unless the Gas Companies give their consent to its adoption as a standard.

"(13) We further recommend that the illuminating power of coal gas shall continue to be recorded as heretofore in terms of the light given by a specified number of cubic feet (to wit, 5 cubic feet) burnt per hour from the standard London argand burner, but that, in testing the illuminating power of the gas, the requirement that the gas shall actually be consumed at this rate be rescinded, so as to allow the Gas Referees to sanction a mode of testing in which the gas shall be burned from the standard London argand burner at whatever rate is found requisite in order that it may give a light equal to that of the prescribed number of candles, and in which the illuminative value of the gas shall be calculated as being inversely as the rate at which such gas had to be burned during the testing so as to give this amount of light."

The Report is signed by Prof. William Odling, F.R.S. (Chairman), Mr. W. J. Dibdin, Dr. E. Frankland, F.R.S., Dr. A. Vernon Harcourt, F.R.S., Mr. George Livesey, Dr. William Pole, Mr. George Rose-Innes, Prof. A. W. Rucker, F.R.S., Dr. W. J. Russell, F.R.S., Mr. G. C. Trewby, and (subject to the omission from (13), line 7, of the words "the Gas Referees to sanction") by Mr. H. E. Jones. Prof. Vivian B. Lewes was the Secretary of the Committee.

SCIENTIFIC EDUCATION IN AMERICA.

UPON the occasion of the laying of the corner-stone of a new building for a Museum for Dartmouth College, Hanover, U.S., Prof. A. S. Bickmore recently delivered an address, in the course of which he dealt with the methods of scientific instruction in America. The College was originally designed to elevate the Indian race in America, hence its location at Hanover, New Hampshire, in 1770. It was named after Lord Dartmouth, who took a deep interest in the aborigines of the New World, and who was the principal benefactor of the school established for their education.

We extract the following from the report of Prof. Bickmore's address in the *New York Times*:—

"The present is pre-eminently an educational age, and the princely gift from one of our alma mater's loyal sons for the purpose of endowing a 'professorship of palæontology, archæology, ethnology, and kindred subjects, and for the erection of a building for preserving and exhibiting specimens illustrating the aforesaid branches,' is in perfect harmony with the judgment of the leading educators of our times, namely, that the greatest benefit it is our privilege to confer upon coming generations is to provide ever-increasing means for their mental improvement.

"As we meet to-day to lay the corner-stone of the noble edifice so generously provided for by the late Dr. Ralph Butterfield, and to celebrate the commencement of a structure which will add so largely to the educational facilities of this college, I invite you to consider with me, as a subject suggested by this occasion, 'The Place in Modern Education of the Natural Sciences and their Museums.'

"In a period which will ever be famous in history for the great donations that are being constantly made by our private citizens for the public good, it is worthy of our careful consideration that the most munificent gifts are almost exclusively for the purpose of promoting education. In the United States where even the existence of 'a Government for the people and by the people' must ever rest upon the intelligence and the integrity of each individual citizen, it is not a matter of desirability, but simply one of necessity, that the promotion of public instruction shall ever be a question of paramount importance.

AMERICAN SYSTEM OF TEACHING.

"Our American system of instruction may be rapidly summarised. First and lowest is the kindergarten, which may be regarded as still in its experimental stage, but which is certainly destined to become one of our most effective methods of mental training. Next come the public schools, supported by taxation, with their primary and grammar grades, and the high schools and private academies. Above these are the colleges, with their ever-increasing series of elective studies; and then the universities, with their special schools of science, medicine, law, and theology; and finally, the great post-graduate institutions, composed of entirely distinct corporations for the creation of great museums of science and art, and the accumulation of exhaustive libraries.

"As nearly as it is possible to ascertain, we have been expending twice as much per individual for public education as England, but as she increases her grants for that purpose, our provision must be enlarged in the same ratio, and especially ought we to introduce the latest and most improved methods for imparting instruction.

"The National Educational Association, at its meeting at Saratoga in 1892, appointed a committee, with President Eliot at its head, to suggest improvements in the studies of our secondary schools, and in their report those educators state their opinion that 'the study of both plants and animals should begin in the lowest grade, or even in the kindergarten, and that such studies, with geography subsequently added, ought to count in an examination for college.' Indeed we find the latter study already in the curriculum of Harvard University. In 1882, just ten years before President Eliot's committee was appointed, we began to seek to render our Museum of Natural History in New York City an aid to the instruction given in our public schools, by placing in each of them a small cabinet of the rocks, corals, shells, insects, and birds of our own country. We also organised for the teachers a series of illustrated lectures, describing the collections on exhibition in our halls, and picturing the regions from which they came. Our first audience consisted of twenty-five teachers and three officers of our Board of Education. Last year, under the auspices of the State Superintendent of Public Instruction, we spoke directly at the museum, and indirectly by the repetition of our lectures elsewhere, to 103,000 of our educators and other citizens, and now, through a provision made by the last Legislature, our visual instruction will be repeated in the public schools of every city in our State, and in all the villages having a population of 5000 and upward, so that during the coming year we shall reach 800,000 pupils, besides large audiences of adults on the public holidays. The measure of success that has attended our labours has been largely due, first, to our belief that it is the duty and the privilege of every educational institution of every grade to try to render a distinct benefit to each class of the citizens, wherever it may be located, and, secondly, to the illustrative method employed based on the maxim that 'the eye is the royal avenue to the mind.'

"To the question, what kind of a collection in natural history should be desired for each of these grades of instruction, we would reply that it should exactly correspond to the curriculum of study adopted by that grade. A college museum should possess a full series of the animals, plants, and minerals of the State in which it is situated, with typical specimens of the orders of these natural kingdoms from other States and other Continents; and also a library that will enable its teachers to keep up with the general progress of their departments. Even this simple plan may be made to absorb more money than most of our colleges are likely to acquire for such purposes during many generations, on account of the unfortunate tendency in these times for many a friend of education to found a new institution which may bear his name.

"In this presence I hardly need to add that every student should be encouraged to improve his leisure hours in taking long walks through all the region surrounding his place of study, in order to make his own observations and his own deductions upon the physical geography and geology of the places visited. His vacations may in this way become quite as important as the same length of term time. If during these travels he will gather minerals, fossils, or make a small cabinet of botanical specimens or insects, he will not only gain important information, but will have discovered the true mode of gaining by healthy exercise [illegible] that relaxation which is a necessary condition to the [illegible] the recitation room; and whatever may be his [illegible] occupation, thankful indeed will he be that he [illegible] early to learn how to forget the overwhelming [illegible] life, and that therefore he is able once more to [illegible] nature as restfully as he did in his college days.

"A [illegible] which has courses of post graduate studies [illegible] college curriculum may follow the same plan, and [illegible] for original research along those lines in which [illegible] may be eminent authorities. However, [illegible] is that when one enthusiastic instructor [illegible] place, the new occupant of the professor[illegible] already given his leisure time to some one of the [illegible] groups of the animal kingdom entirely different from [illegible] by his predecessor, and the books and specimens he finds already gathered will prove of little value to him for the pursuit of his own favourite branch of our science.

MUSEUMS AS EDUCATORS.

"A museum of natural history developed by a distinct corporation may advance education in two different ways—firstly, by the exhibitions of its collections and by illustrated lectures; and, secondly, by securing such exhaustive series of specimens and the books treating of them as to render it possible for original research to be carried on in many or most of the orders of the animal kingdom. Such organisations could favourably utilise an unlimited amount of funds, and even partly to fulfil their mission must absorb enormous sums. They can, therefore, only be created in our great and wealthy cities, and in them only by a happy and enthusiastic co-operation of their State and Municipal Governments, supplemented by large gifts from their wealthiest and most generous citizens. Our museum in Central Park is becoming such an institution for instruction and investigation. The city has provided a site of eighteen acres and $2,500,000 for that part of the structure already erected and under contract. Our specimens and books, the gifts of private citizens, amount to about $2,000,000 more, and yet we have completed less than one-fifth of our proposed edifice. The Art Museum has even a larger property and as comprehensive a plan, and now the Lenox and Astor Libraries, and the Tilden gift are happily united, and together form a third stone in the arch of this central university for the highest culture. So that, while we visit London to admire its group of noble institutions at South Kensington, we are at the same time founding in our new land a similar series on a greater scale, and erecting buildings and accumulating collections at a rate not witnessed on the other side of the sea; but the extensive ground plan upon which we are building the Museum of Natural History embodies the views of the late Sir Richard Owen, the ablest investigator in our science of the present century.

"In such a museum the specimens of minerals, rocks, and even fossils may be nearly perfect in themselves or fairly representative of the formations from which they were taken, but it should be remembered that in the usual mode of exhibition of animals and plants we necessarily lose the charm of their environment. Thus the song-thrush, which in life fills these northern valleys with the magical music of its liquid notes, when mounted and placed in a case is not only mute but uninteresting. The humming birds, in all their array of brilliant gems, to be known must be seen alive, darting to and fro amid the fragrant and richly-coloured flowers which supply their food in the tropical lands where the stately palm-trees wave their graceful fronds. The albatross, as usually mounted, with its wings tamely folded, hardly suggests the noble bird that skims gleefully over the crests of mountainous waves, while the storms are raging in the 'Roaring Forties' of the southern ocean. The chamois can only be appreciated when it is seen aloft on some projecting crag of the Alps, and the Rocky Mountain goat when, after long climbing, we find it surrounded by the splintered peaks of the Selkirks high up on the borders of eternal ice.

"To remedy these defects such a progressive thinker as Sir William Flower wisely proposes an entire change in the present style of taxidermy, and our experience in New York has been that our cases of American birds in their native haunts are among the most attractive as well as instructive displays in our halls. In our illustrated lectures we exhibit on one screen the Rocky Mountain sheep, while we picture on another screen beside it the grand mountain of the Holy Cross, where this rare animal formerly roamed.

"Zoology has attained a prominent place in this country largely through that great investigator and instructor, Prof. Lewis Agassiz, whose marvellous store of knowledge was equalled only by his devotion to his favourite study.

"But while science should be pursued for science's sake, yet we must not under-estimate the value of the technical sciences which take the results of original research and transform them so that they may confer an immediate and practical benefit upon the whole world. It is in this great department of modern education—the applied sciences—that the American people are pre-eminently successful, and in the coming contest for the supremacy among all nations, ours is destined to maintain a commanding place through our untiring industry, inventive genius, and peculiar adaptability to meet new conditions."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Prof. J. W. Judd, C.B., F.R.S., has been appointed Dean of the Royal College of Science, in succession to the late Prof. Huxley.

The following list of Royal scholarships, medals, and prizes awarded last month in connection with the Royal College of Science, London, has just been issued by the Department of Science and Art. Royal scholarships: First year's Royal scholarships, Ernest Smith, George Marks Russell, Frank Fisher, Norton Baron; second year's Royal scholarships, Robert Sowter, Joe Crowther. Medals and prizes:—"Edward Forbes" medal and prize of books for Biology, William George Freeman; "Murchison" medal and prize of books for Geology, John Caspell; "Tyndall" prize of books for Physics, Part I., William Herbert White; "De la Beche" medal for Mining, Robert William Pringle; "Bessemer" medal and prize of books for Metallurgy, John Collet Moulden; "Frank Hatton" prize of books for Chemistry, William Longshaw. Prizes of books given by the Department of Science and Art:—Mechanics, Cecil Alwyne Selpram Baxter; Astronomical Physics, Ernest Edward Leslie Dixon, William Herbert White; Practical Chemistry, Henry William Hutchin; Mining, Robert William Pringle; Principles of Agriculture, William Williams.

The University of Pennsylvania has issued an appeal (says *Science*) asking for an endowment fund of £1,000,000 to meet the immediate requirements of the University. Mr. Thomas McKean has given without restrictions a sum of £10,000 in addition to the £10,000 given a few months ago. A contribution of £2000 has also been received from Mr. Richard F. Loper. It is stated that this is the thirteenth contribution of a similar kind that has been received. We learn from the same source that the University of Cincinnati has received a gift of £9000 from Mr. Henry Hanna, to be used in the erection of a wing in the new University building.

SOCIETIES AND ACADEMIES.

Dublin.

Royal Dublin Society, April 24.—Prof. J. Mallet Purser in the chair. The following communications were read: Dr. E. J. M'Weeney on a temporary variation in the quality of the Vartry water. [This is the water-supply of the city of Dublin.]—Dr. David Hepburn (of Edinburgh), on the papillary ridges on the hands and feet of monkeys and men. The material for this paper was supplied by the anthropological laboratory of Trinity College, Dublin, and the paper was communicated by Prof. D. J. Cunningham, F.R.S.—Mr. Walter E. Adeney, on the course and nature of fermentative changes in natural and polluted waters, and in artificial solutions, as indicated by the composition of the gases in solution.

May 22.—Mr. Thomas Preston in the chair. The following communications were presented: Prof. Emerson Reynolds, F.R.S., note on the spectrum of argon.—Mr. W. E. Adeney, on the chemical examination of organic matters in river water.—Mr. Richard J. Moss, on the preparation of helium.—Mr. Moss also exhibited a simple form of apparatus for the distillation of mercury in vacuo; and Dr. W. Frazer showed some photographs of the natives of Formosa.

June 20.—Dr. J. Joly, F.R.S., in the chair. The following papers were read: Mr. Thomas Preston, on the rectilinear propagation of light. Dr. J. Joly, on photography in natural colours.—Sir J. William Dawson, F.R.S., note on a paper on "Eozoonal structure of the ejected blocks of Monte Somma," by Dr. H. J. Johnston-Lavis and Dr. J. W. Gregory, and reply to the note by the last-named authors.—Dr. G. Johnstone Stoney, F.R.S., criticism of the kinetic theory of gases regarded as illustrating nature. Dr. E. J. M'Weeney, further observations on the Vartry water. Dr. M'Weeney exhibited cultivations of *Phoma Betae*, a fungus that produces a disease of the mangold wurzel.

Paris.

Academy of Sciences, July 29.—M. Marey in the chair.—On the presence of water vapour in the atmosphere of the planet Mars, by M. J. Janssen. Mr. W. W. Campbell has recently asserted that the atmosphere of Mars does not contain water vapour, and has requested further details concerning the author's observations, from which the presence of water vapour had been supposed to be proved. These details are now supplied; the author particularly points out that his Etna observations were carried out under exceptionally favourable conditions, and that the definite and convincing evidence they afforded was confirmed by observations carried out at Palermo and at Marseilles.—On groups of substitutions of the same order and degree, by M. Levavasseur.—On algebraical surfaces admitting of a continuous group of internal birational transformations, by MM. G. Castelnuovo and F. Enriques.—On algebraical machines, by M. Léonardo Torres.—Vibrations of the tuning-fork in a magnetic field, by M. Maurain.—New photographs of lightning flashes, by M. N. Piltschikoff. Several types of lightning flash are defined, and the dimensions are given for certain flashes; for instance, a photograph taken during a storm at Odessa on June 13, shows a luminous band 0·75 mm. wide, caused by a flash at a greater distance than 10 kilometres; the actual width of the flash was therefore more than 62 metres.—A new voltaic cell, by M. Morisot. The cell consists of a carbon pole immersed in 1 : 4 sulphuric acid saturated with potassium bichromate and a zinc pole within a porous cell containing concentrated caustic soda solution (sp. gr. 1·25), this cell being separated from the depolarising acid solution by a second larger porous cell containing dilute caustic soda (sp. gr. 1·05). The E.M.F. of this cell is to begin with 2·5 volts, and remains above 2·4 volts during at least ten hours of uninterrupted action, and with variable external resistance remains constant. The intermediate bath of dilute alkali diminishes the action across the porous diaphragm between the soda and the sulphuric and chromic acids without materially increasing the resistance. The zinc is less attacked than with an acid bath, and may readily be brought into good condition after long use by a short immersion in acid.—Action of aniline on mercurous iodide, by M. Maurice François. The aniline decomposes the mercurous iodide with the formation of the substance diphenylmercurodiammonium iodide $(C_6H_5NH_2)_2HgI_2$, and metallic mercury. The reaction is incomplete and exactly similar to the action of water on bismuth sulphate or mercuric sulphate. The boiling saturated aniline solution dissolves mercurous iodide and redeposits it on cooling in the crystalline form.—Action of nitric peroxide on campholenic acid, by MM. A. Behal and Blaise.—On the products of the condensation of isovaleric aldehyde, by M. L. Kohn.—On the estimation of boric acid, by MM. H. Jay and Dupasquier. The boric acid is distilled over into soda by the aid of methyl alcohol used continuously and the residual soda determined by titration.—On the elimination of lime among those affected with rickets, by M. Oechsner de Coninck.—On the utility of injections of oxysparteine before anæsthesia by means of chloroform, by MM. P. Langlois and G. Maurange. The injection, an hour before the operation, of 4 to 5 cgr. of sparteine or 3 to 4 cgr. of oxysparteine, together with 1 cgr. of morphine, gave rapid narcosis easily maintained with little chloroform and a regular pulse, energetic even when the respiration became superficial.—Influence of toxines on progeny, by M. A. Charrin. Bacterial poisons derived from the mother, like those introduced otherwise into the system, retard the growth of infants by rendering assimilation less perfect.—On the structure of the ectoderm and of the nervous system of parasitic Plathelminthes (Trematodes et Cestodes), by M. Léon Jammes.—Contributions to the embryogeny of simple Ascidians, by M. Antoine Pizon.—On the composition of the monazite sands of Carolina, by M. Boudouard.—Discovery of gigantic remains of fossil elephants, made by M. Le Blanc, in "la ballastière de Tilloux (Charente)," by M. Marcellin Boule.

Berlin.

Physical Society, June 14.—Prof. du Bois Reymond, President, in the chair.—Dr. F. Kurlbaum gave an account of his determination of the unit of light made in conjunction with Prof. Lummer. The unit was based on the light emitted by white-hot platinum foil. Since the radiant energy varies with the temperature, it was necessary to keep the latter constant for a prolonged period, and to be able to re-establish it at any time. This result was arrived at bolometrically by measuring the ratio of the total radiant energy from the glowing foil to the radiation taking place across an absorbing medium. This ratio is dependent upon the temperature of the radiating body, and provides a trustworthy measure of its temperature. It was necessary to find some covering for the bolometer which should absorb all rays as uniformly as possible; after many experiments a layer of

platinum black was found most suitable for this purpose. The absorbing medium employed consisted of a thin layer of water in a quartz cell. The energy radiated from the heated foil passed through a diaphragm of known aperture, whose temperature was the same as that of the bolometer. The errors in determining the unit of light amounted to one per cent., due chiefly to the air currents on the surface of the foil. The unit can now be established at any time in the Imperial Physico-technical Institute (Berlin); but in order to facilitate its accurate establishment at any other place, experiments are being made to determine the temperature of the glowing foil from ratio of the radiation over the range of the visible spectrum.

June 28.—Prof. von Bezold, President, in the chair.—Dr. Kaps exhibited and described some new electric meters constructed by Siemens and Halske, which by the use of constant magnets provide an accurate measure for technical purposes, and are uninfluenced by ordinary variations of temperature. Dr. du Bois described experiments made by Dr. E. T. Jones on magnetic lifting-power. He had already showed that Maxwell's formula holds good for a field whose strength is up to 500 C.G.S., and now passed on to fields of greater strength. In the last set of experiments electro-magnets were employed with a sectional surface of an iron bar passed through the armatures. A magnetic lifting power of 52 kilogrammes per square centimetre of surface was thus for the first time obtained, and Maxwell's formula was found to hold good up to this maximal value; the error was at most five per cent., due as yet to insufficient introduction of corrections. Stephan's formula did not in any way correspond with the results of the above experiments. It further appeared that a lifting power of 150 kilogrammes per square centimetre should be obtainable.

Amsterdam.

Royal Academy of Sciences, June 29.—Prof. Van der Waals in the chair. Prof. Martin presented a work, written by him, and entitled "Die Fossilien von Java." Basing his arguments on the presence of these fossils, the author showed that in Java there are found Upper Miocene, Pliocene and Quaternary sediments. When the distribution of these formations is considered, it appears that in general the newer strata have been formed on the outer side of the older ones, and there can be no doubt that since the time of the Upper Miocene formation a continuous and very slow elevation of the coast ("negativ strandverschiebung") took place, in consequence of which the Upper Miocene, Pliocene and Quaternary sediments of the coast were laid dry. That this shifting of the coast was very considerable, is proved by the Njaliendoeng fossils, found 910 m. above the level of the sea, and this fact further tallies with what is known about Sumatra, where in the "Padangsche Bovenlanden" Neogene sediments have been found up to a height of 1088 m. Not long ago the author showed that during the Quaternary period a considerable movement took place in the eastern part of the archipelago, and numerous facts show that the whole of the Indian archipelago was subjected to this. The author further remarked that he had received interesting fossils from Western Borneo. Among them are: *Perisphinctes* (Waag.), *Pecten* [illegible], and *Corbula*. All these fossils have been found in strata that were formerly known as "ancient schists," which, however, on account of the above-mentioned fossils, can only be reckoned to belong to the Mesozoic period; more particularly they ought to be classed either with the Jurassic or with the Cretaceous formation. In accordance with the present state of our knowledge it is highly probable that the fossils in question have been taken from Jurassic formations. It appears, then, that Mesozoic strata have a very wide distribution in the Indian archipelago. Prof. [illegible] read a paper on *Cynips calyx*. The *Cynips calyx* gall, very common in Austria Hungary on *Quercus pedunculata*, [illegible] in commerce as a first-rate tanning material. In [illegible] Netherlands two or three small localities are known where [illegible] to be found. The dehydration, rehydration and re-[illegible] of colloidal silicic acid, by Prof. van Bemmelen. [illegible] presented some pamphlets by himself and some of [illegible], with reference to Dr. Langemeyer's dissertation, [illegible] the influence of the use of sugar upon muscular labour. [illegible] experiments, made with the ergograph, it is deduced that [illegible] in this way been proved that sugar has a favourable [illegible] muscular labour.—At the request of Dr. C. A. Lobry de Bruyn, Prof. Franchimont communicated that free hydrazine had been prepared by the former in two ways: 1° from N_2H_4HCl with sodium methylate in a methyl alcoholic solution, and 2° by heating the hydrate to 100° with barium oxide. Free hydrazine is a somewhat thick fluid with the smell of the hydrate. It boils without decomposition at 113°·5 and a pressure of 761 m.m., and at 56° if the pressure is 71 m.m. When cooled, it becomes solid, and then melts again at 2°; its density at 23° is 1·0075 and does not, therefore, differ much from that of the hydrate (boiling at 119°). In ordinary air it forms strong vapours and is easily oxidised by oxygen with the formation of nitrogen. In the air it will burn, but not explode, like hydroxylamine, and consequently it is much more stable.—Prof. Kamerlingh Onnes communicated measurements on the capillarity of liquid gases, made by Dr. Verschaffelt in the Leyden laboratory. Carbonic acid and nitrous oxide obey the law of corresponding states; their capillary equation has an exponent approaching the theoretical value given by Van der Waals, and they are not associated fluids.—Prof. Van der Waals presented a paper intended for the report of the meeting, and entitled: "On the critical circumstances of a mixture," being a sequel to what was communicated in the meeting of the section held in May.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Bouches à Feu: E. Hennebert (Paris, Gauthier-Villars).—Balistique Extérieure: E. Vallier (Paris, Gauthier-Villars).—Geological Survey of Canada, Annual Report, new series, Vol. 6 (Ottawa).—Science Readers, Book iv.: V. T. Murché (Macmillan).—A Text-book of the Principles of Physics: Dr. A. Daniell, 3rd edition (Macmillan).—Pan-Gnosticism: N. Winter (Transatlantic Publishing Company).—A Handbook to the Flora of Ceylon: Dr. H. Trimen, Part ; and plates (Dulau).

Pamphlets.—Gegenetische Beitrage: Dr. O. Kuntze (Leipzig, Gressner).—Sobre Peces de Agua Dulce: C. Berg (Buenos Aires, Alsina).—The Grimsby Trawl Fishery, &c.: E. W. L. Holt (Plymouth).

Serials.—Journal of the Institution of Electrical Engineers, July (Spon).—Quarterly Journal of the Geological Society, August (Longmans).—Fortnightly Review, August (Chapman).—Macmillan's Magazine, August (Macmillan).—Scribner's Magazine, August (Low).—Verhandlungen des Naturhistorischen Vereins, &c., Einundfünfzigster Jahrg., Sechste Folge, 1 Jahrg., Zweite Hälfte (Bonn).—Bulletins de la Société D'Anthropologie de Paris, tome vi. 4e serie (Paris, Masson).—Geological Magazine, August (Dulau).—Geographical Magazine, August (Stanford).—Transactions and Proceedings of the New Zealand Institute, 1894, Vol. xxvii. (Wellington, Costall).—Science Progress, August (Scientific Press, Ltd.).

CONTENTS.

THURSDAY, AUGUST 15, 1895.

THE HISTORY OF EVOLUTION.

From the Greeks to Darwin: an Outline of the Development of the Evolution Idea. By Henry Fairfield Osborn, Sc.D., Da Costa Professor of Biology in Columbia College, &c. (New York: Macmillan and Co.

THE object of this most interesting and useful work is to survey the last twenty-four centuries and bring together the thoughts—true and false—upon evolution. Examining and comparing the material which he has collected, the author concludes "that the influences of early upon later thought are greater than has been believed, that Darwin owes more even to the Greeks than we have ever recognised." In supporting this conclusion the author desires to give due credit to the earlier writers, but not to lower in any way the transcendant position occupied by Darwin. Indeed, so scrupulously fair is the treatment that the materials are thoroughly available to those who do not altogether follow the author in his conclusion. And many objections to the conclusion are most prominently brought forward. Thus the great interval between the beginning and the middle of the present century, when all continuity in evolutionary thought seemed to be broken, is described again and again. We read on page 12: "Perhaps the sharpest transition was at the close of the third period, in which a distinct anti-evolution school had sprung up and succeeded in firmly entrenching itself, so that Darwin and Wallace began the present era with some abruptness." Again, on pages 227 and 228, the strong prejudice against evolution which marks this period is illustrated in many ways, and the section concludes: ". . . all the progress which had been made in the long centuries we have been considering was, for the time, a latent force. The Evolution idea, with the numerous truths which had accumulated about it, was again almost wholly subordinate to the Special Creation idea."

The recognition of this strongly-marked gap in the history of evolutionary thought, and, above all, the details which we learn from Darwin's "Life and Letters," tend to throw doubt upon the view that he drew much of his inspiration from the past. The great majority of naturalists could not entertain the idea of evolution unless some explanation of its cause was forthcoming. Darwin treated the process and the cause as entirely distinct, and was convinced of the one long before he had come to any definite opinion about the other. In accepting evolution as against special creation, we fail to find any evidence that Darwin was influenced by the arguments or conclusions of an earlier day. He was influenced and finally convinced by his conclusions from his own observations on the *Beagle* (quoted by Prof. Osborn on p. 233). In looking for the causes of evolution he was equally independent of the past; for he saw that adaptation was the central fact which required explanation, and which had received none at the hands of the naturalists with whose writings he was acquainted.

But whether the thread be broken or continuous, the history of thought upon this all-important subject is of the deepest interest, and Prof. Osborn's work will be welcomed by all who take an intelligent interest in evolution. Up to the present, the pre-Darwinian evolutionists have been for the most part considered singly, the claims of particular naturalists being urged often with too warm an enthusiasm. Prof. Osborn has undertaken a more comprehensive work, and with well-balanced judgment assigns a place to every writer.

The history of thought upon evolution from 640 B.C. to the present day is divided into two main phases, the second of which is further subdivided into three periods.

The first phase, "The Anticipation of Nature: Greek Evolution," and its effects on Christian Theology and Arabic Philosophy, lasted from 640 B.C. to 1600 A.D.

The second phase, "The Interpretation of Nature: Modern Evolution," opens with the period of "Philosophical Evolution," from 1600 to 1800, associated with the names of Bacon, Kant, Herder, Bonnet, Oken, &c. In this period the Greek traditions were largely shaken off, and inductive evolution began.

The next period, that of the rise and decline of "Modern Inductive Evolution," somewhat overlapping the last, is limited by the years 1730 and 1850, from Buffon to St. Hilaire. It depends upon the writings of Linnæus, Erasmus Darwin, Lamarck, Goethe, Treviranus, &c. At the close of this period, Owen and Herbert Spencer are placed.

The last period, that of the re-establishment of "Modern Inductive Evolution" upon a firmer foundation, dates from 1858 to the present day. It is associated with the names of Darwin and Wallace, and marked by the scientific evidences of evolution, by the theory of natural selection, by observation and speculation upon other factors of evolution.

The section which deals with the Greeks has been somewhat unfairly criticised. Some people appear to believe that an account of Greek ideas upon evolution can only be attempted with success by an eminent classical scholar. But classical scholars have already done their utmost in the way of translation and of study. It is now of far greater importance to have a critical account, like that in the work we are considering, by a writer who is an authority upon evolution.

In discussing "The Legacy of the Greeks" (pp. 64–68) the author points out that the first element is "scientific curiosity, their desire to find a natural explanation for the origin and existence of things." The complete dependence of all investigation upon this spirit is maintained, and it is truly said that "the ground motive in science is a high order of curiosity, led on by ambition to overcome obstacles." The final conclusion is that "the Greeks left the later world face to face with the problem of causation in three forms: first, whether intelligent design is constantly operating in nature; second, whether nature is under the operation of natural causes originally implanted by intelligent design; and third, whether nature is under the operation of natural causes due from the beginning to the laws of chance, and containing no evidences of design, even in their origin."

In this section of the work we find, as we might expect, that the genius of Aristotle completely overshadows that of the other Greek writers who attempted to face the problems of the origin and development of living forms.

R

In the long second period, that of the theologians and natural philosophers, "no advance whatever in the development of the evolution idea was made . . .; scientific speculation and observation were at a standstill, except among the Arabs" (p. 70).

As we advance towards the work of the naturalists and philosophers of the two last centuries, the difficulties and dangers of interpretation increase. It is even easier to read preconceived notions into the single passages of dead writers than into the phenomena of nature; and we all know that the latter process is only too easy. If the results are not to be in the highest degree misleading, the author must, like Prof. Osborn, be entirely free from bias, and must possess a cool and critical judgment.

We meet with constant and timely protests against the rash conclusions which may be reached by selecting isolated passages from an author, and dealing with them apart from their context, and the full recognition of the great danger which underlies this too common practice, viz. that we unconsciously read into such passages our present knowledge (p. 80).

Prof. Osborn considers that too high a place has been assigned to Oken and Treviranus by Haeckel and Huxley respectively, and that Naudin's supposed anticipation of natural selection is far from being as satisfactory as Quatrefages and Varigny maintain. The suggestion that Oken anticipated the cell theory is acutely criticised: it is suggested that his conception of the cell as a sphere was probably only a result of the transcendant position occupied by this geometrical form in his system of philosophy (p. 124).

The suggestion (on p. 235) that Darwin's 1844 Essay should be published will, the present writer feels assured, meet with warm approval from the wide circle of readers who are eager to learn all that can be learnt of the history of Darwin's views upon the great work of his life.

The hope is expressed (on p. 245) that we shall learn the steps which led to Wallace's independent discovery of natural selection. That information is fortunately now before us, and we know that Wallace was led to the discovery by reflecting on Malthus' "Essay on Population," as he lay ill of intermittent fever at Ternate (quoted, without reference, in Milnes Marshall's "Lectures on the Darwinian Theory," London, 1894, pp. 212, 213, and to be found in the abridged form of the "Life and Letters of Charles Darwin"). Thus another most important detail is added to the extraordinary coincidence of the independent discovery of natural selection.

There is comparatively little to criticise in the volume. The idea of the marine origin of life, traced to Thales, is stated to be "now a fundamental principle of evolution" (p. 33): but at the end of the volume it is more correctly asserted that we are now too wise to answer the query, "Where did life first appear?" (p. 247).

Concerning the debated question as to whether Lamarck was aware of Erasmus Darwin's writings, and made use of them without acknowledgment, the author (pp. 154, 155) quotes a passage from the "Animaux sans Vertèbres," in which Lamarck states that his theory is the first which has been presented. This he considers to be "satisfactory evidence that Erasmus Darwin and Lamarck independently evolved their views." But if Lamarck borrowed without acknowledgment, it would be but a small step further to write the passage in question.

The statements and conclusions to which exception is chiefly to be taken concern the life of Darwin himself, which the author professedly treats in a very brief and imperfect manner, any detailed account being beyond the scope of this volume.

The author speaks (p. 227) of "Huxley's somewhat guarded acceptance of the theory" on the first appearance of the "Origin," and implies that he became a much stronger supporter of evolution in later years. But in reality his convictions on this subject never changed. In his letter to Darwin, written November 23, 1859, the day before the publication of the "Origin," Huxley expressed himself as "prepared to go to the stake, if requisite, in support of" those parts of the book which deal with evolution as apart from natural selection. As to the latter he says: "I think you have demonstrated a true cause for the production of species, and have thrown the *onus probandi* that species did not arise in the way you suppose, on your adversaries." And these were Huxley's views up to the last occasions on which he spoke on the subject, at the Oxford meeting of the British Association last year, and at the anniversary of the Royal Society when he received the Darwin Medal. On both occasions he carefully distinguished between evolution and natural selection, being prepared to defend the former to the uttermost, while he declined to commit himself upon the latter.

It is contended (p. 239) that Darwin's faith in natural selection reached its climax in 1858, and then gradually declined. The evidence quoted in support of this conclusion is a letter to Carus in 1869, in which Darwin says: "I have been led to infer that single variations are of even less importance in comparison with individual differences than I formerly thought." But this passage proves a strengthening, and not a weakening of his belief in the efficiency of natural selection, inasmuch as it is considered competent to work upon the minute differences which separate individuals instead of upon the ready-made material provided by single variations, however conspicuous. By "single variations" he meant single individuals differing widely and conspicuously from the average of their species. His letter to Carus was written shortly after he had been convinced on this point by Fleeming Jenkin's review of the "Origin" (*North British Review*, June 1867). A careful study of vol. iii. of the "Life and Letters" leaves no doubt upon this point; while the facts thus brought out tend to refute the argument on p. 245 as to the supposed antagonism between Darwin's and Wallace's conception of the operation of natural selection as expressed in their contributions to the Linnean Society in 1858.

A passage in the sixth edition of the "Origin" is referred to (p. 242) as having been published in 1880, and is therefore considered to be "among Darwin's last words upon the factors of evolution." The passage in question is referred to p. 424 of the "Origin," but occurs on p. 421 of the copies I have consulted. In it Darwin expresses his belief that evolution has been effected "chiefly" by natural selection, "aided in an important manner by the inherited effects of use and disuse of parts; and in an

unimportant manner . . . by the direct action of external conditions . . ." This passage is considered by Osborn to prove that the progressive tendency towards the explanations of Lamarck and Buffon which he believes Darwin exhibited from 1859 onwards culminated at the close of his life. But the sixth edition appeared in 1872, and the date 1880 is merely that of a reprint. The words in question were certainly written before the former date, and even in the fifth edition (1869) Darwin inserted the word "chiefly" to qualify an expression of confidence which might have been interpreted as a belief in the all-sufficiency of natural selection.

The fact appears to be that there was no progressive change in Darwin's attitude on this subject, but that his opinion fluctuated as various classes of evidence were brought before him, and at the very end of his life his belief in the direct action of external conditions was seriously shaken by the results of Hoffmann's experiments. The effect produced on him is well shown in his letter to Semper, written July 19, 1881, less than a year before his death ("Life and Letters," vol. iii.). But although Darwin's opinion fluctuated as to the relative value of the supposed causes of evolution other than natural selection, his views as to the paramount importance of the latter never varied in any of his published utterances. The words which conclude the Introduction of the 1859 "Origin" are repeated without change in each succeeding edition and reprint. "Furthermore, I am convinced that natural selection has been the main, but not the exclusive means of modification."

The printing and general get-up of this interesting work leaves nothing to be desired, being far above the average that obtains in scientific publications. It may confidently be predicted that the book will be widely read and greatly appreciated. E. B. P.

THE ELEMENTS OF ARCHITECTURE.

Architecture for General Readers, &c. By H. Heathcote Statham. 8vo. London: Chapman and Hall, 1895.

THE aim of this treatise, as stated in the preface, is certainly a good one, namely, to supply the "general reader" with the means of criticising architecture in an intelligent manner, and principally by giving an analysis of the two most logical and complete styles that have ever existed, namely, the Greek and the Gothic; the former representing the trabeated, and the latter the arcuate system of building. Our author, however, very properly does not confine his attention to these two styles and their later developments, but also makes wide digressions in the direction of Egyptian, Byzantine, and Mahommedan structures, all of them being copiously illustrated and discussed at considerable length. The work exhibits throughout the author's great and varied acquaintance with his subject, and cannot but be of much interest and value to any reader who desires to dive more deeply than amateurs are accustomed to do into the principles which ought to guide the professional architect, and which, indeed, do guide all those who achieve anything worthy of the art in which they practise.

In page 20 the importance of planning is properly insisted on. The plan is shown to be the very "backbone" of the structure, and the attention of the "general reader" is rightly called to this. It may be doubted, however, whether the general reader is prepared for the minute criticism, which we find a little further on, respecting certain competition designs, which criticism is rendered the more difficult to follow, in consequence of the small scale of the plans by which these designs are illustrated, and he may, perhaps, wish that he had been led into such deep water more gently. In page 31, with reference to the proportions of buildings as affecting the eye, the author appears to doubt whether (with the exception of the late Mr. W. W. Lloyd's discovery of the system which prevails in the Parthenon) any definite and clear case has been made out for the establishment of proportion theories. The author is probably quite justified in his refusal to accept any general adoption of a system for proportioning buildings "on the basis of geometrical figures, especially triangles of various angles." There could not possibly be any æsthetic value in confining the main lines of the architecture within such limits; but rectangular proportions in low numbers (of which nature are the proportions of the Parthenon) are on a different footing, and it is extremely probable that they do produce harmonious effects. They are to be found in many other Greek examples besides the Parthenon, and in one Gothic building at least, namely, the work of Bishop Grosetete in the nave of Lincoln Cathedral (see the *Transactions* of the Archæological Institute of Great Britain, &c., for 1848), where rectangular proportions of this character come out without any "coaxing" with remarkable exactness; and as Bishop Grosetete, besides being a great ecclesiastic, was one of the most prominent philosophers of his day, there is the more reason to accept it as having been intentional.

In p. 34, the chief characteristics of the Egyptian, the Greek and the Gothic are summed up in a few words, as Mystery, Rationalism, and Aspiration. In p. 43, the meed of merited praise is given to Mr. E. L. Garbett's excellent little treatise on "The Principles of Design in Architecture." In p. 58, doubt is thrown on the wooden origin of the Greek entablature. The reader, however, may be referred to MM. Perrot and Chipiez' recent work on "The Arts of Primitive Greece," in which this derivation is shown from the remains at Tiryns, Mycenæ, and Orchomenus. In p. 73, the Corinthian example of the temple of Jupiter Olympius at Athens should not be attributed to a Roman source; it dates from Antiochus Epiphanes, the Greek founder, and the prototype of the capital is found in the *tholos* at Epidaurus, a pure Greek building. No doubt at the time the Athenian temple was built, about 170 B.C., Rome was pushing her way towards the East, and Antiochus himself had been sent as a hostage to Rome after the defeat of his father by Scipio. There may have been something political in his employment, as we are told of a Roman citizen as his architect, but the architecture itself, at that date, could not but have been thoroughly Greek.

In p. 78, the author well illustrates his argument, showing the superiority of constructive simplicity in a design over another decorated with meaningless architectural detail, by the contrast of London and Blackfriars Bridges; but it is not so clear, as maintained in the previous page that the combination of columnar and arcuate design in the same wall is a "Roman sham." It is no doubt a

departure from primitive simplicity, but there seems no reason for calling it a sham, in cases where both types are used constructively. The "general reader" may certainly be justified in passing over the "approximate theory" of the strains of arches, but the subject of pendentives (in p. 95) is more to the point, having very important relation to the construction of cupolas. Much more seems to be made in the criticism on the shams of St. Paul's (p. 98) than the subject warrants. The design is blamed because the interior cupola is distinct from the external. There would be as much reason to blame the magnificent central towers of some of our cathedrals because the open lantern chamber over the crossing does not rise to the summit of the tower or spire. The author, however, duly praises Sir Christopher Wren's first design, the Greek cross plan, of which a good judge, the late Rev. J. L. Petit, has maintained that if this design had been executed it would have been the finest interior in the world. On the subject of vaulting (pp. 107–116), the development of which is well and clearly followed out, it is stated that the pointed arch was *invented* for the purpose of facilitating the construction. This could hardly have been the case, because the pointed arch had been used in the East long before the period referred to; but its great applicability to that favourite architectural feature was then recognised, and when once introduced for constructive reasons, it soon began to influence the whole structure.

In p. 125 commences a chapter on the theory and use of mouldings, which play so important a part in architectural design that it is quite essential that an amateur who desires to form a right judgment on architectural subjects, either historically or critically, should study their development and application; he will find the subject clearly and logically explained in this chapter. In chapter v. are some judicious remarks on ornament, showing on the one hand that however valuable a help it may be, the art is really independent both of sculpture and carved ornament, and that the latter is inferior in expression to mouldings properly used. In pp. 184–188 are some just views on the combination of architecture with scenery. Without going so far as to say that a spire on a hill—such, for instance, as Harrow—must necessarily be ill-placed, the statement of the incongruity of this feature in a mountainous country may be supported by citing the example of incongruous effect of the Ambleside spire in a Westmoreland valley.

The work ends with an historical sketch, which shows much thought and learning. The author can, however, scarcely be correct in speaking of such structures as the Treasury of Atreus at Mycenæ as formed of large blocks of masonry with no architectural details whatever. It is possible that the ornate elaboration of the Beehive tombs at Mycenæ and Orchomenus, as shown in Perrot and Chipiez' work, before referred to, may be a good deal exaggerated; but there certainly exists evidence for a very considerable amount of architectural embellishment. In speaking of the derivation of the Corinthian capital, it seems unnecessary, with the small amount of evidence to the contrary which exists, to relegate to the regions of fable the touching little story told by Vitruvius (chapter iv. p. 1[illegible]) of its invention by Callimachus, especially as the earliest known example, in the temple at Bassæ, was the work of a contemporary, and probably a friend of the reputed inventor.

In p. 255 the very important derivation of the dome is traced from the Pantheon, of which the date (in the reign of Hadrian) has lately been established, and then the addition of the spherical pendentive by Justinian's architect (Anthemius of Tralles) in the great church of St. Sophia. To this is added the derivation of the architecture of the Western churches—which is traced—following Prof. Baldwin Brown ("from the Schola to Cathedral"); from the Roman house, of which the atrium and peristylium became the forecourt or parvis and the porch, whilst the basilica supplied the apse, and the widening of the basilica on each side of the tribunal gave the germ of the transepts of our cathedrals. In the summary of the different contributions made by the European nations to Gothic architecture, Italy is denied altogether a specimen of true Gothic—and yet it possesses in Milan Cathedral an interior perhaps more impressive than that of any other church.

"PARTURIUNT MONTES."

The Story of the Plants. By Grant Allen. (London: George Newnes, Limited, 1895.)

MR. GRANT ALLEN tells the story of plants in a readable and very inaccurate manner. The keynote to his work is struck in his preface, in which he informs his reader that he has "wasted comparatively little space on mere structural detail," and, later on, that he makes "trivial sacrifices of formal accuracy" in order to expound general biological relationships. It is true that he apologises for these amiable little weaknesses, but adds, in the same breath, that he lays before his "untechnical readers all the latest results of the most advanced botanical research." It is impossible to avoid giving some samples of these "latest results."

For Mr. Grant Allen, the plant is essentially the *green* plant, and the essential function of this plant is constructive metabolism. On the other hand the animal is the very opposite of this, "he is a destroyer, as the plant is a builder." But we fancy most people will hardly admit this antithesis nowadays. Plants and animals both exist by breaking down complex bodies to simple ones, but plants as a whole can get the energy required for first building up these complex bodies at a less expensive outlay than animals, and the green plants, as Mr. Allen perfectly correctly observes, are further able to make use of sources of energy (*i.e.* vibrations of ether) from which their less fortunate relatives are debarred. But to draw the distinction just quoted as the *essential* difference between the two kingdoms, is obviously misleading. However, Mr. Allen is at least consistent in his views, since he states that the first plants "must have been green."

In the account given of the *modus operandi* of the building up of organic matter in the plant, the author's claims to up-to-date knowledge will, we fear, hardly be admitted. Chlorophyll is said to be the active agent in splitting up (under the influence of sunlight) the carbon dioxide and water to form starch. Now every student knows that chlorophyll can do no such thing, and further he knows, or should know, that starch is certainly not a primary product of assimilation. The latter, perhaps,

is a "trivial detail," but Mr. Allen hastens to insist on the importance of "living chlorophyll" as the "original manufacturer and prime maker" of all the material of life, either vegetable or animal. Evidently chlorophyll is here doing duty for the alliance of chlorophyll with a vastly more important substance, protoplasm, but the author could hardly expect "untechnical readers" to appreciate this; and his statement that chlorophyll is a variety of protoplasm will certainly not meet with the assent of botanists. Again, the statement that "plants alone know how to make protoplasm" is one which is contradicted, fortunately for us all, by the experience of daily life; in order, however, that we may be quite clear as to the author's conception of protoplasm, he defines it (in italics) as "*the only living material we know*"; and this would seem to make it clear that he had not by a *lapsus calami* written protoplasm when he meant proteid. For a continuation of this subject, the critical reader may refer to pp. 190-191.

When Mr. Allen comes to deal with what we gather from his preface he considers the most important part of his work, we find evidences of hasty generalisations on insufficiently ascertained facts. Many plants which are certainly not degenerate, are regularly self-fertilised; and we submit that in most districts in England the humble bee has far more to do with the fertilisation of the Tropæolum than the Humming-bird hawk-moth; and this latter insect is certainly *not* the only one in Europe capable of performing this office.

But it is needless to multiply examples further. All we can say is that those readers who are ignorant of the real facts may find the book pleasant, though we can hardly add profitable, reading.

OUR BOOK SHELF.

Low's Chemical Lecture Charts. (London: Sampson Low, Marston, and Co., 1895.)

THIS is a series of diagrams intended to illustrate various chemical and metallurgical processes and apparatus, and designed more especially for the use of teachers who are preparing students for the examinations of the Science and Art Department, the London Matriculation, Oxford and Cambridge Local, &c.

There is no doubt that a good set of useful diagrams, of convenient size and moderate price, would be gladly welcomed by a large number of teachers, but the charts before us can scarcely be said to fulfil all the requirements of such a set of diagrams. The size of the sheets, namely, 30 in. × 40 in., is sufficiently large for the use of such classes as they are intended for, and it does not render them too bulky for convenient storage. In most cases the illustrations are very roughly executed enlargements of familiar cuts from various text-books and treatises on chemistry, sometimes well chosen, sometimes not. Many of the sheets contain several pictures, and where it happens that the subjects represented are in a manner related, this does not detract from their merit, except in so far as it necessitates the illustrations being smaller than if each occupied a single sheet. But in a number of instances the subjects depicted on the same diagram have no connection; thus, on the same sheet we find a representation of Hofmann's apparatus for showing the volume composition of water, and illustrations of certain apparatus used by Dewar in making experiments at low temperature.

Again, another diagram contains the following illustrations: (1) Hofmann's apparatus for composition of sulphur dioxide; (2) ozone apparatus; (3) apparatus for composition of ammonia; (4) apparatus for composition of hydrochloric acid; (5) Andrews' and Tait's ozone tube; (6) apparatus for composition of nitrous oxide; (7) Smithell's flame cone separator. With so many illustrations on one sheet, 30 in. × 40 in., each one must be almost insignificantly small, and quite erroneous ideas of the relative sizes of various pieces of apparatus are likely to be conveyed to the student. With some of the figures still more serious exception must be taken; thus, Fig. 2, Sheet 14, depicts a piece of apparatus, the design of which is of more than questionable feasibility; while Fig. 2, Sheet 17, is an impossible arrangement.

Many of the metallurgical figures are badly chosen. Thus, the old method for extracting zinc, known as "distillation per descensum," which has been quite obsolete for many years, is brought to life again in Diagram No. 11.

If these diagrams were a little better executed, and could be purchased singly, they would be of much more service to the general run of teachers, who could then select from a catalogue such as they might require.

G. S. N.

Brasilische Pilzblumen. Von Alfred Möller. Mit 8 Tafeln. (Jena: Gustav Fischer, 1895.)

THIS volume forms the seventh part of the "Botanische Mittheilungen aus den Tropen," edited by Prof. Schimper, of Bonn. The title—"Fungus-Flowers"—is suggestive of a popular and æsthetic treatment of the subject, but this impression is somewhat misleading, for Dr. Möller's work is of a strictly scientific character, and appeals more especially to systematic mycologists. At the same time, the extraordinary forms of the Fungi described give a considerable degree of general interest to the book, which is enhanced by the pleasant style in which the subject is treated. Dr. Möller is already well known for his mycological investigations, particularly for his fascinating work on the cultivation of Fungi by South American ants. The "Fungus-Flowers" are simply gastromycetous fungi of the family Phalloideæ, of which that repulsive plant the "Stinkhorn" (*Ithyphallus impudicus*) is the best-known British representative.

The author has been most fortunate in his investigation of the remarkable Brazilian forms of this family, which includes perhaps the most highly differentiated of the Fungi. He has founded no less that four new genera on his discoveries. One of these (*Protubera*) is referred to the Hymenogastreæ, and is of special interest, for it appears to connect that family with *Clathrus* among the Phalloideæ. The other new genera (*Blumenavia*, *Aporophallus*, and *Itajahya*) are members of the Phalloideæ, *Blumenavia* showing affinity with *Clathrus*, while the remaining two belong to the tribe Phalleæ. Eight new species are described in all.

The book is full of interesting details of the occurrence and mode of growth of these Fungi. It is illustrated by eight fine plates, many of the figures in which are from photographs of the specimens, while others represent their more minute structure. The first plate, a coloured representation of "the most remarkable of all Fungi," *Dictyophora phalloidea*, is especially striking. This is not one of the new species, but has never been adequately figured before. This extraordinary fungus bears a general resemblance to *Ithyphallus*, but is distinguished by the presence of an immense net-like indusium surrounding the stem, from which it stands out like a crinoline. The German colonists at Blumenau have given it the name of "the veiled lady."

Dr. Möller's book will be indispensable to students of mycology, and will no doubt attract more general attention to a most interesting group of plants, about which much still remains to be discovered. D. H. S.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The University of London.

SIR JOHN LUBBOCK does not seem to me to appreciate in the smallest degree the facts of the position.

His proposal is, as I and others understand it, that the result of the labours of the Statutory Commission "should be submitted to Convocation for their approval, *to be signified as at a material election.*"

The words which I have placed in italics propose a new procedure which I presume would have to be provided for in the Act. This is what for the sake of brevity has been called the *referendum.*

For reasons which I have sufficiently set out in my former letter I think the institution of the *referendum* extremely undesirable under any circumstances, and peculiarly open to objection in the present instance.

But I think we are now entitled to ask Sir John explicitly what he means when he says "it is the law at present," and that his "constituents highly value this right." In so grave a matter it is difficult to believe that he is indulging in a mere logomachy, or that he means seriously that the veto exercised under existing conditions and the new *referendum* are one and the same thing.

The meaning of the whole business is, of course, very simple. Convocation, in common with the Senate and practically everybody interested in the higher education in London, has expressed its approval of the Report of the late Commission as affording a basis for the reorganisation of the University. As Convocation is not to be moved from its decision expressed in the customary and constitutional way, the leaders of the minority, mainly drawn from the Faculty of Laws, have induced Sir John Lubbock to suggest a fundamental change in our procedure. The hope, of course, is that by this means a different result may be manipulated. I say "manipulated" because I entirely agree with Mr. A. W. Bennett, who in his admirable letter clearly indicates the kind of tactics we may expect. As the avowed object of the whole scheme is to set aside and nullify the action which Convocation has taken, I do not think that the language in which I described it is in any way inappropriate.

Sir John may be as polite as he likes to our intelligence. But what he has done is to constitute himself the instrument of those who would destroy the prospects of academic study in London, instead of making the University of London a better mechanism for the purpose for which it exists. And this is not what we had a right to expect of Sir John Lubbock.

Kew, August 10. W. T. THISELTON-DYER.

Note on Quaternions.

ON reading Cayley's famous memoir on matrices,[1] I have noticed in passing that in McAuley's[2] notation we may write in general,

$$\left.\begin{aligned} &\phi^{-1} = \underset{\phi}{D}\log m, & &\phi'^{-1} = \underset{\phi}{D}\log m\,; \\ &\psi = \underset{\phi}{D}m, & &\psi' = \underset{\phi}{D}m\,; \\ &\phi\underset{\phi}{D}\log m = \phi'\underset{\phi}{D}\log m = \psi^{-1}\underset{\phi}{D}m = \psi'^{-1}\underset{\phi}{D}m = 1. & & \end{aligned}\right\}\ldots\text{(A)}$$

where m is an invariant of ϕ, which being the original linear vector function, ψ is Hamiltonian inverse function, and I is the identity [illegible]; they are respectively defined by

$$mS\lambda\mu\nu = S\phi\lambda\phi\mu\phi\nu = S\phi'\lambda\phi'\mu\phi'\nu,$$
$$\psi = m\phi'^{-1}, \qquad I\rho = \rho.$$

I [illegible] we may prove the above relation by the variation formula

$$\delta Q = -Q_1S\delta\phi\zeta\underset{\phi}{D}_1\zeta,$$

given by Mr. McAuley, thus

[1] [illegible]

[2] [illegible]

$$\begin{aligned}\delta m &= -m_1S\delta\phi\zeta\underset{\phi}{D}_1\zeta = -S\delta\phi\zeta\underset{\phi}{D}m\zeta = -S\delta\phi\zeta\psi'\zeta \\ &= -S\delta\phi i\psi' i - S\delta\phi j\psi' j - S\delta\phi\kappa\psi'\kappa \\ &= -S\delta\phi i\phi j\phi\kappa - S\delta\phi j\phi\kappa\phi i - S\delta\phi\kappa\phi i\phi j \\ &= -\delta S\phi i\phi j\phi\kappa = \delta m.\end{aligned}$$

If W be any scalar function of ϕ, and if its independent variable be m (as it is so in some cases of the problems in elasticity, where m is the volume-dilatation), we might dispense with the notation $\underset{\phi}{D}$, for we may write in general,

$$\underset{\phi}{D}W = \frac{dW}{dm}\psi' \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(B)}$$

Also, if Q be any quaternion function of ϕ, and if its independent variable be m, we have again

$$\delta Q = -\frac{dQ}{dm}S\delta\phi\zeta\psi'\zeta \quad . \; . \; . \; . \; . \; . \; . \quad \text{(C)}$$

For, beginning with McAuley's form, we have

$$\begin{aligned}\delta Q &= -Q_1S\delta\phi\zeta\underset{\phi}{D}_1\zeta = -\frac{dQ}{dm}S\delta\phi\zeta\psi'\zeta \\ &= -\frac{dQ}{dm}[S\delta\phi i\psi' i + S\delta\phi j\psi' j + S\delta\phi\kappa\psi'\kappa] \\ &= \frac{dQ}{dm}\delta m = \delta Q.\end{aligned}$$

SHUNKICHI KIMURA.

Japanese Legation, The Hague, July 16.

To Find the Focal Length of a Convex Mirror.

THE following method is so much simpler than those ordinarily used, that it may be of interest to your readers.

Use as object an opaque screen with a hole and pin-point, and painted white, or covered with white paper.

Set up on the bench in line, say, with the left edge of the hole, the convex mirror and an auxiliary biconvex lens of short focal length (six inches or so), and adjust the lens so that the image of the hole and pin-point is formed side by side with the object. The centre of the mirror is now at the point at which the image would be formed by the lens alone; this position may either be calculated or found (after noting the position of the mirror and then removing it) by means of a screen. Thus the radius is easily measured.

If the focal length of the mirror be greater than f that of the lens, the simplest way of adjusting is to put the lens as close as possible to the mirror, put the object at principal focus of lens, and move the object back until the image is formed as above.

If, however, the focal length be less, we can be sure of finding the position by putting the mirror at a distance of $4f$ from the object, and the lens at $2f$, and moving the lens back until the desired position is reached.

The following is a simple way of making a direct measure of the focal length of a concave lens:—

Use an object like the one mentioned above, an auxiliary convex lens (say six inches focal length) to produce a convergent beam, and an auxiliary plane mirror, placed beyond the concave lens.

Adjust until the image is formed side by side with the object as before, then the rays must be emerging parallel to one another from the concave lens, and hence the convergent beam from the convex lens will (when the concave lens and mirror are removed) form an image at the principal focus of the concave lens. A direct measure can thus be made of the focal length.

I may add that both methods are very simple in practice.

Grammar School, Macclesfield. EDWIN BUDDEN.

Oceanic Islands.

IT is to be hoped that in the programme of the present Government a place will be found for an item humble and unimportant in the politician's eyes, but to the biologist of the utmost urgency—the sending out of a scientific expedition or expeditions to study the fauna and flora of oceanic islands before they are exterminated by continental importations. Let it be granted that men of science are busy with problems of even greater interest than those which such expeditions might help to solve. But among all the ambitious aims of science, it would be hard to find one to which delay would be more ruinous than to this—the

thorough knowledge of the inhabitants, whether animal or vegetable, of oceanic islands. The work must be done speedily, or it will be too late: and it is work that can hardly be undertaken on a sufficiently extensive scale without aid from Government.

Haileybury College. F. W. HEADLEY.

MICROGRAPHIC ANALYSIS.

METALLURGISTS would have been greatly astonished if they had been urged at the beginning of the present century to gather information as to the composition of samples of iron and steel by merely looking at polished and etched specimens through a microscope. The operation is, nevertheless, rapidly taking its place in the ordinary routine of a works laboratory.

As regards the history of the development of this new branch of investigation, it appears that micro-metallography has not been developed from petrography. It is the natural extension of the study of meteoric iron, and, as has often happened in the history of science, it seems to have had more than one independent origin. Priority of date rests with our own countryman Dr. Sorby. In 1864 he submitted to the British Association photographs of opaque sections of various kinds of iron and steel, and he endeavoured to develop a method for the industrial examination of such sections under high powers, preferring polished sections to fractured surfaces. The abstract of his paper is very brief; but looking back, it seems strangely comprehensive and suggestive. He claimed that the sections showed "various mixtures of iron, two or three well-defined compounds of iron and carbon, of graphite, and of slag; and these, being present in different proportions, and arranged in various manners, give rise to a large number of varieties of iron and steel differing by well-marked and very striking peculiarities of structure."

Later, Prof. Martens, in Berlin, without neglecting the examination of sections, carefully studied, in 1878, the general laws which govern the occurrence and formation of fractures, fissures, blow-holes, and crystalline structure in metals and alloys. His work, therefore, presents all the characteristics of perfect originality. It was not long after the publication of Martens' work that M. Osmond, then engineer at the Creusot Works, began, with his colleague M. Werth, investigations on the cellular structure of cast steel. This work was published by the *Académie des Sciences* in 1885, and in order to trace the progress which has been made in micro-metallography during the past ten years, it would be difficult to do better than consult the beautiful monograph by M. Osmond which has recently been published by the *Société d'Encouragement* of Paris.[1]

As M. Osmond justly observes, metallography should in its early days be descriptive; it should enable us to determine the form and nature of the various constituents of alloys, to ascertain their mode of distribution, and to measure their dimensions. Later on, when sufficient data have been established, it will be possible to apportion the observed facts to their respective causes (1) by ascertaining the way in which the structure of a given metal changes under the influence of the three combined factors—temperature, time, and pressure, and (2) it will be possible to trace the relations between the observed facts and their consequences by defining the mechanical properties which correspond to a particular structure.

The first step in the complicated procedure is to cut and polish the opaque specimens of steel. The methods do not admit of condensed description, and the original memoir must be consulted, as even the technical manuals of crafts, in which the polishing of metals plays a part, give but little information that is useful in the preparation of metallic sections for the microscope. It must, however, be added that one method of polishing is specially designed with a view to wear away the softer constituents of the specimen, and bring the harder into relief. It is often useful to attack a polished specimen of steel with a reagent which will colour certain constituents only. For this purpose M. Guillemin treats sections of bronze by oxidation, at regulated temperatures, which produces varied colourations on several constituents of the alloy, while M. G. Charpy prefers an electrolytic attack. It is somewhat surprising to find that an infusion of *coco* (a popular French term for an infusion of liquorice) is very useful for the purpose, which recalls the fact that Japanese artificers have, for centuries, used plum-juice vinegar, decoctions of finely-ground beans (*Glycine hispida*), or extracts of the roots of certain plants, as valuable agents for colouring the peculiar alloys which they employ in art metal-work. It may be that the micro-metallographer has much to learn from the Japanese.

The "attack" of polished specimens is made by suitable reagents, which may be divided into the three classes—acids, halogens, and salts. Of the acids, nitric acid of 36° Baumé appears to be the most useful. Of the halogens the pharmaceutical tincture of iodine gives excellent results, as it removes carbon from the steel, and colours certain portions of the specimen. Such treatment, the nature of which has been so briefly sketched, will serve to reveal the main constituents of steel. These are five in number, and it has been found convenient to give mineralogical names to them, following the suggestion of the distinguished American metallurigist, Mr. Howe. Thus pure iron is called *ferrite*; the carbide of iron, Fe_3C, of Abel, *cementite*. This is not coloured by the infusion of *coco* or tincture of iodine, which latter leaves it of a silver-white brilliancy under vertical illumination. Dilute nitric acid in the cold does not affect *cementite*. The third material is one of the components of the "pearly constituent of Sorby," which may be coloured by coco or by iodine, and M. Osmond proposes the name of *sorbite* for it, though he is uncertain as to its exact constitution. The fourth constituent, to which he gives the name of *martensite*, is that which is ordinarily obtained by the rapid cooling of a specimen of steel during the familiar operation known as "hardening." It is a crystalline, fibrous substance which iodine colours readily either yellow, brown or black, according to the amount of carbon it contains. Now, martensite preserves its characteristic forms equally well in very low carbon-steels which have been hardened, as well as in high carbon-steels which have been subjected to this process. It may be urged, therefore, that martensite is not a carbon-iron compound which has liquated out of the mass, but that it represents the crystalline organisation, formed under the influence of carbon by one of the allotropic forms of iron.

The last of the five constituents of steel, marks the transition of soft iron into hardened steel. The name of *troostite* is after the eminent chemist, and it resembles sorbite, but its composition is as yet uncertain. This name is not well chosen, as a variety of silicate of zinc has long been known as troostite.

It will be evident that a micro-section of a mass of steel closely resembles a rock-section which has constituent minerals distributed through it. It should, however, be pointed out that there are cases in which the existence of these several constituents cannot be sharply defined, as it is frequently necessary to deal with transition forms which defy classification. Sorbite, troostite, and martensite appear to be solidified solutions of various forms of carbon in diverse forms of iron, for it seems clear that metallographic work on steel brings into prominence the existence of allotropic forms of iron.

In order to realise how complicated the structure of

[1] "Methode generale pour l'Analyse micrographique des aciers au carbone," par M. F. Osmond (*Bull. de la Soc. d'Encouragement*, vol. x., p. 4[illegible], 1895).

ordinary steel really is, reference must be made to some facts recorded in Nature, vol. xli. 1889, p. 32. An attempt was therein made to show that notwithstanding the importance of the part played by carbon in the hardening of steel, the phenomena of hardening cannot be explained solely by a change in the relations of carbon to iron. The iron itself appears to change its state, and M. Osmond has shown that it probably assumes at least three distinct allotropic forms, which he designates respectively as α, β, and γ iron.

The fact that the iron itself may exist in more than one state, brings into prominence the causes which underlie the difference between an ordinary rock-section and that of a metal or metallic alloy. In granite, for instance, as the fused mass cools the quartz, mica and feldspar fall out of solution in distinct crystalline masses; and although the fusibility of the mass, and consequently its structure, may be greatly influenced by the presence or absence of a small quantity of impurity, say two or three per cent. of sodium, still, so far as we know, complications do not arise from allotropy of the constituent elements of the rock. In the case of a specimen of carburised iron the conditions are widely different. It is certain that one very vital change in the relations between the carbon and the iron does actually take place at 650° C., that is to say, at a temperature far below the fusing point of the mass. The decomposition of the carbide of iron, Fe_3C, may take place at various rates. Cementite can, for example, under sufficient pressure, resist decomposition at a temperature well above that at which it would ordinarily decompose, and we are confronted with the complications which ensue when carbon is united, not merely with α iron, but with β or γ iron, so as to form either $Fe\beta C$ or $Fe\gamma C$.

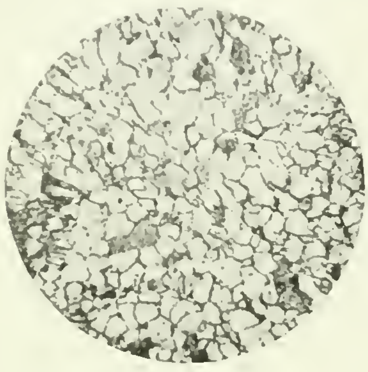


Fig. 1.

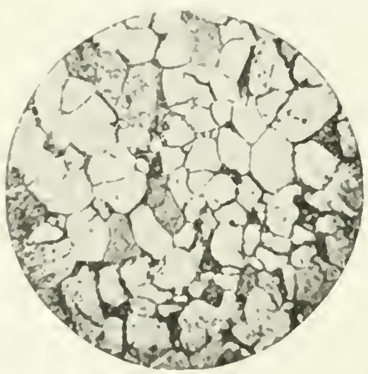


Fig. 2.

A few examples will serve to make the method of investigation clear. The effect of annealing steel is very remarkable. The operation consists in raising the metal to a high temperature and in allowing it to cool slowly. A granular structure is thus developed in the metal, the size of the polyhedral grains being proportional to the temperature to which the metal is raised. If the temperature is over 1000° C. the grains of ferrite (iron) will be large, while the perlite remains outside the grains and arranges itself in the joints round them. Fig. 1 represents a sample of very mild steel containing 0·14 per cent. of carbon which had been forged and etched with dilute nitric acid; while Fig. 2 represents the same steel which has been cooled from an initial temperature of 1015° C. In it the ferrite has arranged itself in larger grains than was the case in the first section, which had not been raised to nearly so high a temperature before cooling. Now compare this with Fig. 3, which shows the effect of raising the steel to an initial temperature of 960° C., allowing it to cool down to a temperature of 770° C., and then cooling it rapidly by quenching it in water. Microscopic examination shows that the interstitial matter is martensite, together with some troostite, while the principal mass is still ferrite in grains. These three specimens, chosen, it should be remarked, from the eighty-five beautiful photographs given by M. Osmond, serve to show how much the structure of the same variety of steel will vary with the thermal treatment to which the metal has been subjected. Fig. 4 shows a sample of more highly carburised steel polished with rouge, which presents a vermicular surface of ferrite and perlite.

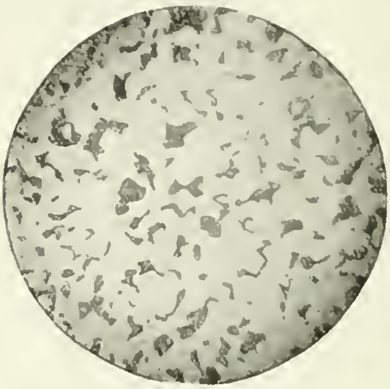


Fig. 3

Fig. 4.

There would appear to be no limit to the applications of micrographic analysis, as all metals and all alloys may be subjected to its action. It is known, for instance, that the qualities of the copper alloys are greatly modified by the addition of minute quantities of deoxidising agents, such as phosphorus, aluminium, or silicon, and

M. Guillemin, in an admirable paper on the metallography of the alloys of copper presented to the French "Commission des Méthodes d'essai des Matériaux de Construction,"[1] has given evidence that it is possible to pronounce with certainty, by the examination of etched surfaces of examples of the alloys, which deoxidiser has been employed.

It remains to be seen in what way the mechanical properties of steel are connected with the structural changes revealed by micrographic examination. In every specimen of steel, as has already been stated, at least three great molecular changes are produced as the metal is raised from the ordinary temperature to a white heat. The belief that the rearrangement of atoms in the molecule of iron (which is, in fact, allotropy) is really fundamental to these molecular changes, is rapidly gaining adherents, but authorities on hardening of steel are by no means in accord as to the true significance of allotropy in relation to that important industrial operation. The writer of this paper has long declared himself to be a pronounced allotropist, and many patient experimenters are hard at work at the problem. M. Charpy,[2] for instance, had already pointed to the peculiar behaviour of steel under longitudinal stress, as proof that the metal undergoes allotropic change. He now seeks, by an elaborate series of experiments, to ascertain whether the mechanical tests of steel which has been quenched at definite temperatures, support Osmond's view as to the significance of the part played by allotropy of iron in the hardening of steel. Charpy's opinion seems to be that, on the whole, his experiments do not afford conclusive evidence in support of Osmond's view. It may, however, be urged that in the case of steel, mechanical tests could not be expected to afford decisive evidence in relation to the theoretical significance of allotropy, because, as M. Osmond's micrographic work shows, the structure of steel is so complex and varies so much with thermal treatment. It is, of course, ultimate structure which determines the strength and elasticity of steel, and none of us claim that allotropy is the sole factor in the production of structure.

The magnetic behaviour of steel, on the other hand, as M. Curie has recently pointed out, is greatly influenced by temperature, for, within the range of 20° to 1350°, rapid variations in magnetic properties of soft iron reveal themselves at about 750°, 860°, and 1280°. This, as he says, is favourable to the views of M. Osmond, because on independent evidence we are led to conclude that at temperatures near these points the metal undergoes allotropic modifications.

It is to be hoped that microscopic analysis will soon take its place in the ordinary routine of every steel works laboratory, and it should be added that in this country two well-known authorities, Mr. T. Andrews and Mr. J. E. Stead, constantly employ it, while Mr. A. Sauveur[3] has originated the system already in the works of the Illinois Steel Company. W. C. ROBERTS-AUSTEN.

THE SCIENTIFIC RESULTS OF THE ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION.

THE annual meeting of the British Medical Association is, no doubt, increasing in importance, since it is becoming a congress for the demonstration of the advance of medicine. The work of the meeting may be considered as belonging to two classes, the practical and the scientific. Many, no doubt, who attend the annual meeting, do so with the object of gaining practical help in both the medical and the surgical treatment of their patients; and this help the annual meeting gives in abundance. One of the most important parts of the meeting, however, is that which is occupied with the progress of scientific medicine, and consists not so much in the announcement of startling discoveries (for with these medicine has but little to do), but in the revision and criticism of the facts discovered by experiment and at the bedside.

Medical science is becoming more exact, as the knowledge of the functions of living tissues (physiology) and their changes in disease (pathology) increases.

It is not so many years ago when the chief subject in what was called physiology was histology, or the structure of the tissues. Physiology proper then rapidly progressed, and although at first it was considered from a somewhat too physical standpoint, and indeed is still so considered by some, yet it has received an enormous impetus by being associated with the study of chemistry and of the action of the chemical constituents of the body on the living tissues. This is evidenced in the excellent address on "Internal Secretion," given by Prof. E. A. Schäfer, F.R.S., of University College, a subject which in its scientific aspects is of a quite recent development. A secretory organ may, like the stomach, salivary glands, &c., separate materials from the blood and pour them into a cavity, in which they are utilised; this may be called external secretion. On the other hand, "some secreted materials are not poured out upon an external surface at all, but are returned to the blood"; these may be called internal secretions. Although it is probable that in the widest sense every tissue has an internal secretion, yet this is most obvious in the ductless glands, such as the thyroid, the suprarenal bodies, and the pituitary body. But in one gland with an important external secretion, viz. the pancreas, there is also an internal secretion which is of great value in the economy.

The subject of internal secretion has developed hand in hand with clinical medicine, and it was the observation of patients which first, as in the case of the thyroid, gave the clue to the line of investigation. It is impossible in this place to give a detailed account of Prof. Schäfer's address; it is well worthy of study by every one interested in the progress of biological science. It will not be out of place, however, to illustrate the subject of internal secretion by quoting as examples the investigation of the pancreas and the suprarenal capsules, the latter of which has been the subject of special study by Prof. Schäfer, in conjunction with Dr. G. Oliver and Mr. Moore.

The association of disease of the pancreas with the presence of sugar in the urine has long been noted; although only a certain proportion of cases of diabetes show any great changes in this organ. If the pancreatic juice be diverted from the intestine, or if the duct be blocked, the animal experimented upon does not die, there is no glycosuria, nor does it apparently suffer any great nutritional change. If, however, the pancreas be totally extirpated, glycosuria appears, and the animal invariably dies; this does not occur, however, if only a part of the organ be removed. More than this, if a portion of living pancreas be successfully grafted into an animal from which the organ is subsequently completely removed, no evil results follow. Besides its obvious and important function of secreting a digestive juice, the pancreas therefore produces some material which it gives to the blood, and which is essential for the continuance of life: this is the internal secretion. On the other hand, it is suggested that the organ nominally separates and transforms some toxic substance which is fatal to existence; this is the theory of auto-intoxication. The internal secretion of the suprarenal capsule is more obvious, perhaps, than that of the pancreas. The capsule is a ductless gland: it has no external secretion. The complete

[1] "Analyse Micrographique des Alliages." (*Comptes rendus*, vol. cxv. p. 232, July 25, 1892.)
[2] "Bull. de la Soc. d'Encouragement," vol. x. 1895, p. 660.
[3] "Trans. Amer. Soc. Mining Engineers," vol. xxii. p. 546.

removal of both suprarenal capsules results in rapid death, which is preceded by great muscular weakness, diminished tone of the vascular system, and some nervous symptoms; a combination of events which is seen in Addison's disease, which is a disease of these organs. From the medullary portion of the gland, Schafer has obtained an extract containing an active substance which is remarkable as producing its effects in very small doses (as little as $5\frac{1}{2}$ milligrams in a dog weighing 10 kilos.), and as being capable of withstanding for some time the temperature of boiling water. This substance increases the duration of the contraction of muscle, as tested by the apparatus ordinarily in use in the physiological laboratory; but it has a more remarkable effect in greatly increasing the blood pressure, a result following a direct action on the peripheral arteries. In the case of the suprarenal capsule, there is thus distinct evidence of internal secretion; that is, of the presence in one part of the gland of a substance which has a well-marked physiological effect. Into all the questions arising out of this subject it is impossible now to enter. The subject is one of vast importance to scientific practical medicine. As the results of future investigation, we may hope to obtain not only a greater knowledge of the pathology of some obscure nutritional diseases, but some indications for their relief and treatment. This has already been accomplished in the case of myxœdema, in which the thyroid gland is degenerated, and in which very great benefit is obtained by feeding the patients with fresh thyroid gland, or by injecting the extract.

One other scientific result of the annual meeting may be viewed. It is the predominant place now given in the study of disease to the question of infection. All disease is not infective, but infection, in theory, has for many decades played an important part in pathology. The great change which has come over medical science is, that the question of infection is now studied from an experimental point of view. Vague theories have given place to facts, which are of prime importance, not only in the understanding of disease, but in its treatment. In the investigation of diseased, as well as of normal functions, the application of chemical methods has been of great service, and is destined to be of still greater importance.

The accurate study of infection deals with a far wider subject than the characteristics of the infective agent; since it is also concerned with the reaction of the body against the micro-organism and the poisonous chemical substances this produces. The study of this reaction of body has, from the morphological point of view, given a clearer view of the processes occurring in inflammation; and from the chemical point of view, it has opened up a wide field of possible therapeutical agents. The prospect is one which is reassuring for the future. The fact that infection is being so closely studied, and that the infective agents in so many diseases have been isolated, is of great importance to the human race; since infection is preventible. The fact that the body, in reacting against an infective disease, produces a substance which counteracts invasion, as well as the poisonous bodies formed by the infective agent, is of as great importance as the first point; since an infective disease may be cured. At the annual meeting, the discussion on pneumonia as an infective disease (a discussion which would have been impossible, and would even have been considered heterodox only a few years ago) as well as the discussion on the utility of the diphtheria anti-toxin, illustrates the point mentioned. In the discussion on diphtheria, the great majority of the speakers, both those who considered the subject from the scientific aspect and those who looked at it simply from the practical point of view, agreed that the use of the anti-toxin in the disease was not only based on a firm scientific basis, but that it had completely changed the aspect of the disease. Whatever the limitations of the treatment by anti-toxic serum may in the future be proved to be, there can be but little doubt that its discovery marks an epoch in the treatment of infective disease.

THE IPSWICH MEETING OF THE BRITISH ASSOCIATION.

THE arrangements for the meeting of the British Association at Ipswich this autumn are making rapid progress. The General Election somewhat interrupted the preparations of the local secretaries, but the excitement being now over, general attention in the locality is again centred on the coming visit of the Association, and great efforts are being made in the town and neighbourhood to ensure the success of the meeting. The chief public buildings in the town are just emerging from the hands of the painter and decorator. The reception room will be located in the Town Hall, the council chamber being the room actually set apart for the purpose, whilst the library will be the writing room. The President's address and the evening discourses will be delivered in the public hall, as will also the lecture to working men. In the matter of Section rooms, the Local Committee will be able to offer the Association very good accommodation, as there are fortunately a number of suitable rooms and halls in the town within a very short distance of each other, and all are close to the reception room. The two halls at the Girls' High School, which were formerly the New Assembly Rooms, and were used for the reception room and for Section E on the occasion of the Ipswich meeting in 1851, will be allotted to Section A (Mathematical and Physical Science) and Section B (Chemistry). About two hundred yards distant is the Co-operative Hall, in which Section G (Mechanical Science) will meet. Section C (Geology) will be accommodated in the Art Gallery adjoining the Museum. Section D (Zoology) and the new Section K (Botany) will have, respectively, the banquet room and the lodge room at the Masonic Hall. The Lecture Hall, adjoining the Ipswich Institute, will be given over to Section E (Geography), whilst across the street, the Working Men's College (formerly known as the Old Assembly Rooms) will be set apart for Section H (Anthropology).

The proceedings will commence on the evening of Wednesday, September 11, when the Marquis of Salisbury will retire from the presidential chair, and Sir Douglas Galton will take his place. The new President will then proceed to deliver his address. The second evening will, as usual, be devoted to a conversazione, which will probably be held in the museum and the adjoining buildings, used as art and technical schools. On Friday evening Prof. Silvanus P. Thompson will deliver a lecture on "Magnetism in Rotation." On Monday evening Prof. Percy F. Frankland will discourse on "the work of Pasteur and its various developments," and on Tuesday there will be a soirée given by the Ipswich Scientific Society and the Suffolk Institute of Archaeology jointly. This, like the first soirée, will probably be held in the Museum buildings. The lecture to working men will be given on the Saturday evening by Dr. Alfred H. Fison, who takes "Colour" for his subject.

In response to a special invitation which the Local Committee issued to foreign men of science, the following gentlemen have signified their intention of being present at the meeting:—Prof. A. Gobert (Brussels), Prof. W. E. Ritter (Heidelberg), Rev. T. Adams (Canada), M. J. Dantzenburg (Paris), Dr. O. Maas (Munich), M. Boule (Museum d'Histoire naturelle, Paris), Prof. Ira Remsen (Johns Hopkins University, U.S.A.), Prof. Runge (Hanover), Prof. E. C. Hansen (Copenhagen), Dr. van Rijckevorsel (Rotterdam), M. G. Dolfus (Paris), His Excellency Don Arturo de Marcoartu, M. E. van den Broeck

(Brussels), Prof. Michie Smith (Madras), M. A. P. N. Franchimont (Leiden), Dr. H. Haviland Field (New York), Dr. Bashford Dean (Colombia College, New York), Prof. J. W. Langley (Ohio, U.S.A.), Dr. Paschen (Hanover), Dr. Conwentz (Dantzic), M. Berthelin (Paris). A large number of the leading scientific men in England have already notified that they will attend the meeting.

The hon. local secretaries for the meeting are Messrs. S. A. Notcutt, G. H. Hewetson, and E. P. Ridley. All communications to them should be addressed to the Museum, Ipswich.

BAILLON, BABINGTON, EATON.

BY the death of Henri Ernest Baillon, France has lost one of her most accomplished botanists, and certainly her leading systematist. Under date of the 19th ult. the writer received the following lines from a friend at the Muséum d'Histoire naturelle, Paris.

"Je vous écris sous une bien pénible impression; M. Baillon est mort hier soir subitement. Dans l'après midi il était venu au laboratoire selon son habitude. À 5 heures et demie il prit un bain: à 6 heures son fils rentrant de l'École de Médecine le trouva mort. On croit que le bain, un peu trop chauffé, a déterminé une congestion.

"C'est une grande perte pour nous et pour la botanique. S'il avait des ennemis implacables, il avait aussi des amitiés fidèles. Je ne doute pas que l'avenir ne montre que derrière un esprit, dont les manifestations parfois acerbes visait moins la personnalité que ce qu'il jugeait être l'erreur, se cachait un cœur sensible a l'excès. Il est un bon nombre de ses élèves pauvres qui savent de quelles délicatesses il savait entourer une aumône.

"Quoiqu'il en soit c'était un grand botaniste; vous le jugez ainsi, n'est ce pas?

"Ses quatre enfants vont se trouver dans la misère la plus profonde qu'on puisse imaginer. Ce qu'il n'a pas dépensé de sa fortune pour la publication de ses livres a disparu dans le gouffre des dettes de celle qui a porté son nom. Aujourd'hui il ne reste rien."

The allusion to Baillon's personal character in the foregoing letter will appeal to the sympathies of those who knew him on this side of the channel. Unfortunately he quarrelled with some of the foremost French botanists of assured position, which led to regrettable and undignified recriminations on his part, and resulted in closing the doors of the Académie des Sciences against him for ever. This embittered his life considerably, and rendered his relations with a section of the botanists of Paris almost unbearable.

For most of the following particulars of Baillon's career I am indebted to the author of the above letter. Henri Baillon, as he usually signed himself, was born at Calais, November 29, 1827, of a family of good position and reputation in the town and district. He studied with great distinction at the Lycée de Versailles, and commenced his medical education at the age of seventeen. In 1854 he became house-surgeon at the Hôpital de la Pitié, Paris, a position obtained only by severe competition; and he was so brilliantly successful in his work, that he was unanimously awarded the gold medal of the Internat, the highest reward at the disposal of the Faculté de Médecine. His candidature for the degree of Docteur en Médecine was a perfect triumph, for he completely held his examiners, both by the elegance of his diction and the depth of his scientific views. In 1863 he succeeded Moquin Tandon in the Chair of Botany at the École de Médecine, and he filled this chair up to the time of his death; and for some time was Professor of Botany at the Lycée Napoléon as well. He was also Docteur ès Sciences. In 1875 he was elected a foreign member of the Linnean Society of London, and last year he received the same distinction from the Royal Society. This gave him much pleasure, and consoled him, in some measure, for the implacability of his own countrymen. In 1866 he and a few others founded the Société Linnéenne de Paris. He was elected president, and continued to act as such until his death. For some years the *Proceedings* of this very small Society were published in Baillon's own periodical, *Adansonia*, and then a *Bulletin Mensuel* appeared, and has continued to appear down to the present time, entirely owing to the energy and industry of the president. This organ was not published, but distributed to the leading botanical establishments; hence there is no record of Baillon's numerous articles therein in the Royal Society's catalogue of scientific papers. Yet, omitting these, the catalogue contains the titles of 230 of his papers, published between 1854 and 1883. But Baillon was a most prolific writer, and covered a considerable range, though systematic botany was his chief study. I need only name his *Adansonia*, twelve volumes, 1866 to 1879; "Dictionnaire de Botanique," four volumes, 1876 to 1892; "Histoire des Plantes," 1867–95, and still unfinished. Baillon, too, was the only French botanist who occupied himself on the rich collections of flowering plants in Paris from Madagascar; being the author of the uncompleted "Histoire des Plantes de Madagascar," forming a portion of Grandidier's great work on Madagascar.

Baillon was one of the few existing botanists having a good knowledge of the phanerogamic flora of the world. As a writer, however, he was more critical than methodical, and many of his original observations and suggestions have been overlooked by botanists who have subsequently gone over the same ground. This is owing to the fact that the titles of many of his articles do not sufficiently describe their contents. Not infrequently a new genus or a new species is described in the body of a paragraph, and sometimes so informally, that only by careful reading is it possible to arrive at the fact. This often caused the author himself chagrin, especially as he was very sensitive and apt to believe that his work had been purposely ignored. I had almost forgotten to mention that the Euphorbiaceæ were one of his favourite families, and his "Étude Générale du Groupe des Euphorbiacées" is one of his most finished works. This is not the place to enter into a more critical examination of his works, but I cannot help mentioning that the illustrations almost throughout are of a high order of merit. Dr. Baillon has been a frequent visitor to Kew and the British Museum during the last thirty years, and many botanists will join me in regret for his sudden death whilst apparently in almost the full vigour of life.

The veteran Professor of Botany, Charles Cardale Babington, in the University of Cambridge, whose death has lately taken place, was born at Ludlow in 1808, and educated at St. John's College, Cambridge, taking his B.A. in 1830 and M.A. in 1833. As long ago as June 1830, he was elected a Fellow of the Linnean Society; yet there are still two of earlier date in the Society's list, namely, Dickinson Webster Crompton and William Pamplin, both elected the previous January. There are only two others, Thomas Archer-Hind and James Bateman, who have been Fellows of the Society for upwards of sixty years. In 1851 Babington was elected a Fellow of the Royal Society, and among the fifteen of that year, it may be mentioned, were the late Prof. Huxley, Lord Kelvin, Sir James Paget, and Sir Gabriel Stokes. In 1861 he succeeded the Rev. J. S. Henslow in the Botanical Chair at Cambridge, a post he held up to his death, though for many years he was incapacitated from performing the duties. Prof. Babington was, in his early years at least, a prolific writer, his first paper appearing in 1832. His writings were almost exclusively on the British flora; and his name will stand in the history of British botany as the inaugurator of a more critical delimitation

of species than had previously found favour in this country. Taking Koch and Fries as his models, from whom he largely borrowed, he published the first edition of his "Manual" in 1843. This new departure caused considerable commotion and opposition from the older school of botanists; and the fact that Babington did not possess the critical acumen and originality of the masters in his adopted school, sometimes exposed him to attacks. Nevertheless the "Manual" was a success, passing through eight editions, the last of which appeared in 1881; and it still enjoys great favour, even among those who do not go so far in the matter of species. Babington was also author of several local floras; the first being the "Flora Bathoniensis," 1834; followed by the "Flora Sarniensis," 1839, and a "Flora of Cambridge," in 1860.

Daniel Cady Eaton, who belonged to a school of American botanists, of whom very few survive now, was the grandson of Amos Eaton, the author of the formerly famous "Manual of the Botany of North America," which passed through many editions; and son of General Amos E. Eaton, also a devotee of natural history. D. C. Eaton was born in 1834, and early evinced a liking for botany. After a successful career at school and college, he experienced many changes, including service in the federal army during the civil war. In 1867 he was called to the Botanical Chair of Yale College, New Haven, which he held until his death. As an author he will be best remembered by his writings on North American, Mexican, and West Indian ferns. His principal, or at least most popular, work is his "Ferns of North America," illustrated in colours by J. H. Emerton and C. E. Faxon.

W. B. H.

NOTES.

DR. BERGH, of Copenhagen, has been elected a Correspondant of the Paris Academy of Sciences, in the Section of Anatomy and Zoology.

THE resignation is reported of Mr. R. Trimen, F.R.S., Curator of the South African Museum, Cape Town, and also of Mr. R. L. J. Ellery, C.M.G., F.R.S., Director of the Observatory at Melbourne.

THE deaths are announced of Dr. Adolf Gerstacker, Professor of Zoology in the University of Greifswald; Dr. Pellegrino Strobel, Director of the Natural History Museum at Parma; Prof. H. Witmeur, Professor of Mineralogy and Geology in the University of Brussels; and Dr. W. Fabritius, Astronomer at the Kieff Observatory from 1876 to 1894.

THE French Association for the Advancement of Science met at Bordeaux last week. It was at Bordeaux that the Association held its first meeting in 1872, and this year the same cordial hospitality was accorded to its members as was given twenty-three years ago. The president of the recent meeting was M. Émile Trélat, and in his presidential address on "La Salubrité," he indicated the place of hygiene among the sciences, and traced its limits.

THE annual congress of the British Institute of Public Health was opened at Hull on Thursday last, under the presidency of the Mayor. On Friday, Sir A. Rollit delivered an address as president of the municipal and parliamentary section of the Institute, and Dr. Cameron delivered an address in the section of preventive medicine. It was resolved on Monday "That in the interest of public health all municipal and local authorities should be empowered to provide crematoria, and that a petition be presented to Parliament in support of the Bill about to be promoted to secure this object." Prof. W. R. Smith brought forward the subject of the influence of schools on diphtheria, and in the course of his remarks contended that schools did not play that important part in the spread of diphtheria which they had been supposed to do. The final sitting of the congress was held on Tuesday, when the reports of the several sections were adopted, and a resolution was passed that every house in a watering-place where lodgers were accommodated should undergo a survey by the sanitary authority, and that a certificate of fitness should be compulsory.

THE annual summer meeting of the Institution of Junior Engineers, the headquarters of which are in London, takes place from August 17 to 24, the rendezvous being Belgium. The towns to be visited include Antwerp, where the municipal docks, M. Kryn's diamond-cutting works, and other places of interest will be opened to members' inspection. At Ghent, MM. Carel's engine works, M. de Hemptinne's cotton-spinning works, and M. Van Houtte's nursery gardens will be seen; at Brussels, the electric lighting station; whilst at Liège, the works of the Société Cockerill, the Vielle Montagne zinc works, the St. Leonard locomotive works, the Val St. Lambert glass works, the Small Arms Factory, and the Electric Tramway Installation will be visited. In honour of the Institution a banquet is to be given by the Liège section of the Society of Engineers from the University, and the members will also be the guests of the Société Cockerill. An excursion to Verviers, where the Chamber of Commerce will entertain the visitors, is arranged for the purpose of seeing works in connection with the woollen cloth industry. Here MM. Peltzer's works and those of M. Duesberg-Delrez, La Vesdre, and M. Hauzeur Gerard fils, will be opened. The celebrated Gileppe reservoir, from which Verviers receives its domestic and manufacturing supply, is also included in the programme. A large number of members have notified their intention of being present at the meeting, which promises to be one of the most successful the Institution has held.

AN auto-mobile carriage race between Chicago and Milwaukee, promoted by the *Times-Herald* of Chicago, will be decided on Saturday, November 2, the object being to encourage and stimulate the invention, development, perfection, and general adoption of motor carriages. The amount offered in prizes is 5000 dollars, apportioned as follows: First prize, 2000 dollars and a gold medal, open to competition to the world; second prize, 1500 dollars, with a stipulation that, in the event the first prize is awarded to a vehicle of foreign invention or manufacture, this prize shall go to the most successful American competitor; third prize, 1000 dollars; fourth prize, 500 dollars. The third and fourth prizes are open to all competitors, foreign and American. The rules laid down stipulate, among other things, that no vehicle shall be admitted to competition which depends in any way upon muscular exertion, except for purposes of guidance. Competing vehicles which derive their power from petroleum, gasoline, electricity, or steam, and which are provided with receptacles for storing or holding the same, will be permitted to replenish the same at Waukegan, Ill., and at Kenosha, Wis., but at no other points.

DURING the past week the weather over the United Kingdom has been very unsettled, owing to the advance of various low-pressure areas from the Atlantic. Several heavy thunderstorms have occurred, the most severe being on Saturday night, the 10th inst., over the southern and south eastern parts of England. In London the storm was very violent, and the lightning was of unusual brilliancy. The disturbance travelled from south to north, and was accompanied by heavy rain. An exceptionally heavy thunderstorm also occurred at Holyhead on the same night, and the rain measured there on the next morning amounted to 2·08 inches. The *Weekly Weather Report* of the 10th inst. states that the rainfall for the week exceeded the average in all districts, the amount over England being about twice as much as the mean.

The Meteorological Office has received through the Colonial Office a report from the Governor of Hongkong, according to which it appears that the colony was recently suffering from a great drought; the rainfall from January 1 to June 23 last having been only 13·7 inches, being a deficit of no less than 28·7 inches on the mean of the corresponding period of the previous five years. The Governor draws attention to the fact that between October 1893 and April 1894, the colony suffered much from want of rain, and that the plague of the latter year was supposed to have originated from a deficient water supply. Though the drought of the first half of this year has been far more serious than that of 1893–94, the plague has not yet re-appeared in an epidemic form; but the reservoirs had, at the date of the despatch (June 26), only about a week's supply left in them. From a return furnished by the Director of the Hongkong Observatory, it appears that the greatest deficiency has occurred during May and June, when it amounted to 11 and 12½ inches respectively.

La Technologie Sanitaire is the title of a new journal devoted to questions of water supply and applied hygiene. It is published in Louvain, and is edited by a civil engineer, Victor J. Van Lint. The first number, amongst other contributions, contains an interesting and useful article by M. Ad. Kemna, the well-known Director of the Antwerp Waterworks, on "The Theory of Sand Filtration." The practical genius of the English in the past is emphasised in commenting upon Simpson's introduction of sand filters in London in the year 1839, and we are told that having produced such brilliant results, it is not surprising that as a nation we are so slow and reluctant to adopt more modern methods and change our system of technical instruction! Besides original articles, reviews of books are also appended, and what, perhaps, is one of the most useful features of this undertaking, is the bi-monthly issue of a supplement, international in character, containing a bibliography of books, pamphlets, &c., published on subjects connected with water supply, together with short notices of public hygienic enterprise in different parts of the world.

Infra-red light is invisible to us either because the humours composing the eye are opaque to it, or because the light is incapable of exciting the retina. Cima and Janssen have adopted the former explanation, but the alternative one has been accepted by Tyndall, Engelmann, and others, while Helmholtz maintained that the strong absorption suffered by infra-red rays in their passage through the eye is sufficient to account for their invisibility. That they are strongly absorbed has been found by all observers, but Herr E. Aschkinass proves, in the last number of *Wiedemann's Annalen*, that there is no sudden increase of absorption beyond the red end of the spectrum, and that the absorptive powers of the various media of the eye are practically the same as that of water. The apparatus used for this investigation contained a fluor-spar prism and a bolometer. Thin layers of the humours of an ox eye and a human eye were interposed in the path of the rays from a zircon burner, and the absorptive effects noted by means of the bolometer. It was found that at a wave-length of 0·81 μ, the limit of the visible spectrum, the absorption was 5 per cent. for the whole human eye. This increased to 10·5 per cent. at 0·872, reached 60 per cent at 0·98, decreased to 34·5 per cent. at 1·095, and finally reached 100 per cent., or total absorption, at 1·4 μ. This shows that a large proportion of infra-red light does reach the retina through the eye, but is not capable of affecting the nerves and producing visual sensation.

The last number of the *Wissenschaftliche Beihefte zum Deutschen Kolonialblatte* (Bd. viii. Ht. 2) is a further illustration of the care with which the German Colonial Society is organising the scientific investigation of German Africa. The present number contains the calculations by Dr. Fritz Cohen, Dr. L. Ambronn, and Dr. W. Brix, of the astronomical observations made by Dr. Grüner in Togo-land, and by Ramsay and Stuhlmann in German East Africa. There are also valuable tables of meteorological observations, made in German South-West Africa, and in Kondeland, and from the Marshall Islands; from the last locality comes an especially useful table showing the diurnal variation in atmospheric pressure, and giving the mean reading for every hour for each month in the year. Preuss contributesa report on the geography of the Smaller Cameroons, and Steinberg one on the diseases of the natives of the Marshall Islands. There is also a detailed study, by Dr. O. Warburg, of a beetle (*Herpetophygas fasciatus*) parasitic on the coffee trees in German East Africa. A good plate shows the insect in its various stages, and also illustrates its ravages on the trees.

Herr Oscar Neumann has published a preliminary account of his recent important expedition across Masai-land to Uganda in the last number of the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* (Bd. xxii. No 4–5). Herr Neumann went out to East Africa in November, 1892, and after spending some months in preparation there, left for the interior on April 27, 1893. The caravan, consisting of 135 men, started from Tanga, and passing the southern border of the Usambara country, crossed Nguru to Irangi. Here a series of accidents, exhaustion of supplies, and some severe fighting with the natives, during which Herr Neumann was wounded in the mouth by an arrow, compelled the expedition to retreat southward to Mpwapwa. After resting there the party went northward across Irangi to the Gurui Mountain. This was ascended, though with considerable difficulty. Upon the higher slopes an interesting series of Alpine plants were found, including Azalea and Rhododendron. No trace of a crater remains near the summit, but some small craters occur in an adjoining valley. From Gurui the expedition followed up the East African Rift Valley, along Baumann's route past the salt lakes of Manyara and Natron. He examined the volcanoes Doenyo Kavinjiro and D. Ngai; on the latter he found a steam vent below the summit. From this point he followed Fischer's track past Nguruman to the south-west of the volcano of Suswa. Thence he turned westward to the shore of the Victoria Nyanza in Kavirondo, where the expedition again had great difficulties with the natives. Marching round the Nyanza through Usoga, he reached Uganda, but the excessive caution of an English officer prevented his reaching Mt. Elgon. From Uganda, which he describes as unhealthy and poorer than Usoga, he returned along the English road, across Mau, and past Naivasha and Machakos to Kibwezi, whence he diverged to Taveta, and Kilima Njaro, and thus back to the coast at Mombasa, where he arrived on February 5, 1895. The zoological collections made are very extensive, including 600 species of birds, 90 species of reptiles and amphibia, 50 species of mollusca, and about 1000 species of insects, and 90 species of mammals, of which five have been described as new by Matschie.

Dr. Otto Kuntze has recently issued, under the title of "Geogenetische Beitrage" (Leipzig, 1895, 78 pp.), a series of papers dealing with various geological problems, on which his journeys have thrown light. The first paper gives the evidence for some oscillations of level in the Andes, based on the inclination of some beds of iron-stained sands and laterite, and on the distribution of plants. He states the evidence with care, as it shows that the alterations of level have occurred quietly and without any sudden catastrophic changes. A second paper discusses the evidence on which it is claimed that there was a glaciation in Carboniferous times. The phenomena, often regarded as a proof of this, is attributed by Dr. Kuntze to wind erosion. He gives a figure showing perched blocks and

rounded rock surfaces in the Sierra de Tandil in the Argentine Republic, which have thus been formed. The third article in the series discusses the organic and chemical theories of the origin of the Chilian deposits of saltpetre. The next subject considered is the method of the silicification of fossil wood: the author readvances his old theory, and replies to the criticisms made by Rothpletz and Solms-Laubach upon it, and advances nine arguments against Solms-Laubach's rival theory. The fifth paper describes cases in which deposits of salt have been formed under continental instead of marine conditions, which the author explains as due to the decomposition of minerals containing chlorine in rocks destroyed by subaerial denudation. The last and longest paper in the collection, rediscusses the old problem of the formation of coal. He considers the three alternative theories as to whether coal is allochthon, *i.e.* formed from vegetable material deposited elsewhere than on its place of origin; or is autochthon, or formed by the decay of plants *in situ*; or is pelagochthon, *i.e.* formed under the sea. The author advocates the last. He gets over the difficulty of Stigmaria, by declaring that his fellow botanists are wrong, and that its supposed rootlets are really floating leaves. He says that the figures, given in the text-books, are all copied from one source, and declares that there are no specimens in the museums of "Dresden, Vienna, London, Paris, Berlin, &c.," which give any support to the rootlet theory. He gives an ideal view of a landscape in the Carboniferous period, showing the Stigmaria spreading over the floor of a sheet of water, with the "rootlets" rising as aquatic leaves.

MR. JAMES R. GREGORY, the mineralogist and dealer, wishes it to be known that he has removed from 88 Charlotte Street, Fitzroy Square, to more convenient premises at 1 Kelso Place, Kensington, W.

MESSRS. CHAPMAN AND HALL have been constituted sole agents in this country, the continent, and the colonies, for the sale of the important scientific and technological publications of Messrs. Wiley and Sons, of New York.

THE August *Journal* of the Anthropological Institute contains papers on Prehistoric remains in Cornwall; the northern settlements of the West Saxons; changes in the proportions of the human body during the period of growth; the languages spoken in Madagascar; and on a collection of crania of Esquimaux. There is also a description, by Mr. M. V. Portman, of the methods that should be employed by anthropological photographers.

WE have received a copy of a "Report on Slavery and the Slave Trade in Zanzibar, Pemba, and the Mainland of the British Protectorates of East Africa," by the Special Commissioner of the British and Foreign Anti Slavery Society. The Commissioner spent pretty nearly six weeks in East Africa in studying the subject. Probably the most valuable and trustworthy conclusion in this report, though perhaps not the one to which [illegible] attaches most importance, is that "the whole question of slavery in Zanzibar and Pemba is a very complicated question."

THE volume of *Transactions and Proceedings* of the New Zealand Institute for the year 1894, has reached us. A few of the papers have already been noted in these columns, and as more than seventy papers are included in the volume now [illegible], it is only possible for us to refer to a few of them. A [illegible] list of Coccids, reported from Australia and the [illegible] up to the end of last year, is given by Mr. W. M. Maskell. Sir W. L. Buller, K.C.M.G., F.R.S., has several [illegible] papers in the volume, and Captain F. W. Hutton, F.R.S., adds to our knowledge of the axial skeleton in the Dinornithidae, and there are a number of other papers referring to the same [illegible]. Prof. Arthur Dendy describes some land planarians, bringing the total number of species found in New Zealand up to twenty. The editor of the volume, Sir James Hector, K.C.M.G., F.R.S., contributes several papers to it, and the Rev. W. Colenso, F.R.S., with others, make contributions to the knowledge of the botany of New Zealand.

AMONG the new editions lately received is a translation of Prof. Oscar Hertwig's book "Die Zelle und Die Gewebe," published by Messrs. Swan Sonnenschein and Co. The work has been translated by M. Campbell, and edited by Dr. H. Johnstone Campbell. As we reviewed the original edition in 1893 (vol. xlvii. p. 314), it is only necessary to express satisfaction that such an important treatise on the functions and structure of cells has been brought within the reach of students who do not read German easily. Under this translation from the German, we find on our table two translations into German of papers by British men of science. The papers are published by W. Engelmann in Ostwald's *Klassiker der Exakten Wissenschaften*. No. 61 of this series contains George Green's essay on the mathematical analysis of the theories of electricity and magnetism, edited by Dr. A. J. von Oettingen and Prof. A. Wangerin, and No. 62 is a translation of papers on the physiology of plants, published by Thomas Andrew Knight between 1803 and 1812. This is edited by Prof. H. Ambronn. A third volume (No. 60), just received in the same series, contains papers by Jacob Steiner on geometrical construction, and is edited by Dr. Oettingen. In the Aide-mémoire Series, published by Gauthier-Villars, we have received two books on ballistic subjects, viz. "Balistique Extérieure," by M. E. Vallier, and "Bouches à Feu," by Lieut.-Colonel E. Hennebert. We have also before us "An Elementary Text-book of Mechanics," by Mr. W. Briggs, and Mr. G. H. Bryan, F.R.S., published in the Tutorial Series of the University Correspondence College. The volume is concisely and clearly written, and may be recommended as a useful text-book.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. R. Norton Stevens; a Yellow Baboon (*Cynocephalus babouin*, ♀) from Parrapatti, Eastern Coast of Africa, presented by Mr. J. V. Williams; a Kinkajou (*Cercoleptes caudivolvulus*, ♀) from Demerara, presented by Mr. Sydney Matthews; three Alligators (*Alligator mississippiensis*) from Florida, presented by Mr. Ernest H. Shackleton; two Green Turtles (*Chelone viridis*) from Ascension, presented by Commander Duncan Campbell; a Common Viper (*Vipera berus*), British, presented by Mr. A. Old; a Macaque Monkey (*Macacus cynomolgus*) from Java, a Lion (*Felis leo*, ♂) from India, a Sooty Phalanger (*Phalangista fuliginosa*, var.) from Tasmania, a Larger Hill Mynah (*Gracula intermedia*) from Northern India, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, a Derbian Sternothere (*Sternotherus derbianus*) from West Africa, three South American Rat Snakes (*Spilotes variabilis*) from South America, deposited; a Blossom-headed Parrakeet (*Palaeornis cyanocephalus*) from India, a Tuberculated Iguana (*Iguana tuberculata*) from the West Indies, purchased; three Pumas (*Felis concolor*), eight Black Salamanders (*Salamandra atra*), born in the Gardens, two Triangular-spotted Pigeons (*Columba guinea*), two Crested Pigeons (*Ocyphaps lophotes*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE ROTATION OF VENUS.—Since our note last week on this subject, Signor G. Schiaparelli has published in the *Astronomische Nachrichten* (No. 3304) two letters concerning markings observed on the planet in July, and their bearing upon the question of rotation. The planet was observed from July 3 to 8 under all kinds of conditions, and the conclusion arrived at was that "the aspect of the dusky markings distributed over the disc did not undergo any important modification in form or situation during this period. Save a few small exceptions, all the variations

observed belong to the category of those apparent changes of diurnal period which may be explained by the varying state of rest and purity of the atmosphere and the different grades of illumination of the background of the sky. I have sought with particular care for an indication of any change of place of the most pronounced markings, with reference to the horns and with reference to the terminator; but I have not been able to make out anything with certainty which would indicate a more rapid rotation." This letter is accompanied by a sketch showing a marking diverging from the north pole, and another and more decided one proceeding from the terminator near the south pole in an easterly direction, and then curving round towards the north in the direction of a meridian. This marking underwent some slight change between July 5 and 7, the marking along the meridian only appearing on the latter date. In conclusion, Schiaparelli observes that the period of 224·7 days appears to be placed beyond all reasonable doubt. The second letter, written after the publication of Brenner's observations, contains a detailed description of Brenner's great marking, which is identical with the marking just described, and also with that observed in December 1877. Schiaparelli remarks that the view, advanced by him in "Considerazioni sul moto rotatorio del Pianete Venere," that the markings, though in reality atmospheric phenomena, depend to a certain extent upon the topographical conditions underneath, and recur under the same conditions, appears to be confirmed.

THE OBSERVATORY OF YALE UNIVERSITY.—The report of Dr. W. L. Elkin on the work done and in hand at the Observatory of Yale University, has been received. From the report we learn that the series of heliometer measures on the parallaxes of the first magnitude stars has been brought to a close, and the definitive results will be presented in the near future. The series on the parallaxes of the larger proper motion stars, on which Dr. Chase has been mainly engaged, now comprises 99 stars, all but two of which have been observed at two parallax maximum epochs, in general on three nights. Before drawing any conclusions from these data, it is proposed to secure two further epochs for each star, in order to eliminate the effect of the proper motion. An arrangement has been made with Dr. Gill that the observations and discussion of the *Iris* series for the determination of the solar parallax should be printed and included with his similar investigations on *Victoria* and *Sappho*. For the photography of meteor trails, an equatorial mounting, to carry a number of cameras, has been constructed and set up. The mounting carries four cameras, two with lenses of about 6 inches, and two with lenses of about 5 inches effective aperture. Some valuable results will, undoubtedly, be obtained from the photographic data accumulated by instruments of this kind. Already the Yale Observatory is in possession of some twelve impressions of Perseid trails, four of which were secured there and two at Ansonia by Mr. John E. Lewis, working in collaboration with Dr. Elkin. Prof. Barnard has sent three plates exposed also on August 9, 10, and 11, 1894, for about 8 hours in all, which show four and possibly five meteor trails. And Prof. Pickering has found on an examination of the Harvard Observatory plates one fine trail on a plate taken August 8, 1893, and sent it to Yale for measurement. Dr. Elkin has carried out a discussion of these trails, which will be very shortly ready for publication, and seems to lead to some interesting conclusions.

THE NEBULA N.G.C. 2438.—The first of a new series of celestial photographs, taken by Dr. Isaac Roberts, appears in the current number of *Knowledge*. It is a photograph of a portion of the constellation Argo, and shows the beautiful cluster M 46, and the involved nebula N.G.C. 2438. The nebula is a very small one, and was classed as a planetary nebula by Sir John Herschel; Lord Rosse, however, on some occasions, observed it as an annular nebula with two stars and a suspected third one enclosed; Lassell described it as a planetary nebula with two stars involved. The photograph, which was exposed for 90 minutes in the 20-inch reflector, shows the nebula to be as perfectly of the annular type as that in Lyra. It is circular in form, with three stars in the interior, the ring being most condensed on the north following side. The brightest star is near the centre, and is estimated at from 13th to 14th magnitude; on the south preceding side is a star of about 16th magnitude, and a still fainter one almost touches the ring on the south preceding side. There are indications of faint luminosity in the interior of the ring.

The cluster depicted in the same photograph is a "magnificent aggregation of stars between the 9th and 16th magnitude."

THE VOYAGE OF THE "ANTARCTIC" TO VICTORIA LAND.[1]

ALLOW me first to explain that my scientific observations were made under the disadvantageous circumstances of a sailor before the mast on board the whaler *Antarctic*. There seemed no choice between adopting this course and remaining on shore, and I was consequently able to take very few instruments. This explanation may to some extent lighten the criticism of my results.

We left Melbourne on September 20, 1894. It was originally our intention to spend a few weeks in search of sperm whales off the south-west of Tasmania; but not meeting with any, we steered for Royal Company Islands. On October 18 we had snow on board for the first time. It came in heavy squalls, bringing a large specimen of the *Diomeda exulans* albatross on board for refuge. At night it was moonlight, and at twelve o'clock the Aurora Australis was visible for the first time, with white shining clouds, rolling from west to east, at an altitude above the southern horizon of thirty-five degrees. The *Antarctic* was at the time in the vicinity of Macquarie Island, in latitude about 50° south. The aurora seemed to be constantly reinforced from the west, the intensity of the light culminating every five minutes, dying out suddenly, and regaining its former brilliancy during the succeeding five minutes. The phenomenon lasted until two o'clock, when it was gradually lost in an increasing mist. As the snow was heavy, and there was little probability of any material benefit from landing, we set out for Campbell Island on the 22nd, and dropped anchor in North Harbour on the eve of October 25, drifting the following day down to Perseverance Bay, a much safer harbour, where we filled our water-tanks and made final preparations before proceeding south. Campbell Island shows from a great distance its volcanic origin and character, undulating ridges rising in numberless conical peaks to from 300 to 2000 feet above sea level. The land around the bay is rich in vegetation, and most of the island is covered with grass, on which a few sheep seem to live in luxury. Numerous fur seals were basking on the rocks, and we also found many sea-leopards (*Stenorhynchus leptonyx*). They seemed to thrive well, their skins being without scar or cut, and, except human beings, they appear to have no enemies in these waters.

While duck-shooting on the Campbell, I came on three graceful waders of the snipe type (*Novæ Zealandiæ*). In the interior of the island grass was everywhere to be seen except where stunted brushwood covered the ground. I have no doubt that some of the hardy species of Scandinavian trees would do well on this island.

We weighed anchor on October 31. During the next few days, proceeding further into the fifties, the air and water remained at an equal temperature of 44° F. A large number of crested penguins were seen jumping about like small porpoises. We met with several icebergs from 100 feet to 150 feet in height. These bergs were solid masses of floating ice, with perpendicular walls and an unbroken plateau on the top.

On the 6th of the following month, in lat. 58° 14′ and long. 102° 35′, we sighted an immense barrier of ice, or chain of icebergs, extending for about forty to sixty miles from east to north-west, in fact as far as the eye could reach, the top being quite level and absolutely white, and the greatest height 600 feet. The perpendicular sides were dark ashy grey, with large worn green caves. Several icebergs, similar to those we had encountered before, were floating in all directions, and were undoubtedly children of this enormous monster.

By the time we had reached 55° the albatross had left us, as likewise the Cape pigeon (*Daption capensis*); but the white-bellied storm petrel still followed in our track. A lestris, with dark brown head and white bordered wings, and a small blue petrel put in an appearance. On December 7 we sighted the edge of the pack ice and shot our first seal, which was of the white kind (*Stenorhynchus carcinophaga*), its skin being injured by several deep scratches. We had also a very heavy snowfall, the vessel being covered on deck and rigging for the first time.

On December 8, in lat. 68° 45′, long. 171° 30′, large streams of ice drifting around us, a strong ice blink appearing towards the south, and the presence of the elegant white petrel (*Procellari Nivea*) gave us unmistakable evidence that we had now before us those vast ice-fields into which Sir James Ross successfully entered with his famous ships *Erebus* and *Terror*, on January 5, 1841. In the evening we slowly worked our way in

[1] Abstract of a paper read by Mr. C. E. Borchgrevink at the Sixth International Geographical Congress on August 1.

through the outer edge of the ice-pack, which consisted of large and heavy hummocky ice. I saw multitudes of the *Argonauta Antarctica* everywhere in the pack, usually swimming in cavities in the ice-floes to escape their enemies the whales. The large-finned whale (*Physalus Australis*) was spouting about in all directions. The white petrels were numerous here, and I secured more of them. The white-bellied petrel departed at the edge of the pack, leaving the icy regions to its darker, hardier brethren (*Thalassidroma Wilsonii*). We shot several seals, but they were scattered about sparsely, most having scars and scratches in the skin. Sir James Ross noticed similar wounds on the seals, and it has been supposed that they are inflicted by the large tusks in battle between themselves. My opinion, however, is that these scars must be ascribed to the action of a different species. The wounds are not like those inflicted by a tusk, being from two to twenty inches in length, and straight and narrow in shape, and where several are met with on the same animal, they are too far apart to have been produced by the numerous sharp teeth of the seal. Nor do I consider that they are due to the sword-fish, though that is doubtless doing mischief there. If my opinion, that these wounds are inflicted by an at present unknown enemy of the seal, proves correct, it may serve to explain the strange scarcity of these animals in regions where one would expect to find them almost everywhere.

When we entered the ice-pack the temperature of the air was 25° F., that of the water 28° F., which latter temperature was maintained all through the pack. Penguins were about in great numbers.

On the 14th we sighted Balleny Island, finding it in lat. 66° 44′, long. 164°: this agreeing with Ross. The ice-floes became gradually larger as we approached land, and it was evident that the ice-pack then around us was in great part discharged from the glaciers of Balleny, some of it carrying stone and earth. Although the higher part of the island was lost in mist, we got a good view of its lofty peak, which rises to a height of 12,000 feet above the sea level. The size and shape of the ice about Balleny was a source of considerable danger to our vessel, covered as it is with snow to a depth of several yards, and running out under water in long sharp points. It is not likely that a vessel depending entirely on sails would long survive in such ice. The air temperature at Balleny was found to be 34° F., that of the water 28° F.

Finding the pack so impenetrable in this locality we resolved to work eastwards, in the track which the *Erebus* and *Terror* had followed. On December 22, in lat. 66° 3′, long. 167° 37′ E., I shot a seal of ordinary size and colour, but with a very thick neck, and no sign of scars, a kind which none of our old sealers on board had ever seen before.

On Wednesday, the 26th, we crossed the Antarctic circle, and on New Year's Eve were in lat. 66° 47′, long. 174 8′ E. at twelve o'clock. In lat. 67° 5′, long. 175° 45′ E., I secured a specimen of *Aptenodytes Forsterii* a large penguin. I only secured four of these birds altogether, and never saw it in company with others of its kind. On the 14th, in lat. 69° 55′ and 157° 30′ E., we came again into open water, having spent 38 days in working our passage through the ice-pack. A clear open space of water was now before us. We steered straight for Cape Adare on Victoria Land, and sighted it on the 16th of January. On the 18th, in lat. 71° 45′, long. 176° 3′ E., the temperature of the air was 32°, and of the water 30°. The cape, which is in 71° 23′ and 169° 56′ E., rises to a height of 3779 feet, and consists of a large square basaltic rock with perpendicular sides. From there we saw the coast of Victoria Land to the west and south as far as the eye could reach, rising from dark bare rocks into peaks of perpetual ice and snow 12,000 feet above the sea level, with Mount Sabine standing out above the rest. I counted as many as twenty glaciers in the immediate vicinity of the bay, one of which seemed covered with lava, while below a layer of snow appeared another layer of lava, resting on the surface of the glacier. A volcanic peak about 8000 feet in height had undoubtedly been in activity a short time before. On the 18th we sighted Possession Island, and effected a successful landing on the North Island, being the second to set foot on this island, Sir James Ross having preceded us fifty-four years before. The island consists of vesicular lava, rising in the south west into two pointed peaks 300 feet high. I scaled the highest of these, and called it Peak Archer, after A. Archer, of Rockhampton, Queensland. To the west the island rises gently upward, forming a bold and conspicuous cape, to which, not having been christened by Ross, I gave the name of Sir Ferdinand von Mueller. I quite unexpectedly found vegetation on the rocks about 30 feet above the sea level, vegetation having never been discovered in so southerly a latitude before. We gave to this island, which I judged to be about 300 to 350 acres in extent, the name of Sir James Ross Island. Possession Island is situated in lat. 71° 56′, long. 171 10′ E.

On January 20 we steamed southwards, and on the 21st sighted Colman Island at midnight. Finding the eastern cape of this island unnamed, we called it Cape Oscar, in honour of his Majesty our King. I noticed great irregularities in our compass at Colman Island, and undoubtedly it contains secrets of scientific value. On the 22nd, being in lat. 74° S., and no whales appearing, it was decided to head northwards again, although all regretted that circumstances did not permit of our proceeding further south.

On the 23rd we were again at Cape Adare. Icebergs of large size were everywhere to be seen, and showed distinctly whether they were broken from the big barrier or discharged from the glaciers on Victoria Land. We landed at Cape Adare that night, being the first human beings to put foot on the mainland. Our landing-place was a kind of peninsula or landslip, gently sloping down from the steep rocks of Cape Adare until it ran into the bay as a long flat pebbly beach. The peninsula formed a complete breakwater for the inner bay. The penguins were, if possible, even more numerous here than on Possession Island, and were found in the cape as far up as 1000 feet. Having collected specimens of the rocks, and found the same cryptogamic vegetation as on Possession Island, we again pulled on board. We now stood northward, and in lat. 69° 52′, long. 169° 6′ E., again ran into the ice-pack. On February 1, in lat. 66°, long. 179° 31′ E., we reached open water, having this time spent only six days in the ice-pack. On the 17th the Aurora appeared stronger than I ever saw the Aurora Borealis. It rose from south-west in a broad stream towards the zenith, and down again towards the eastern horizon, being quite different in appearance from when we last saw it on October 20. It presented long shining curtains rising and falling in wonderful shapes and shades, sometimes seemingly quite close to our mastheads, and it evidently exerted considerable influence upon our compass-needle. In lat. 44° 35′ and long. 147° 34′ we met with a great number of sperm whales. After struggling for several days with a furious storm of distinctly cyclonic character, we sighted the coast of Tasmania on March 4, and entered Port Philip on the 12th, five months and a half after our departure from Melbourne.

As my report shows, we had comparatively high temperatures during our voyage, higher than Sir James Ross experienced, and higher than those observed last year by the whaling fleet south of Cape Horn. The minimum temperature we experienced within the Antarctic circle was 25° F., the maximum 46°. The average temperature from 200 readings each month was 32°.5 for January, 30° for February. The temperature of the water remained at 28° F. all through the ice-pack, rising 1° wherever a larger sheet of water broke the ice-fields. In the large bay in South Victoria Land the temperature remained nearly constantly about freezing-point. The question naturally arises: Has the average temperature at the shore of Victoria Land risen during the last fifty-four years, and has vegetation for the first time developed in those southern latitudes since Ross was there? It does not seem probable that the appearance of vegetation on Possession Island would have escaped the observation of the naturalists who accompanied that expedition. It is evident that a warm current with a constant direction northwards breaks the ice-fields at the very place where Sir James Ross and we penetrated to the open bay of Victoria Land. Within the Antarctic circle the barometer at 29 inches always indicated calm, clear weather, and even at 28 inches it remained fine. This low barometric reading is remarkable considering the dryness of the air. The prevailing wind in the bay seemed to be from the east, but at Cape Adare a change appeared to take place, and westerly winds are there, to all appearance, predominant. The direction of the movement of the ice is distinctly north-easterly, and the scarcity of ice in the bay of Victoria Land is undoubtedly not alone due to warm currents, but also to the protection from drift ice afforded by the shore from Cape Adare down to the volcanoes Erebus and Terror.

The rocks on Possession Island seem all to be of volcanic origin, and represent basaltic lava flows which have taken place during late geological epochs. The specimens I brought from South Victoria Continent differ but little from those I found on Possession Island. One peculiar rock I collected has an indistinct granular structure, and resembles much the garnet sand-

stone of Broken Hill; it seems to bear some close relation to granitite. The specimen is composed of quartz, garnet, and felspar fragments. This rock holds out hopes that minerals of economic value may occur in these regions.

The peninsula on which we landed at Cape Adare must be some seventy acres in extent: on the top of the guano were lying the primitive nests of the penguins, composed of pebbles. Some hundreds of yards up these landslips I came upon two dead seals, which from their appearance must have lain there several years. I made a thorough investigation of the landing-place, because I believe it to be a place where a future scientific expedition might safely stop even during the winter months. Several accessible spurs lead up from the place where we were to the top of the cape, and from there a gentle slope leads on to the great plateau of South Victoria Continent. The presence of the penguin colony, their undisturbed old nests, the appearance of the dead seals, the vegetation on the rocks, and, lastly, the flat table of the cape above, all indicated that here the unbound forces of the Antarctic circle do not display the whole severity of their powers. Neither ice nor volcanoes seemed to have raged at the peninsula at Cape Adare, and I strongly recommend a future scientific expedition to choose this spot as a centre for operations. At this place there is a safe situation for houses, tents, and provisions. I myself am willing to be the leader of a party, to be landed either on the pack or on the mainland near Colman Island, with Ski, Canadian shoes, sledges, and dogs. From there it is my scheme to work towards the south magnetic pole, calculated by Ross to be in 75° 5′ and 150° E., Colman Island lying in 73° 36′ S. and 170° 2′ E. I should have to travel about 160 miles to reach the south magnetic pole. Should the party succeed in penetrating so far into the continent, the course should be laid, if possible, for Cape Adare, in order to join the main body of the expedition there.

As to the zoological results of future researches, I expect great discoveries. I base my expectations on one point—on the scars found on the seals, which in my opinion point to the existence of a large unknown mammal within the Antarctic circle. Although the white polar bear of the Arctic has never been found in the south, I should not be surprised to discover similar species there. It would indeed be remarkable if, on the unexplored Victoria Continent—which probably extends over an area of 8,000,000 square miles, or about twice the size of Europe—animal life hitherto unknown on the southern hemisphere should not be found.

It is of course possible that the unknown land around the axis of rotation may consist of islands, only joined by perpetual ice and snow; but the appearance of the land, and the colour of the water with its soundings, in addition to the movements of the Antarctic ice, point to the existence of a mass of land much more extensive than a mere island.

It is true that the scientific results of this expedition have been few, but my little work gives me at least the satisfaction of feeling that it will fill a useful, if molecular, place among those strong arguments which for years have accumulated, and which prove that further delay of a scientific expedition to South Victoria Continent can scarcely be justified.

WEATHER FALLACIES.[1]

IN the long and patient pursuit which the attainment of all accurate knowledge exacts from man, it may sometimes be instructive to turn one's gaze backward and contemplate the errors which have been corrected, the fallacies which have been demolished, and the superstitions which have been lived down; and this consideration has prompted me to take for the subject of this year's address that wide range of human opinions which may fitly be classed under the head of "Weather Fallacies."

Nothing could have been more in accordance with the law of growth in other branches of knowledge than that Meteorology should, in its earliest dawn, have been with difficulty able to emerge from the mists and darkness of guesses and surmises such as have surrounded the transfer of any truth from the barbaric to the philosophic stage.

It is to the Greeks that we must look for the first real weather observations after the matter had passed through what may be called the mere savage phase: and we find Hesiod, Theophrastus, and Aratus presenting us with an early code of rules, which serve at least to show us how little we have ourselves advanced in some matters since their time.

[1] An address delivered to the Royal Meteorological Society, by Mr. Richard Inwards, President. (Reprinted from the *Quarterly Journal* of the Society.)

One of our Fellows, Mr. J. G. Wood, has just given to the world an excellent and scholarly translation of the work of Theophrastus, which has not previously been put in an English garb, and Mr. Wood has done the whole country a great service in giving us this translation of the "Winds and Weather Signs," a book which contains a host of rules and observations about the weather, and which, as might have been expected from the production of the favourite pupil of Plato and Aristotle, is singularly free from errors of the grosser and more superstitious kind, such as were plentifully produced in Western Europe many centuries later.

But long before the time of Theophrastus, and probably very soon after the invention of agriculture itself, there were weather gods and weather fallacies; for we find that Jupiter Tonans and Pluvius—the thunderer and the rain-maker—were set by men on the highest pedestals. And centuries after this, Lucian tell us that it was usual in his time to offer prayers for suitable weather, and he recounts in his "Dialogues" how two countrymen were at the same time offering up contrary petitions—one that not a drop of rain might fall until he had completed his harvest, while the other prayed for immediate rain, in order to bring on his backward crop of cabbages—both suppliants only too sure to find that the ears of the image were deaf as the stone of which they were made, and that the wheels of the universe would not wander or turn back for any selfish ends of man.

In considering these early times, when the weather had to be studied from cloud, sky and sea, and from the behaviour of the animals and plants, we must be ready to excuse men for doing that which is still too frequently a cause of error, viz. foretelling what they most wished for, and putting down as universal law that which was only a coincidence of totally independent events. In considering weather fallacies it will be impossible to follow a chronological order, so I shall treat them, or rather a small portion of them, under the heads of saints' day fallacies, sun and moon fallacies, and those concerning animals and plants, while finally I shall consider the almanack makers, weather prophets and impostors who have from time to time furnished the world with materials for its credulity or its ridicule.

The first class of weather fallacies which I shall scarcely more than mention, are those which refer to the supposed connection between the weather of any day in the week or year, and that of any other period, and it may be as well to state at the outset that there is no kind of foundation in fact for any of these so-called rules. They are for the most part born of the wish to see certain kinds of weather at certain times of year, and, like all these predictions, were faithfully remembered when they came true, and promptly forgotten when they failed. One has often heard—

"Fine on Friday, fine on Sunday."

Or that "Friday is the best and worst day of the week," and the superstition even extends to hours of the day, for we have

"Rain at seven, fine at eleven,"

which is only another way of saying that rain does not usually last four hours, and the rule generally fails when applied to daily experience; but the host of proverbs connected with saints' days are more difficult to deal with, on account of the longer time which elapses between the prophecy and its fulfilment or failure. All or most of these proverbs concern the days of certain saints, though I think no one imagines that this is anything more than a convenient method of fixing the date, because our ancestors had a saint for every day, so that they naturally referred to the day by his name.

There are forty weather saints, among the most prominent of whom is undoubtedly St. Swithin, whose day is July 15, and the superstition is that if it should rain on that day it will rain for forty days after. Now, as Mr. Scott observes, this date is very near a well-known bad time in wet years, as the terms, long in use, of "St. Margaret's flood" and "Lammas flood" abundantly testify. The fact that some of these heavy rains began on July 15 has been enough material for the adage-monger, and so we have another "universal" law laid down, a law which is, however, constantly broken, as every student of the weather very well knows. The whole thing is a fallacy of the most vulgar kind, and ought speedily to be forgotten, together with all the adages which make the weather of any period depend on that of a distant day.

Turning in weariness from this class of superstitions, which may be said to belong to the self-exploding order, we are next met by an extensive array of authorities who, under the protecting

shield of astronomy, profess to have framed infallible rules for the weather as judged from the ever-varying relative positions of the sun, moon, and planets. They attack us systematically and persistently, appealing to analogy, to reason, and to common sense. But it is sometimes necessary to be on our guard, even against common sense, in considering problems to which uncommon sense has for centuries been devoted without avail. The well-known action of the sun and moon upon the ocean tides is generally the starting point of these theorists, and it is soon shown to common sense that when the earth is nearer the sun, or the moon is nearer to the earth (it being remembered that they move in elliptic orbits), or when both sun and moon are, as it were, pulling together, as at new moon, there ought to be a tide of atmosphere caused by their influence similar to the tides of the ocean, which such agencies undoubtedly produce. But we find that whatever so-called reason, analogy, and common sense may seem to dictate, the facts will not follow in the path marked out for them; and the atmospheric tides refuse to ebb and flow, except in a most infinitesimal degree, quite disproportioned to their supposed moving forces. The theorists must try again, and they do so by pointing out that the moon and earth move in planes which are inclined to each other at an angle, and that at some times of the year the attraction of the sun and moon are acting in somewhat widely diverging lines, whilst at others they are pulling more nearly in the same plane. Here is, they say, a clear case. At times, when the angle is greatest, there should at any rate be worse weather caused by the conflicting forces. When the moon is said to be "on her back," or, in other words, when the line of the shadow boundary of the half-moon or crescent is much inclined to the earth's axis, then is the time, say they, for tempests and commotions to come. But again the spirits from the "vasty deep" do *not* come when called, and we have to invent other causes for our earthly disturbances.

It may be safely said that a new moon theory as to the weather comes out at least once a year, and it has been attempted to connect the honoured name of Sir William Herschel with a table which professed to show the dependence of weather changes on those of the moon.

By the kindness of Mr. Symons I am able to show you on the screen a much magnified representation of this production, now very scarce, and which has the name of Herschel in large capitals, no doubt as a sort of ballast to give it weight and steadiness, though it does not definitely state that Herschel had anything to do with it. Herschel's own letter on the subject runs as follows:—

Sir

I am glad of an opportunity to say that prognostications of the weather are so much above the knowledge of astronomers that I have taken uncommon pains publicly to contradict reports of predictions that have been ascribed to me. You may therefore be assured that what you have heard of my opinion about the frost is without the smallest foundation. If at any time Slough should be in your road, I shall be very glad to see you here, and remain

Sir

your most obed^t
Serv^t

Wm Herschel

Slough
near Windsor
Feb 6 1814

So that any fellow of the Society who sees one of these diagrams in the future will know it as a fraud.

Of course it is in the power of every one to check the predictions which are so often made with respect to the changes of the weather taking place on the change of the moon; but many eminent men have occupied themselves with the subject, and the result is that no correspondence between the two classes of phenomena has been established.

Dr. Horsley examined the weather tables of 1774, as published by the Royal Society, and out of 46 changes of weather in that year only ten occurred on the days of lunar influence, only two of them being at the new moon, and none at all at the full. M. Flarguergues, of Viviers, found also as the result of twenty years' observations, that the barometer readings taken when the moon was furthest from the earth averaged 755 millimetres, and when nearest, 754 millimetres, showing a difference of 1 millimetre or about ·04 inch, and this in a direction against the theory, the pressure being greater by that amount when the moon was farthest from the earth.

Various other weather seers have pinned their faith on lunar cycles, and have predicted that weather changes would repeat themselves, as soon as the sun and moon got back into the same relative positions, which they do in nineteen years, with only an error of an hour and a half. Others, such as Mr. G. Mackenzie, advocated a cycle of 54 years, but it may be summarily stated that all the cycles have broken down, and that, as far as we know, there is no definite period after which the weather changes repeat themselves.

Other fallacies about the moon are numerous, such as that the full moon clears away the clouds; that you should only sow beans or cut down trees in the wane of the moon; that it is a bad sign if she changes on a Saturday or Sunday; that two full moons in a month will cause a flood; that to see the old moon in the arms of the new brings on rain, and many others, of which a catalogue alone would take up a good deal of space. M. Flammarion says that "the moon's influence on the weather is negligible. The heat reaching us from the moon would only affect our temperature by 12 millionths of a degree; and the atmospheric tides caused by the moon would only affect the barometric pressure a few hundredths of an inch—a quantity far less than the changes which are always taking place from other causes." On the whole we are disposed to agree with the rhyme which thus sums up the subject:

> The moon and the weather
> May change together;
> But change of the moon
> Does not change the weather.

Even the halo round the moon has been discredited, for Mr. Lowe found that it was as often followed by fine weather as by rain, and Messrs. Marriott and Abercromby found that the lunar halo immediately preceded rain in 34 cases out of 61. We always have a lingering hope that some future meteorologist will disentangle the overlapping influences, and arrive some day at a definite proof that our satellite after all has something to do with our weather.

About the sun, also, there are many fallacies, and ever since the discovery that the spots which appear on his surface have a period of greatest and less frequency, there have been theorists in shoals who have sought to prove that this fact rules our weather. It has undoubtedly been found that the frequency of sun-spots and the variations of the magnetic needle are intimately connected; and it is almost equally well established that the aurora appears and disappears in some sort of sympathy with the sun-spot variations. But this, up to the present, is as far as we can get in this direction, for our weather seems to have no definite relation to these changes.

The more recent discoveries of prominences visible round the disc of the sun during an eclipse, and of the light clouds only seen in M. Deslandres' spectro-photographs, will no doubt call out new weather theories on the subject. And I must confess to a wish that those mysterious flame-like bodies rushing from the sun millions of miles into space, will be found to have some influence on the upper layers of our earth's atmosphere; but I also hope that we may be saved from a theory on the subject until more facts are before us.

Coming down to earth again, we are met by a long array of fallacies connected with the behaviour of animals and plants, and which have a supposed connection with weather changes. Few of these are so well grounded that they may be considered as proved, and as nothing is sacred to a meteorologist, our veteran

Fellow, Mr. E. J. Lowe, F.R.S., has endeavoured to put some of the rules from this source to the test of definite observation. He took a number of well-known signs said to indicate change, and carefully noted what happened after each sign, and although he does not say that all indications from animals, birds and plants are useless, yet certainly those he did investigate seemed utterly to break down.

He took the well-known signs of bats flying about in the evening, many toads appearing at sunset, many snails about, fish rising much in lake, bees busy, many locusts, cattle restless, land-rails clamorous, flies and gnats troublesome, many insects, crows congregating and clamorous, spider-webs thickly woven on the grass, spiders hanging on their webs in the evening, and ducks and geese making more than usual noise. Mr. Lowe found that in 361 observations of the above signs, they were followed 213 times by fine, and only 148 times by wet weather; so that even after the prognostications for rain, there was a greater preponderance of fine weather. He called a day fine when no rain was measurable in the rain gauge. Mr. Lowe says that even swallows flying low cannot be depended on, as, especially at the close of summer and autumn, they almost invariably skim the surface of the ground, and Mr. Charles Waterton, the naturalist, decided, after careful observation, that the unusual clamour of rooks forms no trustworthy sign of rain. These must, therefore, swell the list of fallacies, although there are many other rules which have not been so carefully examined, but which may still be true. My own impression is that although it is painful to dismiss the animals from their ancient position as weather prophets, we may consider them as indicating what they feel, rather than as predicting what is to come, and that their actions before rain simply rise from the dampness, darkness or chilliness preceding wet weather, and which render these creatures uneasy, but not more so than they affect man himself. The sheep turning its back to the wind (one of the best known signs of rain) is probably only that it may shelter its least protected part from the effects of the weather; and many of you must have observed sheep sheltering their heads from the heat by getting them into the shade of each other's bodies in a similar way.

As to cows scratching their ears, and goats uttering cries, and many other signs of bad weather, they are at least very doubtful; whilst the adage about the pig which credits him with seeing the wind, carries with it its own condemnation.

The medicinal leech is still left on the list of weather prophets, though he has no doubt had his powers exaggerated; and two books have been written about his behaviour during changes of weather. One is by Mrs. Woollams, who, during a long illness, watched a leech in a bottle, and carefully noted what it did; and the other is by a gentleman at Whitby, who came to the conclusion that the leeches could be made to give audible and useful storm warnings. So he contrived the instrument, of which I now show you a drawing taken from his book. No one would imagine from its appearance what its use could be. It consisted of twelve glass bottles each containing a leech in water, and so arranged in a circle, in order, as the humane inventor states, that the leeches may see each other and not endure the affliction of quite solitary confinement—this rather reminds us of Isaac Walton, who told his pupil to put the hook into the worm "tenderly, as if he loved it"—in each bottle was a metal tube of a particular form, and which was made somewhat difficult for a leech to enter, but into which it would endeavour somehow to creep before a thunderstorm, according to its nature. In each tube was a small piece of whalebone, to which a gilt chain was attached, and so arranged, on the mouse-trap principle, that when the whalebone was moved the bell at the top of the apparatus was rung by means of the chain. There were twelve leeches, so that every chance was given that one at least would sound a storm signal. The author called this apparatus the "Tempest Prognosticator," a name which he preferred—and I think we shall agree with him—to that of atmospheric electric telegraph conducted by animal instinct. He went on to state in his little book that he could, if required, make a small leech ring the great bell of St. Paul in London as a signal of an approaching storm. The book is written in all seriousness, and a number of letters are appended from gentlemen who certify that correct atmospheric indications were at various times given by the leeches. The name of the inventor of this ingenious contrivance was Dr. Merryweather—himself a learned leech.

Plants have also their advocates as weather indicators; and there is no doubt that in most cases they act in sympathy with changes in the dampness, gloominess, or chilliness of the air, and as these conditions generally precede rain, one cannot term the indications altogether fallacious. The pimpernel and the marigold close their petals before rain, because the air is getting damper, while the poplar and maple show the under surface of their leaves for a similar reason. Indeed, an artificial leaf of paper may may be made to do the same thing, if constructed on the same principle as the natural one—a hard thin paper to represent the upper side of the leaf, and a thicker unsized paper for the lower side; these will, if stuck together, curl up or bend down in sympathy with the hygroscopic condition of the air. A slip of ordinary photographic paper will do the same, and will curl up at once when placed on the hand.

The same slackness which moisture produces in plants applies in some degree also to insects, some of which can only fly in sunshine, so that there is a chain of weather signs all following from a little dampness in the air. The flowers close their petals and shut in their honey, the insects cannot fly so high, and the swallows seeking them skim the surface of the earth, and even then the threatened shower may not come.

In 1892 attention was directed to a plant, the *Abrus precatorius*, a beautiful shrub of the mimosa kind, which has the property of being sensitive in a high degree, so that its pinnate leaflets go through many curious movements, and it was claimed that these form a guide of unerring certainty to foreshow the coming weather. Even earthquakes were said to be predicted by this wonderful plant. If it closed its leaflets upward, after the manner of a butterfly about to settle, fair weather was shown; when the leaflets remained flat, changeable and gloomy weather was indicated; while thunder at various distances was to be foretold by the curling of the leaflets, and the nearer the thunder the greater the curl, until when the points of the leaflets crossed, the thunderstorm was indicated as being overhead. Changes of wind, hurricanes, and other phenomena were to be shown by the various curious and beautiful movements of the leaflets and stalks. These movements undoubtedly took place, but when the plant was submitted to the unprejudiced observation of Dr. F. W. Oliver and Mr. F. E. Weiss, at Kew Gardens, those gentleman failed to find any connection between these movements and the weather, and Dr. Oliver made a report on the matter, which hits the heart of the whole subject of plant movements, by ascribing them for the most part to the agency of light and moisture. Mr. Scott, of the Meteorological Office, gave the finishing stroke to the theory by proving that the movements had no connection with either cyclones or with earthquakes, so that the sensitive plant may be considered as out of the list of weather guides, in spite of having been made the subject of an English patent.

It is a most common observation in the country that a large crop of hips, haws, and holly-berries indicates a severe winter to follow, and it is generally pointed out that nature thus provides winter food for the birds. This, too, is a fallacy.

Another weather fallacy, for which artists are responsible, is that flashes of lightning take the form of long angular lines of a zigzag shape, and of which I show you an example, taken from a work on the subject. This, when compared with the next view, which is a photograph taken direct from nature, shows that the artist had very little understood the true form of the lightning flash, which consists of numbers of short curves joining each other, something like the course of a river depicted on a map, or in some degree like the outline of a clump of leafy trees seen against the sky. But, as far as I know, there were only two artists whose acute vision saw lightning in anything like its true form. One was Turner, who long before the time of photography, scratched his lightning flashes with a penknife, making short curved dashes across the picture; and the other was Nasmyth, the astronomer and engineer, who also saw the lightning in its true form, and duly noted the same, only to be confirmed years afterwards, when it became easy to photograph the lightning flash itself. While on the subject of lightning, I may mention that it is recorded that in one case at least a rheumatic man who had been confined to bed six weeks, received a shock from a stroke of lightning, jumped from his bed, and ran down stairs completely cured. This is related in the *Gentleman's Magazine* for June 1820.

It has been often stated that the noise of cannon will produce rain, and it is not unusual in the Austrian Tyrol to hear the church bells ringing to avert thunder. These are fallacies. The experiments in America made recently to test whether rain could be produced by exploding a large quantity of gunpowder in the air, resulted in nothing except noise and smoke, though the thing was well worth trying.

Empedocles of old is credited with the invention for chasing

away the Etesian winds by placing bottles made of the skins of asses on the hills to receive them. Timæus relates this. After hearing this about Empedocles, one is not surprised to learn that he thought there were two suns, that the moon was shaped like a dish, and that the sea was the sweat of the earth burnt by the sun. All this will be found in Stanley's "Lives of the Philosophers."

Almost in our own time, too, a "pluvifuge," or machine for blowing away rain, was proposed in Paris. This, too, was a fallacy.

To give an account of all the various ceremonies in savage and civilised countries which have been resorted to for the purpose of changing the course of the weather, would be here impossible; but such rites have a common origin and a common result. They begin in error, and end in failure. In India, the rain-god is imagined to pour down showers through a sieve: in Peru there was supposed to be a celestial princess, who held a vase of rain, and when her brother struck the pitcher, men heard the shocks in thunder. In Polynesia rain comes from the angry stars, stoning the sun; while in Burmah it is still the custom to haul down rain by pulling at a rope. New Caledonia has its regular rain-making class of priests, and in Moffatt's time the rain-makers of South Africa were held in even higher estimation than the kings; and on the other side of the world the Alaskan propitiates the spirit of the storm by leaving tobacco for him in a cave. In our own country, too, there have been weather witches of various grades, and one described in Drayton's "Moon Calf"

> "Could sell winds to any one that would
> Pay them for money, forcing them to hold
> What time she listed, tie them in a thread
> Which, ever as the seafarer undid
> They rose or scantled as his sails would drive
> To the same port whereat he would arrive.

The Finlanders at one time drove a profitable trade by the sale of winds. *After* being paid, they knitted three magical knots, and told the buyer that when he untied the first he would have a good gale; when the second, a strong wind; and when the third, a severe tempest.[1] Sir Walter Scott also mentions that King Eric, also called "Windy Cap," could change the direction of the wind by merely turning his cap round upon his head; and old Scotch women are mentioned who, for a consideration, would bring the wind from any desired quarter.[2] The Mandan Indian rain-maker had a rattle by the noise of which he called down rain from heaven by the simple process of keeping on long enough. It is safe to say that these are all fallacies.

From the rain makers we may now turn for a moment to the almanack makers, and any one who will look up an old almanack of the early part of the last century, will find the greater part of it filled with lucubrations on the influence of the stars and constellations; he will also find a column giving for every day the parts of the body which are particularly under the celestial influences on the given dates, and when one sees for the first time this column reading—head, chest, legs, knees, feet, &c., one wonders what it can mean; but it was then so well understood, as not even to require explanation, and there was generally too a rude woodcut of a hideous human figure, tattooed with the various signs of the zodiac to show the same thing. The sort of knowledge that passed for meteorology in 1703 may be learned from the following extract from "Meteorologia" by Mr. Cock, Philomathemat. 1703 a rare book in the possession of Mr. Symons.

"The twelve signs are divided into four sorts, for some be earthy, others watery, a third sort aery, and the fourth sort is fiery." The author then goes on to state that "Jupiter in the S[illegible]r (whatever that may be) opposed by Saturn in the Lion did raise mighty South west winds. . . . Observe when a planet is in an earthy sign he was lately dried up by perambulating a fiery sign, and after that, immediately having made his progress in an earthy sign, is quite bound up from moisture."

It seems incredible that our ancestors, only a few generations back, should have bought, paid for, and believed, such stuff as this. The early almanacks boldly gave a prediction for the weather for every day in the year; but after a time confined themselves to a general statement of the weather, for instance "Partridge's Almanack" for 1835 has the following prophecy for June: "Fertilising showers attended with thunder and lightning" [illegible] for the first ten days. "Fair and at times hot" for the middle of the month, and "refreshing rain for the grass and corn" for any time between the 21st and the end of the month.

Authors of weather almanacks had already begun to seek safety in vagueness. Some of these almanacks rose to a great popularity on the strength of one lucky guess; and I think it is told of this same Partridge's almanack, or some other of the class, that it owed its reputation to a curious prophecy of extraordinary weather for July 31, when hail, rain, snow, thunder, &c., were freely indicated. Forgetting that the month had 31 days the almanack maker had omitted to insert the weather prediction for the last day, and a boy was sent from the printing office to know how the space was to be filled up. The weather prophet was too busy to attend to him, but at last in a passion, said: "Put down hail, rain, snow, thunder, anything"; and the boy taking it literally told the compositor, who duly set into type the extraordinary prediction, and which by a freak of nature came true, and made the fame and fortune of the almanack maker. This story, if not true, is at least *ben trovato*, and shows the force of the bard's statement

> "Our indiscretion sometimes serves us well
> When our deep plots do pall."

The *British Almanack* for 1831, published by the Useful Knowledge Society, had no weather predictions.

Patrick Murphy published a popular weather almanack, and his fame is said to have commenced by a lucky hit in one of the earlier issues by which he indicated which would be the coldest day of the year. There is a copy of this almanack for 1838 in the library of the Society, and some former owner has evidently taken the trouble to pencil in the actual weather opposite to that predicted. There were, according to this annotation, 89 incorrect forecasts, 91 doubtful, and the rest correct.

This Patrick Murphy was not a mere charlatan. He had a system, and though he differed from Sir Isaac Newton and the Royal Astronomical Society, he gave much study and research to the subject of meteorology—as shown by his various books. There was an Astro-Meteorological Society as late as 1861, and we have some numbers of its *Records* in our library.

Next comes the subject of weather prophets as distinguished from mere almanack makers; and who profess, sometimes for pelf, at other times for honour and glory, to predict the weather for any future date. These are always arising, and they do not lack a certain number of followers, who, possessing a large angle of credence, duly trumpet forth the successes of their chiefs, when they are so fortunate as to make any. The stock-in-trade of a prophet is of a slender and cheap description. He must have an inventive mind, a store of self-confidence, an insensibility to ridicule, and, above all, a keen memory for his successes, and a prompt forgetfulness of his failures. He should by choice have a theory, and this should be of the elastic order, so that if a predicted event does not punctually occur, he will be ready with a sort of codicil to amend it. Hence we find that the firing of guns has been cited as a sufficient reason for falsifying a weather prediction; and railways, too, are said to have an adverse influence, one author (not a prophet) telling us that they may be considered as "large winnowing machines, perpetually fanning and agitating the air with prodigious power, ploughing the air, as it were, and causing waves of vast extent, which, invisibly enlarging like the waves of the ocean, probably meet each other, clash, and produce modified effects, as resultants from adverse motions."

One of the first weather prophets mentioned in that delightful old book, Stanley's "Lives of the Philosophers," was Democritus, the Milesian, known as the "laughing philosopher," who foresaw a dearth of olives, and by buying up all he could get might have made a fortune, but gave it back to the bargainers with the remark, "You can see now that a philosopher can get rich when he pleases." Then there was Pherecydes, of whom Pythagoras was a favourite pupil, who predicted an earthquake three days in advance by the taste of the water from a certain well. Perhaps the earliest of all was Elijah, who from the top of Carmel pointed out the coming squall cloud, and predicted a great rain. He forms a good model for imitation to the modern weather prophets, for he did not prophesy until he saw the storm coming, and he made no secret of his method. We have still amongst us in our country, mostly without honour, seers who supply us with weather predictions in various forms, from the modest duodecimo almanack to the flaring broadsheet which compels attention; but it would be a task too long to enter on a systematic refutation of their contradictory guesses at the weather. The last of these broadsheets that caught my eye had for the days of the gale of December 1894, which Mr. C. Harding has described to us, the tame announcement of

[1] [illegible]
[2] [illegible]

"generally overcast." This did not err on the side of boldness when considered with reference to one of the severest gales of the century.

A Spanish peasant whom I heard of in Andalucia, and who had the reputation of a weather prophet, wisely said, if you want to know the weather for to-morrow, ask me early in the morning. The Indian weather prophets who made a failure had to be silent altogether for the rest of their lives: and this causes us to regret that some of our own seers were not born in that distant land.

As to the so-called weather forecasts, they only come under the title of this paper when they fail, and as eight out of ten are said to be correct, I shall only say that they are honest attempts on the part of civilised governments to warn their people as far as possible against the march of known disturbances. I could wish that the term "weather indications" or "indicated weather" had been adopted, so as to make this plain to all, and that oftener, when the signs were vague, we had the simple announcement of no change indicated.

The director of this system so well known to us, and who is playfully called the "Clerk of the Weather," sometimes receives valuable hints, even from children: and I must quote one such communication.

"Please, Mr. Clerk of the Weather, tell the rain, snow, and hail to stop for the afternoon, and rain in the night."

I may conclude this section by saying that it is a great fallacy to suppose that there is such a thing as a weather prophet. All the great authorities agree that in the present state of our knowledge no human being can correctly predict the weather, even for a week to come.

And now we must consider a class of weather fallacies of which the victims can only excite in a well-regulated mind feelings of sadness and compassion, rather than the ridicule to which at first sight they seem more naturally entitled. I mean those weather prophets in whom the delicate mechanism of the mind is touched by disorder or decay, even if it has not already fallen under the stroke of complete dementia, and who believe that they can not only foresee the weather, but, by an effort of their own minds, control the elements and compel the clouds.

These patients I had hoped only existed in small numbers; but, on perusing the correspondence of a prominent meteorologist, kindly lent me for the purpose, I find that there are many of this class whose name, like that of the ancient wanderer among the tombs, is "Legion," and who still come on, each prepared to drive the chariot of the sun, or by an exertion of his own will, odylize (the word I suppose will come) all the powers of nature.

Dr. Johnson's Astronomer says in "Rasselas":—"Hear me, therefore, with attention. I have diligently considered the position of the earth and sun, and formed innumerable schemes, in which I changed their situations. I have sometimes turned aside the axis of the earth, and sometimes varied the ecliptic of the sun, but I have found it impossible to make a disposition by which the world may be advantaged. What one region gains another loses. Never rob other countries of rain to pour it on thine own."

This type of patient, as well as those who would use their supposed power for the purpose of creating fine weather during the holidays of the people, belong to the more noble sort, but there have been others, like the notorious Friar Bungay, who for sordid reasons have professed to exert a similar power. The only wonder is that anybody ever believed them.

Now, as this malady of the mind is not incurable, I will venture to offer a practical suggestion, and would recommend these patients who have nursed themselves into the belief that they possess the keys of the weather, to seek the hill-top on a summer afternoon—the air and exercise will do them good—and watch the fine fleeces of cumulus cloud as they sail majestically across the sky, each with its attendant shadow below. Let the patient concentrate his attention upon one single feathery cloud, and try by the exertion of his utmost force of will to make it pause for a moment in its career; and, if he fails—"as fail full well he may"—then let him banish from his mind for ever the idea that he can by his own will dominate the whole firmament. And if he has ever gone into print upon the subject, let him go home, and, like Prospero, his prototype, say

> "Deeper than ever plummet sounded,
> I'll drown my book,"

and so save the world from the trouble of investigating much pure nonsense. To these sufferers I can only repeat the words of one of our own kings to the last man he touched for the evil—"I wish you better health and more sense."

I must be forgiven for having only made a selection from the vast catalogue of fallacies which have accumulated about the subject, and I must continue to regret that there are still people who are ready to believe that the saints' days rule the weather, that the sun puts out the fire, that warm water freezes sooner than cold, or that a man is a prophet because he says so himself.

This Society is clearing the ground of many weeds, and already the fallacy of the "equinoctial" gales has been exploded (by Mr. Scott), while the churchyard ghost of the supposed fatal "green Christmas" has been most effectually laid by a recent statistical paper by Mr. Dines.

Some one may ask, after all this clearing away of fallacies—What have we left? and I would venture to refer him to all the patient work which is being done in various countries, and by which a real Science of Meteorology is being slowly built up, while to the outdoor weather student I would offer this consoling reflection—There is still the sky.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Prof. A. H. Church and Dr. Fream have been offered and have accepted Honorary Professorships at the Royal Agricultural College, Cirencester. These gentlemen were both formerly professors at the College, and both took part in the recent jubilee celebrations.

It would be a great advantage to the numerous students of science and technology if the scientific works in all public libraries were arranged in a separate class, and catalogued separately. This has been done for the Central Free Public Lending Library of Nottingham, by Mr. J. P. Briscoe, the librarian, and Mr. T. Dent. All the scientific books in the library have been arranged according to the science to which they refer, and indexed according to subjects and authors. The list will thus be of great assistance to students.

New technical schools, presented to Winsford, in the salt district of Cheshire, by Mr. Joseph Verdin, at a cost of £8000, were opened by the Duke and Duchess of Westminster last week. The money is part of a fund of £26,000, originally intended to compensate property owners from subsidences brought about by brine-pumping. As he was unable to transfer the fund, the Charity Commissioners were applied to, and it was decided that £12,000 should be used in the erection and endowment of technical schools at Winsford and Northwich.

Preceding a historical account of the Owens College, Manchester, contributed by Mr. P. J. Hartog to the current *Record* of Technical and Secondary Education, the function of university colleges in technical education is discussed. Mr. Hartog points to a fundamental distinction established by the Royal Commission on Technical Instruction between (1) institutions for the instruction of manufacturers and higher managers, and (2) institutions for the instruction of foremen and workmen engaged in industrial pursuits. He rightly remarks, however, that the distinction is still vague in the mind of the public, and even in that of many educationalists. It has become more vague through the use of the words "polytechnic" and "technical school" to render the German *polytechnicum* and *technische hochschule*, to which they are not at all equivalent. The *polytechnicum* and *technische hochschule* educate managers and manufacturers; our polytechnics and technical schools, with their day-schools for lads and their night-schools for adults occupied during the day, educate foremen and workmen. It is not necesssary to insist on the inestimable value of the latter class of school; but it is of the first importance that the public should perceive the distinction referred to by Mr. Hartog, and that they should not think that they are following the example of a country like Switzerland, which eleven years ago was spending over £14,000 a year on the Zürich Polytechnic, when they vote a large sum to one of the many English polytechnics and technical schools, now springing up so rapidly with the help of funds derived from the Customs and Excise duties, while but meagre support is given to the institutions for the training of managers and manufacturers. As Mr. Hartog remarks, the university colleges combine the faculties of a German or Swiss university with those of a *polytechnicum*, but the existence of the technical part of the instruction given is often ignored because it is called university teaching, and not technical instruction, and

because side by side with the teaching of science there is the teaching of the "humanities." The remarks conclude with a statement of the amount allotted from the public funds to university colleges. Out of the sum available under the Local Taxation Act about £600,000 a year is devoted to technical education, but only £23,854 was given to fourteen university colleges in England and Wales in 1892–3 by twenty local authorities, in addition to a sum of £20,550 provided by the Treasury, of which nearly half (£13,300) went to the three Welsh colleges alone. The support certainly seems insufficient for the great services rendered by the colleges to the nation.

The third Report of Mr. J. A. Bennion, the Director of Technical Instruction in the County Palatine of Lancaster, was presented to the County Council a few days ago. It is clear from the report that every effort is being made by the Committee to expend judiciously the funds at their disposal. A sum of £28,500 was distributed among the urban and rural districts of the county last year. The following amounts were voted for work in special subjects:—Navigation, £250; Sea Fisheries, £300; University Extension Lectures, £500; Horology, £250; Mining, £500; Silk Industry, £500; Plumbing and Sanitary Science, £750; Horticulture and Bee-keeping, £500; Practical Agriculture (including Veterinary Science, Poultry-keeping, and allied subjects), £1000. In addition to the ordinary sums allotted to each district, special grants, amounting to nearly £1000, were made for the purpose of purchasing apparatus and appliances. University College, Liverpool, and the Owens College, Manchester, each received a grant of £400 for the same purpose. Classes in horology are held at Prescot, but they are quite inadequate for the whole county; and do not impart the thorough teaching, either theoretical or practical, that is given on the continent. A deputation from the Committee visited some of the Continental Schools of Horology, and as a result of their inspection they strongly recommended the establishment of a County School of Horology, similar to the school at Geneva. It was afterwards resolved at a large and representative conference that "it is desirable to establish a Technical School of Horology and Scientific Instrument-making, including electrical, optical, and mechanical instruments, both practical and theoretical, for the County of Lancaster." Efforts are now being made to put this resolution into effect. The establishment of a school to afford effective teaching in subjects relating to the silk industry is also under consideration. It is proposed to found the school upon the model of the Seidenweb Schule of Wipkingen, in Zurich. For the purpose of providing instruction in practical agriculture, a farm and farm buildings, covering nearly 150 acres, has been acquired at Hulton, near Preston. A vote of £650 was made to the Harris Institute for special courses to agricultural students; and a number of lectures on subjects relating to agriculture were delivered in various parts of the county, while agricultural experiments were carried on in several districts.

SCIENTIFIC SERIALS.

Wiedemann's Annalen der Physik und Chemie, No. 7.—Absorption spectrum of pure water for red and infra-red rays, by E. Aschkinass. The "extinction coefficients" of water for the various wave lengths at the red end of the spectrum were determined by the bolometer, and calculated by the formula

$$J' = Je^{-\epsilon d},$$

where J is the intensity of the incident, and J' that of the transmitted light, d the thickness of the layer in cm, e the basis of the Neperian logarithms, and ϵ the "extinction coefficient," which therefore means the reciprocal of the thickness which a ray must traverse in order to be reduced to $1/e$ of its original intensity. Of these extinction coefficients 200 are given, for wave lengths extending from 0·4500 μ to 8·40 μ. The minimum is at λ = 0·5375, being 0·00005, and the maximum of 2733 is attained at λ = 3·02 μ. A second maximum occurs at 6·00 μ. Between 6·7 and 8·5 the values of the extinction coefficients [illegible] between 700 and 1000.—Absorption of radiant heat by [illegible], by Charles Ironey. The liquids investigated were [illegible] in cells between an iron block and a thermopile. Among the results obtained are the following: Whenever, in a [illegible] compound, H, O, HO, or N are replaced by S or halogens, the transmittance of the solution is [illegible]. In homologous series the transmittance is regularly changed by every addition of CH_2, but the direction of this change depends upon the nature of the other atoms contained in the molecule. The absorptive power of a compound does not essentially depend upon the size of the molecule, but seems to be a property of the constituent atoms. The greatest influence is always due to H, N, and also O. In isomeric compounds the diathermancy is different, and the difference is not only connected with the difference of atomic volume of the elementary atoms, but also with the difference of linkage of the atoms amongst each other; in saturated compounds the diathermancy (transmittance) always increases with the atomic volume. The determination of the diathermancy is the most delicate test available for the purity of organic liquids or salts which are soluble in highly diathermanous liquids.

SOCIETIES AND ACADEMIES.

London.

Royal Horticultural Society, June 25.—Mr. McLachlan in the chair.—Mr. Wilson exhibited a pot containing some seedling plants, in blossom, of the North British species *Primula scotica*, which is found in pastures of Orkney, Caithness, and Sutherland. The flowers are homomorphic, not having the stamens and pistils of different lengths as in most other Primulas.—Mr. Jackman exhibited small trees of *Fagus sylvatica*, with the leaves small, entire, and round. As the trees exhibited an erect form, with short branches, it would seem to be the result of some check to growth, the form of the leaf representing a less developed state than that of the ordinary type of tree. Mr. Colinette, of Guernsey, forwarded some hazel wood found in peat near the coast of Guernsey, containing flint implements, stone rings, and pottery, presumably neolithic. No hazel is now known to be indigenous to Guernsey.—Mr. McLachlan exhibited specimens of *Melanostoma scalare* attached to flowering stems of a grass, *Glyceria fluitans*.

July 23.—Dr. M. T. Masters described a curious case of *Cypripedium* malformed, received from Messrs. Sander and Co., in which the sepals were normal, but the two petals and lip were absent. Dr. Masters also drew attention to a peculiarity in the venation of the lobed leaves of *Lavandula dentata*.—Dr. Ch. B. Plowright forwarded specimens of the parasitical fungus *Aecidium nymphaeoidis*, with the following observations: "This Aecidium has been stated by Chodat to be connected with the Puccinia on *Scirpus lacustris*. In November 1877, *Puccinia scirpi* was found floating in the river Ouse at King's Lynn. During the past winter I found it on the bulrushes (*S. lacustris*) in the 'Old Bedford' at Earith, Huntingdonshire. On revisiting the spot this July the Aecidium on Villarsia was met with in great abundance." Dr. Plowright also sent specimens of the fungus *Aecidium chenopodii*, with some remarks upon them. With reference to the specimens of flies attacked by a fungus, brought before the last meeting by Mr. McLachlan, it was reported from an examination made at Kew that "the fungus is *Empusa conglomerata*, Thaxter (a somewhat rare species), parasitic on Diptera, especially the larvae and imagines of Tipulae. Distrib. Europe and United States. This is the first record for Britain."

MM. Letellier et Fils forwarded from Caen some growing plants of thornless gooseberry, from which they have issued four kinds, raised by M. Ed. Lefort, of Meaux, France. The usual triple spines were either quite absent, or represented by mere rudiments only. Mr. Cannell sent some trusses, with small jagged-edged petals of a crimson colour, approximating the original wild form. They appeared among his long-selected beds of sweet williams, the margins of the petals being rounded and smooth.

Paris.

Academy of Sciences, August 5.—M. Marey in the chair.—Experimental study of the transverse vibrations of cords, by M. A. Cornu. The complex vibrations of strings produced as in actual musical instruments have been studied. The transverse vibrations of a string, excited in any way whatever, are always accompanied by torsional vibrations, the torsional elasticity of the cord taking effect in the same way as the transverse component of the tension. Not only is the actual vibration complicated by these torsional vibrations, but, in many cases, the transverse vibrations are themselves rendered more complex by the fact that strings are seldom or never symmetrical about their axes. The vibrations have been studied by means of very

light mirrors attached preferably to the portion of the string near one of its points of attachment or a node. Light figures similar to Lissajous' figures have been obtained. With the mirror attached parallel to the axis, all the components of the vibration are effective; when its plane is perpendicular to the axis, the torsional vibrations are eliminated.—Some considerations on the construction of great dams, by M. Maurice Levy.—The international committee on glaciers. A note by M. F. A. Forel. From the observed facts it is deduced that the general behaviour of glaciers is individual and special to themselves. There are some traits, however, which appear in certain cases in connection with the whole of the glaciers of a country. The duration of the oscillations of glaciers is measured in years by tens, the mean being at least thirty or forty years. The same variations are met with in other glacier regions as well as in the Alps. The committee ask the co-operation of scientific observers to ascertain whether there is coincidence, alternation, or lack of agreement in glacial variations: (*a*) In the different glaciers of the same continent; (*b*) in the glaciers occurring in the same hemisphere; (*c*) in the glaciers of all parts of the earth.—On the Brownian movement, by M. C. Maltézos. The conclusion is drawn that the Brownian movement is a capillary phenomenon.—Lighting by luminescence, by M. A. Witz. Lighting by means of a vacuum tube in circuit with a Holtz machine or Ruhmkorff coil is proved to give a smaller proportion of heat in relation to the quantity of light developed than any other means of obtaining light, yet the light so obtained requires the expenditure of much more energy per candle-power than ordinary sources, and hence the disposition of apparatus will require to be very much modified before light can be produced commercially at a low temperature.—On the nuclei of the Uredineæ, by MM. G. Poirault and Raciborski.—On diphtheritic anti-toxin, by MM. Guérin and Macé. The active substance appears to be of the same nature as the soluble ferments classed under the name "diastase."—On a toxic substance extracted from the suprarenal capsules, by M. D. Gourfein.—Instantaneous hyperglobulia, by peripheric stimulation; consequences, by M. Jules Chéron. Hypodermic injection of artificial serum or stimulating actions on the sensitive skin surface (such as a cold douche, massage, &c.) cause an immediate loss of the anæmic symptoms in patients suffering from anæmia. The result is probably produced by a stimulation of the central nervous system, followed by a bracing up of the vascular system as evidenced by the increase in arterial pressure. The apparent increase in the numbers of red corpuscles is caused by the greater extravasation of serum brought about under the greater pressure.

NEW ZEALAND.

Philosophical Institute of Canterbury, May 1.—Mr. C. W. Purnell, on "true instincts of animals." The definition of the term "instinct" has been greatly narrowed of late years. Formerly every act of an animal betokening intelligence was ascribed to "instinct," but the term is now restricted to acts which are performed in an apparently mechanical manner by generation after generation, and seem to be prompted by some other faculty than intelligence. The author thought that the definition could be still further restricted. Writers upon the subject had not taken sufficiently into account how much the young animal might be taught by the old, and how much it might learn from imitation. The migratory habits of certain birds, for example, were always set down to instinct, but birds usually migrated in flocks, and, in any case, with the young bird it was "follow my leader." The same remark applied to the periodical migrations of the Norwegian lemming, the salmon, and other animals. The nest-building habits of birds could be similarly explained; and even such extraordinary habits as that of the Australian Megapodidæ, which formed immense mounds of vegetable and other matter, and deposited their eggs in the midst, leaving them to be hatched by the heat evolved from the fermentation of the decaying mass. The beaver's remarkable habit of constructing dams and canals, some of which are of great antiquity, and which, if constructed by human beings, would be deemed proofs of considerable engineering skill, illustrated the author's argument. The young beaver remained in the parental lodge until the summer of its third year, when it began housekeeping for itself, so that it had abundant opportunity, during its youth, of receiving instruction from its elders, in the peculiar ways of beaverdom, and when it did make a start in life upon its own account, it still enjoyed opportunities of receiving instruction and of gaining skill by experience. Cats, dogs, and monkeys instructed and corrected their young; and the adult carnivora taught their offspring how to capture and kill their prey. Some of the most remarkable so-called instincts displayed by animals could be accounted for in the same way, and when we came to analyse these instincts, we found them to be nothing more nor less than racial habits, transmitted from generation to generation, and acquired in a similar way to that in which the racial habits of mankind are acquired. Mr. Purnell then referred to the singular instinct of the huanaco, which, in the southern part of Patagonia, resorted to ancient dying places, whither all individuals inhabiting the surrounding plains repaired at the approach of death. Mr. Hudson, author of "The Naturalist in La Plata," attributes this practice to the possession by the huanaco of "a fixed immutable instinct, a hereditary knowledge, so that the young huanaco, untaught by the adults," goes alone and unerringly to the dying place. Mr. Purnell considered this an unwarranted assumption, and that it was a far more likely supposition that, if a young huanaco was *in extremis*, the older members of the herd expelled it from their ranks, as other sick or wounded animals are usually expelled by their fellows, and indicated to it whither it should go. Traditional and tribal memories, perpetuated by communication from old to young, would account for such habits as the hive-constructing habits of the bee and the domestic and military habits of the various species of ants, which were so commonly regarded as typical of the more wonderful development of instinct in the lower animals. The fact that many so-called instinctive acts were really the products of education and experience, did not clash with the view that animals might be and probably were born into the world with a hereditary predisposition to certain tribal habits which rendered instruction in those habits easier and more effective. The mental, like the bodily, structure of any individual animal was the sum and outcome of all its progenitors' faculties, and just as its bodily organisation was better fitted to perform certain acts than others, so its mental organisation was better fitted for certain mental operations than others. Body and mind were correlated and developed in unison. The web-building spiders secreted web-building material in their bodies, and possessed highly specialised organs enabling them to produce the material in such manner and quantity that it can be used in the construction of snares, and just as this specialised anatomical structure has gradually been evolved from simple beginnings, so the mental faculty required for the construction of snares has been evolved with it. The spider is, so to speak, endowed with mental as well as with anatomical spinnerets. If we eliminated all such habits as might have been acquired from teaching or observation, there were left comparatively few fixed habits of animals which, in the present state of our knowledge, could not be accounted for by the individual having received instruction from its fellows, or gained knowledge from its own observation, and it was to such habits that the author proposed to confine the term "instinct." For the purposes of this paper, he would call them "true instincts." These true instincts were found almost solely amongst insects. By way of illustration, he would take the case of the caterpillar of a butterfly (*Thekla*), which fed within the pomegranate, but when full-grown gnawed its way out, and then proceeded to attach with silk threads the point of the fruit to the branch of the tree, so that the fruit could not fall before the metamorphoses of the insect was complete. Here, there was apparently no means by which the caterpillar could receive instruction, since no visible intercourse took place between the butterfly whose offspring the caterpillar was and the caterpillar. In considering this problem, we must firmly grasp the fact that, although the caterpillar, the pupa, and the imago were, to outward seeming, three distinct animals, in reality they were but varying phases of the same animal. Therefore the insect possessed the power of inheriting memories. We could understand how the memory of an inherited habit useful and common to one phase of the animal's existence, might readily be transmitted from the perfect insect to its offspring through the various stages of that offspring's existence. The order in which these memories were transmitted would be the order in which they would manifest themselves in the new life cycle. Did, then, the Thekla possess the power of transmitting the habit referred to? It appeared not unreasonable to suppose that such a habit might become (metaphorically speaking) so ingrained in the mental constitution of the animal as to be capable of transmission from parent to offspring. The life of an insect was short and monotonous, and its range of locomotion limited; its world was a small world; it enjoyed little scope for

variation of habit, and its ways of life consequently tended to become stereotyped upon its mental system, and so transmitted from generation to generation. As the mental nature of the animal grew more complex, instincts became more rare, because the animal exercised more choice in its actions. The fact that the nervous system of the Invertebrata was materially different from that of the Vertebrata, was full of significance in this connection. Amongst true instincts he would class such acts of protective mimicry as those performed by the Phasmidæ; although their alleged practice of shamming death might possibly be constitutional lethargy, which had misled observers. The fear which young animals, including children, usually manifested towards what was really dangerous to them, might also be classed amongst true instincts; although recent experiments by Prof. Lloyd Morgan proved that the fear was not universal. Mr. Purnell next discussed Spalding's experiments with newly-born chickens, ducks, pigs, &c., which went to show that the young of these animals were capable of performing many acts, apparently intelligent, without instruction. It must be borne in mind that the young fowl, duck, or pig came into the world with its intelligence pretty fully developed, although it grew wiser as it grew older, and all the acts mentioned by Spalding were intelligent acts, not acts performed in an unvarying fashion, but acts varying with surrounding circumstances. He therefore concluded that these acts could not be attributed to instinct, but were directed by intelligence. What he had denominated "true instincts" suggested an analogy with reflex actions, but the analogy was fallacious. Singleness was of the very essence of a reflex action. The action might be complex in its manifestation, but it was essentially one act, of which active consciousness and reflex action were contradictory terms. A true instinct commonly involved a sequence of acts, directed towards a definite end, while the acts were consciously performed.

New South Wales.

Linnean Society, June 26.—Prof. T. W. E. David, Vice-President, in the chair.—(*a*) Notes on the Omeo Blacks; (*b*) on the Monaro Blacks, with a description of some of their stone implements; (*c*) a native burial-place, near Cobbin, Monaro, by R. Helms.—Descriptions of some new *Araneidæ* of New South Wales (No. 5), by W. J. Rainbow. Three new species of orb-weavers of the genus *Nephila* from New England and Sydney were described. The fact was recorded of a young bird (probably *Estrilda temporalis*) having been caught in a web of *N. ventricosa* in the vicinity of Sydney; also that Mr. A. J. Thorpe, of the Australian Museum, had seen an emu wren (*Stipiturus malachurus*) entangled in the web of one of the *Nephila* at Madden's, near Belle Plains (N.S.W.); also at Cape York, several of the blue warblers, notably *Malurus browni* (Vig. et Hors.) and *M. amabilis* (Gould). It was pointed out that it is only young birds and those of weak wing power that are arrested by such webs; and doubt was expressed as to the correctness of the assertion of some writers that birds so caught are devoured by the spiders. The author also pointed out that each web is placed in position by the unerring instincts of the spider, simply because the situation is such as will assure abundance of food in the shape of insects, and that it is merely an accident when a bird becomes entangled in the toil. The paper concluded with a description of the mode of coition in the *Nephila*, and a list of the previously described Australian species of the genus.—On the methods of fertilisation in the *Goodeniaceæ* (part ii.), by Alex. G. Hamilton. Eleven species of *Dampiera* were treated. Of these four are usually cross-fertilised by the aid of insects, but in the remaining seven while cross fertilisation is possible by insect aid, yet self fertilisation must occur more commonly.—On a new fossil mammal allied to *Hypsiprymnus*, but resembling in some points the *Plagiaulacidæ*, by Robert Broom. The remains described under the name of *Burramys parvus* are those of a small marsupial not larger than an ordinary mouse. The form is specially interesting in having but three true molars in each jaw; and a very large grooved premolar with serrate edge very similar to that found in the Eocene genus *Neoplagiaulax*. Its affinities were dealt with at some length, and an endeavour was made to trace its relationship phylogenetically.—On some new or hitherto little-known land shells from New Guinea or adjacent islands, by C. E. Beddome. Three new Papuan species, viz. *Hemiplecta brumeriensis*, *Papuina brumeriensis*, and *Pupina beddomei*, were described, and other known land shells from German New Guinea were discussed.—Plants of New South Wales illustrated. No. viii. *Acacia lanigera*, A. Cunn., by R. T. Baker. This is by no means a rare plant in New South Wales, and yet of the several descriptions that have been published from time to time, not one is sufficient in detail to accurately determine the species; in the specimens described in the Flora Australiensis the pod was incorrectly matched. The author gave the results of an examination of perfect material from many localities, and his paper should prove of assistance in the future in the elucidation of cognate species which at present are not easy of determination.—Description of a new species of *Acacia* from New South Wales, by J. H. Maiden and R. T. Baker.

Gottingen.

Royal Society of Sciences.—The *Nachrichten*, Part 2 for 1895, contains the following memoirs of scientific interest:—

February 9.—W. Voigt: Some applications of the thermodynamic potential. Franz Meyer: On the structure of discriminants and resultants of binary forms (second note).

February 23.—E. Ritter: On the representation of groups of functions by means of one base.

March 9.—J. Orth: On mucous tissue and myxomata, with special reference to the hydatidiform mole.

March 23.—A. von Koenen: On the relation of river-valleys to erosion and to the deposit of diluvial and alluvial formations. O. Mugge: On the plasticity of ice-crystals.

May 11.—O. Wallach: Researches from the University Laboratory of Gottingen. (1) On a method of preparing ketones; (2) on derivatives of piperonal (heliotropin); (3) the oxidation-products of terpinol; (4) the reduction-products of carbon. R. Dedekind: On an extension of the symbol (a, b) in the theory of moduli. E. Netto: On the structure of the resultants of binary forms.

BOOKS, PAMPHLET, and SERIALS RECEIVED.

Books.—Traité de Mécanique Générale: H. Resal, Deux. Edit., Tome 1 and 2 (Paris, Gauthier-Villars).—L'Arithmétique Amusante: E. Lucas (Paris, Gauthier-Villars).—Traité D'Arithmétique: C. A. Laisant et E. Lemoine (Paris, Gauthier-Villars).—Philip's Handy-Volume Atlas of the World: E. G. Ravenstein (Philip).—Philip's Systematic Atlas, School Edition: E. G. Ravenstein (Philip).—A Glossary of Greek Birds: Prof. D. W. Thompson (Oxford, Clarendon Press).—Descriptive Catalogue of the Spiders of Burma preserved in the British Museum: T. Thorell (London).

Pamphlet.—Baby Buds: E. Ethelmer (Congleton, Mrs. W. Elmy).

Serials.—Engineering Magazine, August (Tucker).—Journal of the Anthropological Institute, August (K. Paul).—Strand Magazine, August (Newnes).—Himmel und Erde, August (Berlin, Paetel).—Sitzungsberichte der Physikalisch-Medicinischen Societät in Erlangen, 26 Heft, 1894 (Erlangen).—Journal of the Franklin Institute, August (Philadelphia).—American Journal of Science, August (New Haven).—American Naturalist, August (Philadelphia).

CONTENTS.

THURSDAY, AUGUST 22, 1895.

TWO BOOKS ON ARCTIC TRAVEL.

The Great Frozen Land. By Frederick George Jackson. London: Macmillan and Co., 1895.)

Ice-bound on Kolguev. By Aubyn Trevor-Battye. (London: Archibald Constable and Co., 1895.)

BOTH these books are well worthy the attention of every one interested in Arctic travel. But little was known about the island of Waigatz, and still less of Kolguef. Both books are profusely illustrated, and provided with useful maps, but some of Mr. Jackson's pictures are borrowed without acknowledgment. As might naturally be expected, the Samoyedes occupy the greatest share of attention, but some information respecting the fauna and flora of both islands is added, and the difficulties of travelling are dwelt upon with considerable detail.

The "Great Frozen Land" has been compiled by Mr. Arthur Montefiori from Mr. Jackson's journal of his trip across the tundras of European Russia, from the Kara Gates to the Varanger Fiord *viâ* Ust Zylma and Archangel. In one of the appendices, Mr. Montefiori explains the object, method, and equipment of the Jackson-Harmsworth Polar Expedition, and in another appendix Mr. Joseph Russell Jeaffreson adds some notes on the ornithological results of Mr. Jackson's journey.

The narrative begins on August 25, 1893, outside the lagoon of the Pechora, and ends on January 18, 1894, at Vadsö, the frontier town of Norway. The greater part of the book has been devoted to the Samoyedes, but the real object of the journey was neither ornithological nor anthropological, otherwise it would not have been undertaken in winter. Mr. Jackson, as everybody knows, was planning an expedition to Franz Josef Land, and the very practical idea occurred to him that a winter among the Samoyedes must give him a personal acquaintance with the difficulties of land travelling in the high north, and might suggest a successful way of battling with some of them.

Mr. Jackson must be congratulated upon his editor. Mr. Montefiori has spared no pains to make the book interesting. The information which Mr. Jackson himself procured, especially on the island of Waigatz, is valuable, and it is supplemented by quotations from Rae, Castrén von Strahlenberg, Purchas his Pilgrimes, and the works of various other travellers.

Unfortunately the ornithological part has not fallen into such good hands. There are a dozen or more gross mistakes in the spelling of the names of the birds, and in addition there are some curious inconsistencies. In the preliminary observations we are told that Mr. Jackson brought home "of swans—not Bewick's—but the common variety of that region," in spite of which the only swan in the list (No. 28) is Bewick's swan. Mention is made of grossbills. (Does the writer mean crossbills or grosbeaks?) Of the little stint (No. 45) it is stated that the only authentic eggs were those taken by Middendorff. There is no reason to believe that Middendorff ever found the eggs of the little stint. The eggs which he records as being those of *Tringa minuta* were probably those of *Tringa ruficollis* and possibly those of *Tringa subminuta*. The first identified eggs of the little stint were taken on July 22, 1875, by Mr. Harvie-Brown, on the eastern shores of the lagoon of the Pechora, and a few days later a score had been obtained by the expedition. Other eggs equally authentic have since been taken in Lapland, Novaia Zemblia, and Kolguef. It is extremely unlikely that the identification of the species in the list is always correct. No. 10 doubtless refers to *Phyllosopus tristis*, and not to the chiffchaff: No. 12 is more likely to be a redpole than a siskin; No. 39 is doubtless *Ægialitis hiaticula*, and not *Æ. curonica*, and No. 53 is more likely to be *Stercorarius richardsoni* than *catarractes*. In but few cases is the exact locality given, so that on the whole we must condemn the list as worse than useless.

Mr. Jackson went out on one of Captain Wiggins' numerous voyages to the Yenesei, and was left on the southern shore of the Yugorski Strait, with little or no knowledge of the language of the country, to fight his way as best he could. He was anxious to go to the Yalmal Peninsula, but the Samoyedes declined to take him there. After reading the account of the difficulties which Drs. Finsch and Brehm encountered, it must be admitted that their decision was very wise. Mr. Jackson was, therefore, obliged to content himself with exploring Waigatz Island, and succeeded in making the detour in a fortnight. The north of the island enjoys a milder climate in winter than the south, the Yugorski Straits being frozen over, whilst there is always more or less open water in the Kara Gates.

Winter came upon the adventurous traveller rather suddenly during the second week of October, and on the 13th he began his sledge journey to the Norwegian frontier. During the three months that this occupied, Mr. Jackson lived among the Samoyedes and picked up many useful hints as to dress, food, &c., as well as accustoming himself to camping out in the snow, travelling by sledge, using snow-shoes, &c. This information and experience will doubtless be of great value to him on his expedition to Franz Josef Land. It is worth something to know, instead of only to suspect, that you have pluck to face the difficulties of Arctic travel, and every one wishes a safe return to a traveller who with but small previous experience has gone to try his luck in battling with enormous difficulties.

Mr. Trevor-Battye's book treats of the journey which he made in 1894 to a still less known part of the Arctic Ocean. The island of Kolguef lies about 150 miles to the west-north-west of the lagoon of the Pechora, whilst the island of Waigatz lies about as far to the north-east of that basin. Mr. Trevor-Battye sailed from Scotland in the steam-yacht *Saxon* on June 2, and landed, with his bird-skinner, on the west coast of Kolguef on the 16th; but as ill-luck would have it, they went again on board, and did not finally leave the vessel until the 21st, after the ice had driven them to the north of the island. On August 18, a Russian merchant from the Pechora arrived on Kolguef, and Mr. Trevor-Battye and his companion left in his boat on September 18, and after a nineteen hours somewhat perilous sail, reached the mainland. In two months he was back again in England.

Mr. Trevor-Battye appears to have kept a copious journal, and very interesting reading it is. It bears internal evidence of having been written on the spot by

one who was well trained in habits of observation, and accustomed to the drudgery of making daily notes of what he saw. The remarks on the peculiarities of the Samoyedes are valuable from their originality, and are an important contribution to the ethnology of Siberia in Europe. The value of the ornithological appendix is in strong contrast to that in Mr. Jackson's book; but it must always be remembered that Mr. Trevor-Battye is himself an ornithologist, and travelled at a time of year when the country was full of birds. Mr. Jackson makes no pretension to any knowledge of ornithology, he travelled at a season when birds were very scarce, his mind was occupied with other thoughts, and he had the misfortune to entrust the few skins he brought home to hands as inexperienced as his own.

Mr. Trevor-Battye's account of the way in which the Samoyedes surround the geese when most of them are unable to fly, because they are moulting their quills before migrating to the coasts of Western Europe to winter, is most graphic.

On the south-east coast of Kolguef the sea is shallow, and at low tide there is much sand exposed within the line of the outer barrier of piled-up ice, which lies some three miles out to sea. In this lagoon thousands of geese retire towards the end of July to moult their flight feathers. When they are in this more or less helpless state, the Samoyedes slip down in their boats through the fog and get behind them, and gradually drive them on shore, where a decoy net has been staked out to receive them. Once inside this trap they are slaughtered without mercy to provide food for the winter. The day's bag was 3300 brent geese, 13 bean geese, and 12 white-fronted geese. Fortunately for the two species of grey geese, they moult a little later than the black geese, so that most of them were able to fly. The Samoyedes told our travellers that the bernacle goose nested at the north of the island.

Mr. Trevor-Battye was fortunate enough to obtain eggs both of the grey plover and little stint. Mention is made on page 209 of the capture of two examples of the curlew sandpiper, but curiously enough this bird does not appear in the ornithological appendix.

There is an interesting appendix on the flora of Kolguef. The cloudberry, one of the most delicious of fruits, which is found on the highest summits of the Peak of Derbyshire, and on the Craven Mountains in Yorkshire, was in flower by the second week of June, but the fruit did not ripen before August 25.

Both Kolguef and Waigatz have an island climate, very different from that of continental Siberia; and it might be said of both of them, as is frequently said of Lapland, that they have eight months winter, and four months no summer. The frequent rains are no doubt very favourable to the growth of many species of plants, but they sadly interfere with the pleasures of camp-life. When the north wind brings down fogs from the Arctic ice in June, and snow followed by rain in July, varied with thunder in August, and frosts in September, it requires some enthusiasm for birds or flowers to enjoy the fight with the storms. There are, however, some compensations. If there be little sunshine there is no night, and when the north wind blows the plague of mosquitoes is stayed. HENRY SEEBOHM.

ANOTHER BOOK ON SOCIAL EVOLUTION.

The Evolution of Industry. By Henry Dyer, C.E., M.A., D.Sc., &c. (London: Macmillan and Co., 1895.)

THIS work contains much valuable suggestion, many admirable sentiments, and a selection of choice extracts from the best writers on social philosophy; but it is hardly what one would expect from its title. The idea of evolution is, no doubt, more or less present to the author throughout his work, and some of its main characteristics are referred to and illustrated by the phenomena of industrial progress; but there is a want of system and of logical connection in the treatment of the subject, and an entire absence of the unity of design, forcible reasoning, and original theory which were such prominent features in Mr. Kidd's work.

Dr. Dyer's book is an eclectic one, inasmuch as it adopts from previous writers such ideas and principles as commend themselves to the author. His frequent quotations are often followed by the remark—"there is much truth in this"—and it is sometimes rather difficult to determine what are his own conclusions. It would not be difficult for both individualists and socialists to find support here to their own views; but the general impression made by the volume is, that the author is profoundly dissatisfied with the present state of society, and is inclined to some form of socialism as the only effective remedy.

In the introductory chapter we find many of the objections to socialism very strongly put, though most of these are objections to particular details rather than to essential principles; yet in the same chapter we find statements of fact which answer many of these objections. Thus we are told (p. 21): "Among the co-operators, for instance, we find men managing, with the highest efficiency, concerns of great extent and importance for salaries smaller than those of bank clerks. They find their real salaries in the success of their work, and in the knowledge that it will lead, not simply to individual riches, but to the welfare of the community, and especially of the workers."

After quoting from the late Prof. Cairnes to the effect that no public benefit of any kind arises from the existence of an idle rich class, he adds: "From a scientific point of view, and therefore from a moral point of view, no man or woman, unless physically or mentally disabled, has any right to remain a member of a community unless he or she is labouring in some way or other for the common good. In every organised society, therefore, there can be no rights apart from duties" (p. 37). This principle is thoroughly socialistic, and would lead us very far indeed; but here, as elsewhere, the author seems afraid to carry out his own principles to their logical conclusions. Further on, he tells us that "In some parts of the country as much as between 40 and 50 per cent. of all the deaths that occur are those of children under five years of age, a state of matters which is a disgrace to our civilisation"; and, after quoting some forcible words of Lady Dilke as to much of England's industrial greatness being due to her practically unlimited supply of the cheap labour of her women and girls, he concludes: "It is therefore evident, both from an economic and a moral point of view, that the individualist system of industry, by itself is not sufficient to bring about a stable social

structure." He describes hospitals as institutions "which are founded for the purpose of taking in some of the waste products of our industrial and social system, and for repairing, as far as possible, the injuries which they have suffered"; and he adds: "Such institutions are sometimes pointed out as the glories of our civilisation. They should, on the contrary, be looked upon chiefly as monuments of neglected duties, and the object of all social reformers should not be to extend them, but so to improve social and industrial conditions as to render them almost entirely unnecessary." This will be a new idea to many good people, but it shows that the author is far ahead of the average social reformer.

Again, he points out that the armies and navies of the world afford most instructive lessons in collective action, and that it would be equally possible to have armies of men organised for industrial work, and navies for carrying on such commerce as was essential for supplying the wants of the community; and in his chapter on "Industrial Training," he shows how necessary it has become to supplement the very imperfect means now afforded to apprentices to learn their business by some systematic and well-organised system under local or other authorities.

In the last chapter, on "Industrial Integration," suggestions are made as to the course of future legislation. The author thinks that it will be made increasingly difficult for people to live upon unearned incomes, while the equalisation of opportunities will reduce the rewards of extra ability. How this is to be effected is not made clear; but the author is decidedly of opinion that "the resumption of the ownership of the land by the community is a first essential to equality of opportunity"; concluding with the rather weak remark, that "the methods to be adopted to bring this about will require very careful consideration, and must be comparatively slow in their operation."

After quoting the opinion of the late Mr. Werner Siemens, that the progress of science will lead not to the increase of great factories, but to the return to individual labour, Mr. Dyer adds:—

"The factory system will continue, and no doubt be extended, for the supply of the common necessaries of life, but the applications of electricity and other methods of obtaining motive power will enable large numbers of small industries to be carried on in country districts. This movement will ultimately bring about a society of integrated labour, which will alternate the work of the field with that of the workshop and manufactory. In order that the evils arising from unlimited competition may be avoided, these departments of work will all be so coordinated that a considerable region will, to a large extent, be self-contained as regards its requirements, and will produce and consume its own agricultural and manufactured necessaries of life."

This conclusion has been reached by the present writer and some others, mainly from broad considerations of economy. But when it is set forth in a work which professes to trace and discuss "the evolution of industry," we expect to be shown that it is a logical and inevitable result of the evolution that has occurred and is now going on. This is nowhere done, and in this respect the book must be pronounced a failure, although there is much in it with which every friend of progress and every student of social science must heartily agree.

ALFRED R. WALLACE.

MAYAN HIEROGLYPHICS.

A Primer of Mayan Hieroglyphics. By Daniel G. Brinton. Publication of the University of Pennsylvania Series in Philology, Literature, and Archæology, vol. iii. No. 2. (London: Ginn and Co.)

ALL who are interested in American archæology (and especially those who do not read German) must feel greatly indebted to Dr. Brinton for his "Primer of Mayan Hieroglyphics," for in this little book he has brought together the result of work done during the last few years in America, England, and Germany, and his own extensive knowledge of the subject of which he treats gives the highest value to his selections and his comments.

That there has been a distinct advance made all along the line cannot now be doubted, and material for study has not only increased, but has been made more generally available to the student

Dr. Brinton divides the Maya inscriptions into their three elements—mathematical, pictorial, and graphic, and proceeds to review them in that order. He first describes Prof. Förstemann's interesting investigation into the Maya notation for the higher numbers, and then enumerates the various divisions of time in use amongst the Mayas, and points out that the bringing of these irregular numbers into unison with the lunar and stellar years is the difficult task which lies before the investigator.

"We need not search" [in the inscriptions] "for the facts of history, the names of mighty kings, or the dates of conquests. We shall not find them. Chronometry we shall find, but not chronicles: astronomy with astrological aims; rituals, but no records. Pre-Columbian history will not be reconstructed from them. This will be a disappointment to many; but it is the conclusion toward which tend all the soundest investigations of recent years."

Whilst dwelling upon the elaborate and careful researches of what may be called the astronomical school of investigators, Dr. Brinton does not fail to give an instance of how far they differ from their rivals, by quoting the explanation given of a certain series of figures in the "Codex Cortesianus," which, in agreement with Förstemann, he supposes to represent the position of certain celestial bodies before the summer solstice, whilst Prof. Cyrus Thomas says of them, "It may be safely assumed that these figures refer to the Maya process of making bread"! Such differences of opinion would seem to indicate that the study of the inscriptions has not yet emerged from the stage of guess-work, and to a great extent this is undoubtedly the case; but it is satisfactory to mark how the happy guess-work of the last few years, and the criticism it has provoked, has led to a solid foundation of ascertained fact from which a fresh start can now be made.

Under the heading of "Pictorial Elements," Dr. Brinton gives us a list of the Maya gods and their attributes, gathered chiefly from old Spanish records. Regarding some of those deities, he has already published some interesting studies in "American Hero Myths." He then proceeds to discuss the cosmogony of the Mayas, and in the following pages deals with the pictorial representations of the Maya divinities, referring continually to the list published in 1892 by Dr. Schellhas in the *Zeitschrift für Ethnologie.*

Students appear to be now fairly well agreed about the order in which the glyphs are to be read, and on the identification of the signs representing days, months, and some of the other divisions of time; but there still remains for consideration a large number of glyphs to which the most varied and contradictory interpretations have been given.

The most essential qualification for a student of Maya inscriptions is without doubt a thorough knowledge of the Maya language as it is now spoken in Yucatan. Dr. Brinton, who is a distinguished philologist, has doubtless learnt all that imperfect dictionaries and grammars can teach him, and on that account alone would hold a foremost position in the investigation. But the only way to acquire the special knowledge which is now so much needed is a prolonged residence in Yucatan itself, which can be reached in five days from New York; and it would be good news should we hear that Dr. Brinton has used his great influence in persuading some of the well-endowed universities or colleges in America to establish travelling scholarships for the study of native American languages, and had placed the Maya language first on the list.

OUR BOOK SHELF.

Harrow Butterflies and Moths. Vol. i. By J. L. Bonhote, M.B.O.U., and Hon. N. C. Rothschild, F.E.S., F.Z.S. 8vo. Pp. xi. and 95. Plate. Harrow: Wilbee, 1895.

AT the present day, natural history receives considerable encouragement at our larger public schools and colleges, many of which now boast a Natural History Society of their own, and publish a journal of their own. The naturalists of Harrow School have struck out a bolder path, and have begun to issue a series of manuals of their local fauna, of which this is the second, the first, by Mr. Barrett-Hamilton, having been devoted to the birds of Harrow.

The volume before us includes the *Macro-Lepidoptera* to the end of the *Noctuæ*, and is illustrated by a useful plate presented by the Hon. Walter Rothschild, representing the antennæ of the three British species of *Ino*, the neuration of *Papilio machaon*, and the egg, larva, and pupa of *Vanessa cardui*. The second volume will include the remainder of the *Macro-Lepidoptera*, and the *Pterophoridæ*. South has been followed for Latin names, and Newman for English names, and the indefinite term "variety" has been very properly abandoned.

The district included comprises, roughly speaking, a radius of about five miles from Harrow Hill, and incorporates the notes of a considerable number of observers, the majority being connected with Harrow School. It consists mainly of a record of localities, times of appearance, and habits, with occasional notes on species not found in the district, or on aberrations.

As a record of the present fauna of a restricted locality, this little book will be of permanent value, in view of the changes which are always taking place in the appearance, disappearance, and variation in distribution and abundance of individual species. One or two species which we should hardly have expected to meet with are included in the list, such as *Lycæna corydon*, but we are surprised to miss not only such species as *Aporia cratægi*, which was common round London at the beginning of the century, though probably no Harrow records were kept so far back, but to find no Fritillaries recorded, except *Argynnis selene, euphrosyne, paphia* and *Melitæa aurinia*. The fondness of *Vanessa atalanta* for fruit is noticed; and we may remark that *V. antiopa* also shares this habit with its congener.

Altogether, we have to congratulate the authors and the Harrow School Scientific Society on having produced a very creditable little book, and we hope that it will serve as an incentive to the members of other School Scientific Societies to go and do likewise. W. F. K.

Hand-list of Herbaceous Plants Cultivated in the Royal Gardens, Kew. (Sold at the Royal Gardens, Kew.)

ABOUT a quarter of a century ago, the border-flowers in which our grandsires delighted were all but pushed out of existence by "bedding plants" and ribbon-borders of glaring hue. Nurserymen who had good stocks of the older favourites found them unsaleable, and discarded them accordingly. Then came a change, largely owing to the influence exerted by Mr. Robinson's publications. "Herbaceous" and "Alpine" plants were once more received into favour, and are probably more numerous and more extensively cultivated than ever they were. Kew, as usual, has been responsive to popular demands. In times well within the memory of the present generation, the plants we speak of were grown there, as in other botanic gardens, in ugly gridiron-like beds, an arrangement which might have been suitable for strictly botanical purposes, but which was as unattractive as possible.

To obviate this, and to allow of the plants growing in the most natural way possible, the new rockery was formed, mainly, we believe, after the plans of Mr. Dyer. At any rate, it now forms one of the most attractive features in the garden, and with the frames and "Alpine House," serves excellently to illustrate this class of plants.

A proper catalogue, of course, became necessary, for, unfortunately, the names and descriptions in the most popular books on the subject, are not to be depended upon. The present publication is an alphabetical list, the only information given in addition to the names, being a mention of the botanist responsible for the name, and a general indication of the native country of the plant.

The names of the botanical authorities are given in the contracted form adopted in scientific works; but in a list of this character, which is mainly intended for unscientific readers, the names should either be given in full, or an explanation of the abbreviations supplied.

No fewer than 6000 species, it appears, are now grown at Kew, including, we see, as many as a hundred species of Carex.

A Manual of Book-keeping. By J. Thornton. Pp. 527. (London: Macmillan and Co., 1895.)

THE late Prof. Cayley is quoted by the author to have said of book-keeping, "It is only its extreme simplicity which prevents it being as interesting as it otherwise would be." But what was simplicity to the master of pure mathematics is very far from being so to the average shopkeeper, as witness the testimonies of Official Receivers in Bankruptcy. As Mr. Thornton points out, a general opinion among uneducated tradesmen is that book-keeping was invented to conceal the facts; and therefore they think the least they know about it the higher is their code of commercial ethics. This book will undoubtedly assist in removing such mistaken opinions; it is the clearest exposition of the principles and practice of book-keeping that we have yet seen, and the most original in design. The science and art of the subject are dealt with simply; the matter is arranged in an admirable manner; and by subordinating details to principles, the author has made his book worthy of the attention of all students who wish to acquire a sound and scientific knowledge of book-keeping.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The University of London.

Mr. Thiselton-Dyer now narrows his attack to my suggestion that in voting on the new Charter, members of Convocation should do so "as at a Senatorial election," *i.e.* by voting papers. This seems a very narrow basis for so severe a condemnation.

The reason for this provision was, I presume, that as many members of Convocation are professional men, masters of schools, &c., it is in many cases difficult, if not impossible, for them to come up to London.

The provision applies, I may add, not only to Senatorial, but also to Parliamentary, elections. I cannot see why Mr. Thiselton-Dyer should assume that a vote so taken would "destroy the prospects of academic study in London." That, however, is not an attack on me, but on the Constituency.

High Elms, August 17. John Lubbock.

Plant-Animal Symbiosis.

In Prof. Stewart's collection at the Royal College of Surgeons there is a preparation of a mimosa which protects itself from browsing animals by providing in its great thorns a domicile for a species of vicious, stinging ants. I believe this example of plant-animal symbiosis comes from one of the West Indian Islands, while on the mainland of America the same species of mimosa exists, but suffers greatly from the depredations of animals, because there is no suitable ant to come and ward them off. If my recollection of the distribution is correct, the following note of a similar phenomenon in South Africa, I think, is of considerable interest.

In a recent tour through the Karroo, in search of the skeleton of the Dicynodons, I came across a mimosa tree which here forms the chief fuel, on one of the lower branches of which there were some very large thorns; one of these had a little oval hole bored just beneath the summit. On breaking it open, there issued an incredible number of ants, considering that they were packed in the space of a pair of spines about four inches long and half an inch in diameter. The asexual forms were of the usual two kinds: the soldiers were about a quarter of an inch long, brown, and very attenuated, showing very markedly the influence of surroundings on form; while the workers were scarcely half the size of their protectors, and of a darker hue. The sexual forms I did not see. The ants emerged from the crack in a very sleepy manner, and did not seem at all aggressive; this may have been on account of the cold, which would affect them more than their relatives which live in the earth. Embedded in the soft wood of the stem, where the two spines meet, were several aphides, which thus were able to feed themselves on the sap of the tree, and yet always be within the house of their owners. In the West Indian thorn-tree the leaves offer a further inducement to the ants to remain constantly near them, by providing at the extremity of the leaflets little masses of a nutritious substance adapted to the digestions of their guests; in the South African tree there is a mass situate at the base of the leaves, similar to that in the cherry, which probably serves the same object. On returning shortly afterwards, I found the ants had trekked with all their cattle, and I failed to trace their whereabouts. The locality was the gold-fields of Spreeunfontein, in the Prince Albert district. Ernest H. L. Schwarz.

Cape Town, August 1.

Definitions of Instinct.

I have read with interest the abstract of Mr. C. W. Purnell's paper which you published in last week's Nature (p. 383). I think he is in error in supposing that young birds do not afford us examples of truly instinctive activities. The way in which a young moorhen swims with accurate coordination, before the down is well dry after hatching, and before it can walk steadily, is very instinctive. I would suggest to Mr. Purnell that there is a wide field for observation open to him among his native birds. If he will hatch some of them out in the incubator, and carefully note what they can do prior to experience, and how their activities are modified by experience, he will help to solve some of the difficult problems of habit and instinct.

I have myself advocated a restriction in the meaning of the term somewhat similar to that for which he argues. I shall be obliged if you can find space for the provisional scheme of terminology thus suggested in *Natural Science* for May 1895, which I have since somewhat extended and amended. To bring it into line with modern biological thought, a good deal of stress is laid on the question of heredity, and on the distinction between the definiteness which is congenital and that which is acquired. It may be premised:

(1) That the terms *congenital* and *acquired* are to be regarded as mutually exclusive. What is congenital in its definiteness is, as prior to individual experience, not acquired; the definiteness that is acquired is, as the result of individual experience, not congenital;

(2) That these terms apply to the individual. Whether what is acquired by one individual may become congenital through inheritance in another individual, is a question of fact which is not to be settled by implications of terminology;

(3) That the term *acquired* does not exclude an inherited potentiality of acquisition under the appropriate conditions. Such inherited potentiality may be termed *innate*. What is acquired is a definite specialisation of an indefinite innate potentiality;

(4) That what is congenital and innate is *inherent* in the germ-plasm of the fertilised ovum.

Congenital movements and activities: those the definite performance of which is antecedent to individual experience. They may be performed either (1) at or very shortly after birth (*connate*), or (2) when the organism has undergone further development (*deferred*).

Congenital automatism: the congenital physiological basis of those movements or activities the definite performance of which is antecedent to individual experience.

Physiological rhythms: congenital (and connate) rhythmic movements essential to the continuance of organic life.

Reflex movements: congenital, adaptive, and coordinated responses of limbs or parts of the body: directly evoked by stimuli.

Random movements: congenital, more or less definite, but not specially adaptive movements of limbs or parts of the body; either centrally initiated or directly evoked by stimuli.

Instinctive activities: congenital, adaptive, and coordinated activities of relative complexity, and involving the welfare of the organism as a whole; specific in character, but subject to variation analogous to that found in organic structures; similarly performed by all the members of the same more or less restricted group, in adaptation to special circumstances frequently recurring or essential to the continuance of the race; often periodic in development and serial in character.

Imitative movements and activities: due to individual imitation of similar movements or activities performed by others.

Impulse (*Trieb*): the affective or emotional condition, whether congenital or acquired, under the influence of which a conscious organism is prompted to movement or activity, without reference to a conceived end or ideal.

Instinct: the congenital psychological impulse concerned in instinctive activities.

Control: the conscious inhibition or augmentation of movement or activity. While the power of control is innate, its special mode of application is the result of experience, and therefore acquired.

Intelligent activities: those due to individual control or guidance in the light of experience through association (voluntary).

Motive: the affective or emotional condition under the influence of which a rational being is guided in the performance of deliberate acts.

Deliberate acts: those performed in distinct reference to a conceived end or ideal (volitional).

Habits: organised groups of activities, stereotyped by repetition, and characteristic of a conscious organism at any particular stage of its existence.

Acquired movements, activities, and acts: those the definite performance of which is the result of individual experience. Any modifications of congenital activities which result from experience are, so far, acquired.

Acquired automatism: the individually modified physiological basis of the performance of those acquired movements or activities which have been stereotyped by repetition.

C. Lloyd Morgan.

A Scheme of Colour Standards.

THE confusion which has long prevailed, and does not promise any immediate disappearance, in the use of colour names, is an inevitable consequence of the absence of any definite standards of colour. In music and form we have well-established and very satisfactory terms to describe definite sense perceptions, and it would be difficult to conceive how we could dispense with them: but for colour perceptions we have neither any well-defined concepts for those terms which have become well established, nor any definite and well-arranged system of colour terms for common use. Those terms which have acquired a somewhat definite significance are nevertheless used for a very wide range of variation. Vermilion and ultramarine, terms which have been used by many of our best authorities on colour, for want of anything better, as a basis for comparison and analysis, are nevertheless used for very variable concepts. The difference between a Chinese and a German vermilion in pigments is very noticeable. A Winsor and Newton "chrome yellow" and a German "chrome yellow" differ by more than twenty-five per cent. of yellow. Among several samples of blue pigments a still greater variation is generally found. When this is true of such terms, what shall we expect will be the case with that very much larger group of terms whose meaning has never reached any considerable degree of accuracy, as olive, citrine, russet, &c., or that still more vague but innumerable class of terms in vogue in popular usage, like "crushed strawberry," "baby blue," "ashes of roses," "peacock blue," "hussar blue," and a host of others still more vague and transitory?

Naturalists have been at very great inconvenience because of the lack of any agreement in the use of colour terms for botanical, entomological, and ornithological descriptions. Our greatest American authority in descriptive botany is sometimes confusing in his description of flowers because of this lack of any standard terms with admitted significance. Zoologists, too, have sought in vain for some basis of agreement, and the futile attempts to establish some such basis of agreement are familiar to every ornithologist.

In applied science and the arts the inconvenience has, if possible, been still greater, inasmuch as the number of persons interested is larger. And this inconvenience is steadily increasing as the revelations of chemistry disclose hues more and more brilliant, for which new names are as constantly coined. With the rapid advance of the art of dyeing the necessity of some system of colour nomenclature becomes more and more imperative.

The valuable research of Prof. Rood has contributed greatly to our knowledge of colour, and that in a time when much less was known on the subject than now. The later contributions of Abney and Church in England have also given valuable additions to the science of colour. The able works of Chevreul, Koenig, and Von Bezold are also important contributions to the subject. Above all, the masterly works of Helmholtz, Hering, Kuhne, and Carpentier are most valuable contributions upon this subject.

But while these have given the greatest help to our understanding of the nature and relations of colour, none of them has given any solution to the problems just now hinted at. No system of colour nomenclature has been offered, nor any set of colour standards proposed.

It was in view of these difficulties that the writer proposed, about twelve years since, while connected with the Springfield, Massachusetts, High School, as teacher of physics, botany, and zoology, that a series of colour standards, based on the hues of the solar spectrum, and selected by a consensus of colour experts, should be adopted as a foundation of all our colour work, and especially that its use in all educational work be made the means of establishing a better and more accurate knowledge of colour. This proposition was received with favour from the first by those to whom it was mentioned, and an attempt was soon made to put the scheme into material form; but the difficulties which lay in the way of producing any pigmentary hues which could in any adequate degree represent the hues of the solar spectrum were so great, that very little progress was made for several years.

The wave theory of light long ago established the fact that vibrations of an almost infinite variety of wave-length in the luminiferous ether impinge upon the human retina and produce the effect which we call white light. From these we may select any wave length we please, and giving it a name, have a colour as accurately fixed as any musical note or geometrical form. The desirability of such a definiteness in the terms which describe colour, all will immediately recognise. It is not necessary that a large number of thus accurately fixed colours should be made the basis of colour nomenclature, for then the eye could not distinguish between the colours selected. Every eye can, however, unless it be colour-blind, distinguish six well-marked colours in the solar spectrum, for which there are well-recognised names. The theory of three primary colours from which all the other hues of the spectrum are derived is no longer possible with the present knowledge of the laws of light, and the always questionable indigo of the rainbow is no longer recognised as one of the distinct spectrum colours. As a matter of practical convenience we may select any number of colours which is found to be desirable for standards if only they are accurately defined. This has been done for the six commonly recognised spectrum colours, red, orange, yellow, green, blue, and violet.

To obtain the agreement of six or eight persons well skilled in the use of colours as to exactly what portion of a projected spectrum of eight or ten feet long should be selected for each standard was a much less difficult task than would first have been supposed. It was found that very great unanimity of judgment prevailed when the comparison was made. The portion of the spectrum having been selected for each colour, two other things yet remained to be done. These were very important factors of the proposed scheme.

First of all the exact location of the area must be determined by careful measurement of the wave-length of its centre. This would make it possible to ascertain or relocate the standard in any part of the world without any material representation of the colour designated: in other words, this makes possible the use of the designated colour as one of a series of universal standards. To render any set of standards of most permanent and wide value, it is desirable that it be adopted by somebody whose authority will be generally recognised. In the case of standards of measurement, the Government establishes standards in the interests of commerce and equity. In most other cases some learned society adopts the standard, and thus gives it the benefit of its own authority. The American Metrological Society has appointed a committee with instructions to report a recommendation for the establishment of six standard colours. The endorsement of such a society would go far toward the establishment of any scheme of colour nomenclature if the scheme be a practical one. Without the element of practicability no authority could make such a scheme of any value.

After several years of careful study of the practical problems involved, especially the relations of the colour standards to one another, it was found that to select any particular pigment as one of the standards, and make the scheme adapt itself to this as a standard, was not only unscientific but unpracticable. The standards if selected with a view to practical or artistic purposes, and most of all with the hope of making the scheme of educational value, must bear a relation to one another not unlike those of the musical scale. It must be possible at least that the union of these standards should produce the intermediate hues of the solar spectrum in colour if not in purity. After much care six standards were selected and at once put to practical use. This was as early as 1884. The exact measurements of the wave-lengths of these standards were published in *Science* for June 9, 1893. The values there given were as follows: Red 6587, orange 6085, yellow 5793, green 5164, blue 4695, violet 4210, in ten-millionths of a millimetre.

These measurements are for the centre of an area of the solar spectrum represented by fifty of the same units. A measurement differing from either of these by twenty or twenty-five would hardly vary to a degree to be perceptible to the trained eye, much less to the ordinary eye. There is, however, a very great variation in different parts of the spectrum. In the orange, yellow, and green, where the change is rather rapid, a small difference is very readily perceived, while in the red as well as in the blue and violet the same difference would be scarcely noticeable.

To Mr. Milton Bradley, of Springfield, Massachusetts, is due great credit for first undertaking to put this idea of spectrum standards into practical working material. When the idea was first proposed he was largely engaged in the manufacture of coloured papers for educational purposes, and he at once undertook to reproduce the spectrum colours in his educational papers. The task proved no easy one, notwithstanding the great advance which the discovery of the aniline dyes had made in the production of brilliant colours. It was only with the utmost persistency that Mr. Bradley was able to accomplish the task which he had voluntarily undertaken. After long and repeated experi-

ments he succeeded in getting coloured papers which are very good reproductions of the hues of the solar spectrum. These papers have now been used for several years very extensively in kindergarten and primary school work, and they are an important means toward the education of a new generation of students to a true conception of colour, a more careful use of colour terms, and a sharper discernment of colour perceptions.

At a meeting of the Society of American Naturalists, held in Boston, December 31, 1890, I read a paper in which was given a more elaborate carrying out of the scheme which I had previously proposed.

In order that any fixed scheme of colour nomenclature may be of some practical value it must, of course, be readily understood by people of only ordinary intelligence, and must be complete enough to meet the ordinary wants of everyday life. There must be something that is so completely fixed as to be perfectly trustworthy for present and future needs.

In the solar spectrum we have an invariable source from which to derive our spectrum standards, and upon these the whole scheme is to be based.

Since, however, the six spectrum standards do not give a very extensive *répertoire* for common use, to say nothing of the needs of the more artistic, it was proposed to introduce between each two spectrum standards two intermediate hues to be formed by the union of the two spectrum standards in definite proportions. Thus between orange and red would be introduced an orange-red and a red orange. In the former red would predominate, while in the latter orange would be more prominent. Inasmuch as these hues are only intended to be combinations of the spectrum standards, it is not necessary, or even perhaps desirable, that these hues be absolutely fixed. If, however, this is desirable in any particular case, it can be accomplished in a manner which will be indicated subsequently. In addition to the two hues introduced between each two standards it is also necessary to use a violet-red and a red-violet (or two purples, a reddish purple and a violet purple) to represent the actual combinations which occur in nature.

It is also very desirable that the standards be produced in some material form in order that it be of any practical value. The task of reproducing the brilliant hues of the solar spectrum in pigmentary material or in glass is much more difficult than one not acquainted with the matter would suspect. It would not be difficult to select well-known pigments, and then determine the wave-length which most nearly corresponds to the hue of the pigment; but any number of such selections would not form a symmetrical series of colour standards. The colours for such a scheme being selected and their wave-length determined, the other and more difficult problem is that of finding some combination of pigment which will reproduce it. This task of reproducing the spectrum hues was a very difficult one. It is impossible to reproduce some of the spectrum colours with the ordinary pigments either in hue or in quality.

Almost at the very outset of this work in colour it was found that it would be necessary to depend upon the somewhat fugitive aniline colours for some of the standards as the only colour material which would approach the spectrum hues in brilliancy. The difficulty of keeping the standards up to tone, so to say, while using somewhat changeable material, is a serious inconvenience but not an insuperable barrier. With the solar spectrum recognised as the source to which we must always go to correct our standards, the great difficulties of colour-study are met. The most desirable thing now to be accomplished is the discovery of some permanent colour material in which to reproduce the spectrum standards. Some convenient form of tablet would then be produced which could be supplied to all who are willing to provide themselves with it, and to these all questions of colour would be referred. The standards thus established, the intermediate spectrum hues are determined by them.

Now, by the use of the Maxwell discs in the standard colours described above, we may fix upon definite proportions of each which we will use for any other hue. If, for example, we desire to introduce between red and orange two hues, we must first of all know something of the relative effect of the two colours, and combine them in inverse proportion to what we are accustomed to call the value of the colours. The colour which has the lowest value will require to be used in larger proportion than the other. In this case we may take a red and an orange disc and put them together in the manner above described. For convenience of measurement, a disc just a little larger than the coloured discs, with the margin graduated into one hundred degrees, is placed behind the coloured discs, and the sectors adjusted as desired. As the red has the lowest value, more of the red disc must be exposed in order to produce an effect equal to that produced by the orange. If it is desired, therefore, to introduce two hues between red and orange, we must still more increase the proportion of red in the combination which we wish to be most like the red. For our orange-red we may use 70 per cent. of red and 30 per cent. of orange, and for our red-orange 59 per cent. of red and 41 per cent. of orange. By making a scale of values for the six standard hues, we may combine them in the manner we have just illustrated and form two hues between each of the standards, and two more by combining red and violet. These twelve hues, with the six standards, give a sufficiently large variety of hues for practical purposes.

For purposes of colour education, however, it must be borne in mind that pure spectrum colours are not often seen either in nature or art. And while it is very important that the student should be taught the spectrum colours at the outset of his education in order to establish some accurate knowledge, derived from the only source of accuracy, the solar spectrum, it is also important that he should become familiar with the effect produced by the mingling of these spectrum hues with the light reflected from other objects, as well as the effect of shadow upon the colours themselves. The mingling of white light with any colour produces a tint of that colour. The tints are what we most often see in all except the most brilliant colours of flowers, not generally of the standards but the intermediate hues. On the other hand, when a coloured object is seen in shadow, or, what is more common, when the coloured surface is so irregular as to reflect here colour and there give no reflection, the effect is to produce a shade of the colour. In foliage the prevalence of shades is the rule, whether we consider the individual leaves or the masses of foliage. A knowledge of these effects is best acquired by the use of a very few tints and shades of each hue. Any convenient number of tints and shades can of course be designated, but a few will serve all the purposes of ordinary educational work. In their educational papers the Milton Bradley Company use the six spectrum standards, twelve intermediate hues, including the combinations of red and violet, two tints and two shades of each of the pure colours, thus giving in all a range of ninety different modifications of colour. With these are used black and white, together with a variety of greys. The facility with which young children learn to distinguish and designate colour is really quite surprising.

But a still larger proportion of the colour effects of nature and art than those produced from either tints or shades are the result of both light and shadow combined with colour. This effect has been well enough described by the term "broken colour." In order to acquire familiarity with this effect, it is desirable to use a series of broken standards, if not also of the twelve intermediate hues. These should be made, as should also the tints and shades, by using proportions which take into account the value of the colours, and, above all, the proportions of white and black used should be such as to avoid destroying the characteristic effect of the colours. Each of these broken standards may have its tints and shades like the standards themselves by increasing the amount of white or black which is combined with the colour.

But perhaps the most interesting point in connection with the introduction of definite colour standards will be the possibility of talking about colour in a definite language. Without such standards this has been impossible. By the use of the Maxwell discs made in the standard colours we may easily determine the composition of any colour. This is a great convenience in the description of colours, for it renders it possible when it is necessary to give an exact meaning to any colour term. Only with standards which can be accurately fixed is this possible. The use of such terms as vermilion, emerald green, ultramarine, chrome yellow, and similar terms as a basis of colour analysis is exceedingly impracticable, since even these terms, although by far the most definite terms in common use, are quite too variable to give results which can be of any real value. For the sake of convenience, the first letter of each colour is used as the symbol of the colour in all formulæ in which the analysis or composition of colour is expressed. N is used for black, to avoid the repetition of B which is used for blue.

The following formulæ will illustrate the practical application of the idea and the value of the spectrum standards in determining the composition of colours. They will also be of interest as

showing the simplicity of the proposed nomenclature and method of expressing the results of analysis.

The first series illustrates the variability of the pigments used by artists. These analyses are made by Mr. Bradley.

A Winsor and Newton "cinnabar green" gives—Y 14, G 11½, N 74½.

A German pigment of the same name gives—Y 12½, G 11, W 2, N 74½.

A Winsor and Newton "light red" gives—O 24, N 76.

A German pigment of the same name gives—O 18, N 82.

A Winsor and Newton "chrome yellow" gives—O 29, Y 71.

A German pigment of the same name gives—O 35, Y 45, N 20.

A Chinese vermilion gives—R 77, O 23.

A yellow ochre gives—O 24, Y 24, N 52.

An Indian red gives—R 7½, O 17½, N 75.

An emerald green gives—G 63, B 14½, N 22½.

One called "chrome green" No. 2 gives—G 16½, Y 55, N 78½.

The following series illustrates the significance of the terms used in describing the colours of dress goods. A very wide range of tints and shades of the colour which is the basis of each term will often be designated by the same name.

A sample of goods called "écru" is—O 11, Y 13, W 18, N 58.

Another sample marked "raisin" gives—R 18, Y 14, W 5, N 63.

A sample called "ashes of roses" gives—R 8, Y 4, W 14, N 74.

The popular colour called "eminence" gives—R 14, Y 19, N 67.

Another popular colour called "emerald" is—G 21, B 3, N 76.

A sample called "crushed strawberry" gives—R 55, O 5, W 27, N 11.

One having the poetic name "absinthe" gives—Y 35, G 45½, N 19½.

Another called "Marion" gives—R 4, O 3, N 93.

A specimen of "hussar blue" gives this—G 4, B 15, N 81.

A sample called "oasis" gives the formula—Y 7, G 10½, W 8½, N 74.

Another called "dove colour" gives—B 9, W 9, N 82.

Still another, called "prairie," gives—Y 10½, G 14½, N 75.

A colour called "Styx" has this formula—R 9½, W 21½, N 69.

A sample of "peacock blue" gives this—G 4½, B 8½, N 87.

A brown, called "vidette," gives this—O 4½, Y 3, N 92½.

A sample of "navy blue" gives—B 6, N 94.

Another of "Turkey red" gives—R 98, O 2.

A rather dark "plum colour" gives—R 3, Y 4, N 93.

A few analyses of flowers will be of interest to others beside the botanist.

The Fringed Polygala (*P. paucifolia*) is—R 48, Y 52.

The Wistaria (*W. frutescens*) gives—for the wings R 11, Y 89; and for the standard R 9, Y 79, W 12.

The Flowering Quince (*Cydonia japonica*) gives—R 95, Y 2, W 3.

The wild Cranesbill (*Geraneum maculatum*) gives—R 28, V 66, W 6.

The Fosythia (*F. viridissima*) is pure spectrum yellow.

The variations of foliage are worthy of note, and a few examples of analyses of the colour of various leaves will perhaps be of interest.

It is possible that some knowledge of these variations on the part of more of our artists might save us some of the abominable greens which so often appear in paintings, otherwise of an excellent grade.

Leaves of the White Oak give—Y 7½, G 11½, N 81.
,, ,, Apple are—Y 5, G 13, W 2, N 80.
,, ,, Copper Beech give—R 17, Y 2, N 81.
,, ,, Hemlock Spruce—Y 2, G 9, N 89.
,, ,, White Pine give—Y 2½, G 11, N 86½.
,, ,, White Birch give—Y 5½, G 11½, W 1, N 82.
,, ,, Hornbeam—Y 5½, G 12½, N 82.
,, ,, Shagbark Hickory—Y 4½, G 9½, N 86.

With discs made in the spectrum standards colour can thus be analysed and the results, expressed as in the examples just given, can be utilised by any number of persons to determine the particular colour about which a statement is made. As these discs are not expensive, and the means of rotating them very simple, they ought to come into very general use. It is only necessary that they be rotated with sufficient rapidity to cause the colours to blend smoothly. For the purposes of studying the harmony and contrast of colour it is desirable to have discs of several sizes, so that two or three combinations of colour may be made upon the colour-wheel at the same time and compared.

Among the practical applications of such a scheme of spectrum standards as that outlined in the preceding paragraphs, some of the most obvious are the only ones which need be mentioned in this connection.

A firm dealing in large quantities of coloured material desires to order a stock in a particular colour which they have not used, and of which they have therefore no samples. By the old method they must find something as nearly like what is desired as possible, and then dictate as best they can just what variations are to be made. Now they can produce the colour with the discs and send the formula only to their manufacturer, who also has a set of the discs, and he "sets up the colour" and then reproduces it in the material desired. The gain is great in several ways. In the first place it saves the dealer much costly experiment to determine just what he really wants. Again, if he is in doubt as to just what a customer wants, he takes him to his colour wheel and ascertains what the desired colour is, and then communicates it to the manufacturer. The architect may spend much time and effort to have his carefully-planned and beautiful villa painted in colours which will be at once in keeping with the style of architecture and the surroundings of the building; but unless he confine himself to colours ready prepared and of certain composition, he is liable to extreme disappointment. A similar use of the colour wheel with standard discs would greatly reduce his difficulties. The artist who accustoms himself to the analysis of colour effects will soon find that he is able to write estimated formulæ which will be of service to him in the subsequent composition of his observations. Above all, the child who is thoroughly educated in any scheme of colours which has a definite basis, and consists of a well-selected series of standards, is starting with a most valuable groundwork for future knowledge and practice. Hence it is that the introduction of systematic colour work into the kindergarten and primary school has so much of encouragement to those who desire a reformation in the use of the terms which describe colour perceptions. Why may we not hope for the time when a system of colour terms with something of the same definiteness as those used in music shall be in common use? Surely there is need of this, and the time is not far distant when this need will so assert itself as to bring about a revolution in our methods of colour education.

Malden, Mass., U.S.A. J. H. PILLSBURY.

Globular Lightning.

ON June 21, about 6 p.m., Dr. Wallis, Mr. Taylor and myself were in our drawing-room on the ground floor, taking shelter from a passing storm; they were seated, and I stood five paces from them. The doors were all closed against the storm, and I went out and, for cool air, opened one. On returning, I saw a globular light, about the size of the full moon, in the air between Wallis and Taylor, and almost instantly I heard in the room a terrific clap of thunder like a cannon. I suffered afterwards from acute pain down the left side of my face. Taylor, who had an iron-headed golf stick in his hand, felt a twinge up his right arm, and a sensation as of singeing in his hair. Wallis felt nothing at all. We all experienced a sulphurous smell. In the adjoining room, leaning against one corner, were two Martini-Henry rifles in leather cases. One was untouched. The stock of the other was almost shattered, splinters lying about the room. The leather covering of the splintered rifle was torn, but the metal part of the rifle quite unhurt. At the point of the wall where the muzzle of the shattered rifle touched the wall, there was a hole 5 × 2½ and 1½ to 2 inches deep. The wall is of mud and plaster. In the room above were two holes in one wall; that is, the wall above that in which the hole appeared below. These holes were smaller than the one below. Just below the two holes stood a wooden case, iron-bound, and at its foot the matting was torn up, but the floor and the case were untouched. In the second room above, that is, the room over that in which I had seen the globular lightning, the wall near the ceiling was cracked for six or eight feet. This was all the damage done that we could find. G. M. RYAN.

Karachi, July 18.

[The above letter was received from Mr. F. C. Constable, who saw the damage described.—ED. NATURE.]

RECENT STUDIES ON DIPHTHERIA.

It is an acknowledged fact that as regards diphtheria, personal predisposition on the part of its victims plays a most important part.

We find this well illustrated by statistics which show that it is in early childhood that the majority of cases occur, and the heaviest diphtheria death-rate is recorded. Thus Feer in Basel found that the most susceptible age to diphtheria lies between the years 2 and 5 and 5 and 10; but that whilst the mortality amongst children attacked in the *earlier* period was 25·4 per cent., in the *later* period, with practically no diminution in the number of cases, the diphtheria death-rate fell to 7·6 per cent. After this period there is not only a great decline in the number of cases of diphtheria, but also a marked decrease in the percentage of deaths, suggesting that with increasing age the human system is enabled gradually to develop means of protection from this terrible disease.

That some such protective power must also be possessed to a large extent by children, follows from the fact that with a disease practically endemic in some of our large cities, so many children succeed in escaping from its ravages, for it is impossible to conceive that all those who have remained unscathed have never been exposed to infection from diphtheria.

Thus Flügge has worked out an interesting diphtheria-table for the city of Breslau during the years 1886–1890, in which he not only confirms Feer's observations upon the connection between age and the diphtheria death-rate, but he also shows very clearly that even in the most susceptible period of child-life, the number of cases of diphtheria is relatively small when compared with the number of children of the same age who are not attacked.

In what does this protective power against diphtheria infection possessed by many children and a large number of adults consist? This interesting and important question Dr. Wassermann has recently endeavoured to answer by making a very extensive examination of the properties possessed by the blood serum derived from patients not suffering from diphtheria, but admitted on other grounds to the Berlin Institute for Infectious Diseases. Careful inquiries were, moreover, in every case made as to the patient's previous history as regards diphtheria, and only those were included in the investigation who had never had diphtheria.

The serum which was obtained from these strangers to diphtheria was in every case tested for its immunising or protective power by inoculating it along with a recognised lethal dose of diphtheria toxin into guinea-pigs, the latter by itself having been proved capable of killing these animals without exception in from 30 to 48 hours.

The results obtained were extremely interesting. Out of seventeen children varying in age from 1½ to 11 years, eleven yielded serum with highly protective properties as regards diphtheria, for all the animals treated with their serum and virulent diphtheria toxin experienced no ill-effects whatever. Two out of the seventeen children yielded serum possessed of slightly protective power, it being found capable of delaying the death of the infected animals, whilst the serum derived from the four remaining children had no protective properties whatever.

Amongst the adults the number of those yielding an anti-toxic serum was much greater, for out of thirty-four individuals the serum of as many as twenty-eight was found to be endowed with protective properties against diphtheria infection; and, as far as the investigation went, it appeared that the possession of such serum, as well as its strength or degree of efficiency, was more marked with increasing age.

That people who have gone through the ordeal of diphtheria possess such antitoxic serum in their system has been shown by various investigators, but, so far as we know, Wassermann is the first who has proved that anti-diphtheritic serum may also be possessed by individuals who have had no previous experience of diphtheria.

This discovery serves to explain how virulent diphtheria bacilli may be present in the throat of perfectly healthy people, without producing any bad results at all. That such may be the case has been proved by most careful and trustworthy observers, and that their presence does not engender diphtheria, we must now regard as probably due to the possession of anti-diphtheritic serum by the individual who so unconsciously has harboured them. Such may also be, and probably is, the explanation of the harmless presence of virulent diphtheria bacilli in the throats of patients convalescent from diphtheria long after the disappearance of all the typical symptoms.

It does not follow, however, that because at some given time a particular individual has been found the happy possessor of anti-toxic serum he may, therefore, rashly assume that he is for ever after proof against diphtheria infection.

It must be remembered that such serum is possessed in very different degrees of strength by different individuals, and may vary also, in one and the same individual, in its protective character at different times.

Research has shown that people possessing only feebly antitoxic serum can contract diphtheria, but in the majority of such cases it is satisfactory to learn that the symptoms are light, and the disease is mastered without much difficulty.

So far as our present knowledge goes, it would appear reasonable to admit that although the possession or non-possession of antitoxic serum of varying degrees of strength may not be the only circumstance which regulates the fluctuating personal disposition towards diphtheria infection, that yet it may be regarded as an important factor, and Wassermann considers principal cause, in determining the apparent idiosyncracies of diphtheria infection. What the mechanism may be whereby this anti-toxic serum is produced in the system is still a mystery; that it should be possessed by infants only eighteen months old, would incline to the belief that it is natural or inborn, and not subject to later processes of evolution.

On the other hand, however, we have the well-established fact that the serum of animals which have a natural or race immunity to a particular disease, is wholly devoid of power to confer protection from this disease on other classes of animals.

This remarkable circumstance has been once more very clearly demonstrated by Wassermann in the case of diphtheria, to which disease white rats are absolutely immune. In order to test the character of white-rat-serum as regards diphtheria infection, fatal doses of diphtheria toxin were administered to guinea-pigs along with such serum, but in no single case did the latter survive, showing that this serum possessed no anti-diphtheritic properties whatever, and was incapable of protecting animals from diphtheria infection.

Thus, on the one hand, we find that natural or race immunity to a particular disease does not provide protective serum against infection from that disease in other animals, and, on the other hand, that the serum of individuals who have never had diphtheria, does provide in many cases such protective serum.

Now Wassermann argues from these facts that the possession of protective human serum is not natural or born with the individual; for otherwise, as in the case of white-rat-serum, it would be incapable of conferring immunity, that it must therefore rather be regarded as a later acquisition, and subject to evolution processes.

In pursuing this line of reasoning, Wassermann assumes that race immunity found to be characteristic of a particular description of animal is necessarily of the same character as exceptional immunity confined to particular

individuals of a race. In the one case it belongs to the whole race, whilst in the other it is possessed by only particularly fortunate individuals of a race.

Does not this point rather to the operation of exceptional circumstances, in which, possibly, heredity may play a part? How is it that whereas some families appear to have a faculty for contracting every zymotic disease, others exposed to similar conditions, have an equally characteristic faculty for escaping such diseases?

The impression is irresistible that such a faculty is born with or natural to the individual.

It may be argued that the white-rat-race-immunity may also be ascribed to the operation of heredity. This is quite possible, but in the one case the immunity is perfected or heredity has accomplished its work, whilst in the other it is incomplete and is still in an evolutionary stage. The race immunity to diphtheria, or immunity in its perfected condition, is evidently of a different order, and may also very possibly have been developed on quite different lines, from that which we have been discussing in the human subject. In what this difference consists is at present unknown, and until we have a more intimate understanding of the actual condition in the system upon which immunity depends, or a closer insight into the particular agents responsible for its production we cannot hope to arrive at any definite conclusion.

There is, however, another obstacle to a logical acceptance of Wassermann's arguments as to the origin of protective diphtheritic serum in the human system, that is to say, in the light of our present knowledge, for it entails the supposition that such individuals have been subjected to the action of diphtheria bacilli. This supposition is the logical outcome of the bacteriological evidence which is at our present command on this subject. Thus it has been found, over and over again, that the serum of animals artificially rendered immune to a particular disease, is only efficacious in affording protection to other animals infected with *identically the same microbial disease*. This has quite recently been carefully worked out by Pfeiffer, who has shown that the serum of horses rendered immune to cholera is only efficacious in cases of infection from the cholera vibrio, and that it is absolutely inoperative in protecting from an infection due to any other vibrio, however nearly the latter may resemble that of the cholera vibrio.

But we have seen that protective serum may be possessed by individuals who have never had diphtheria, on whom, moreover, careful investigation has not been able to reveal the invariable presence of true diphtheria bacilli. So far it must be acknowledged, then, that we have no working hypothesis which enables us to comprehend aright the circumstances which determine the presence of or control the generation of anti-diphtheritic serum in the human system, and we are therefore powerless to either stimulate or diminish its production; but we are, however, in a position to regulate, to a great extent, the dissemination of diphtheria virus from one individual to another.

It has recently been shown that children taken from diphtheria surroundings, and not themselves suffering from the disease, in a large number of cases carry about with them in their nasal and throat passages typical virulent diphtheria bacilli, and that although they do not necessarily themselves develop the disease, they thus become the dangerous carriers of infection.

It is considered essential, therefore, that no member of a family where diphtheria has occurred, should be allowed to mix with others until a bacteriological examination has shown that diphtheria bacilli are absent from the air passages, neither are those who have recovered from this disease to be permitted to resume their usual occupations until the absence of diphtheria bacilli has been conclusively proved.

In Germany such systematic examinations are rapidly gaining ground, and already in some of the hygienic institutes the practice is regularly carried out. Indeed, in Königsberg, von Esmarch has suggested that to facilitate the universal adoption of such precautions, the throat of the patient or suspect should be wiped with a sterile sponge, and the latter forwarded for bacteriological examination.

The causes at present at work contributing to the generation of diphtheria in London have yet to be found.

If the contraction of diphtheria primarily depends upon the presence or absence of anti-toxic serum in the human system, then it would appear that some causes are at work tending to deprive the individual of the capacity to generate this means of protection.

It is difficult to conceive, and hard to realise, that the advance in sanitary science and improved hygienic conditions of the present day have but resulted in London in increased facilities for generating and distributing the virus of diphtheria, and that so far we have proved ourselves hopelessly unable to fathom this problem, or to stay the progress of this terrible malady.

REPORT OF THE COMMITTEE APPOINTED BY THE SMITHSONIAN INSTITUTION TO AWARD THE HODGKINS FUND PRIZES.[1]

THE Committee of Award for the Hodgkins prizes of the Smithsonian Institution has completed its examination of the two hundred and eighteen papers submitted in competition by contestants.

The Committee is composed of the following members Dr. S. P. Langley, Chairman, *ex-officio*; Dr. G. Brown Goode, appointed by the Secretary of the Smithsonian Institution; Assistant Surgeon-General John S. Billings, by the President of the National Academy of Sciences; Prof. M. W. Harrington, by the President of the American Association for the Advancement of Science. The Foreign Advisory Committee, as first constituted, was represented by M. J. Janssen, Prof. T. H. Huxley, and Prof. von Helmholtz; and after the recent loss of the latter, Dr. W. von Bezold was added. After consultation with these eminent men the Committee decided as follows:

First prize, of ten thousand dollars, for a treatise embodying some new and important discoveries in regard to the nature or properties of atmospheric air, to Lord Rayleigh, of London, and Prof. William Ramsay, of the University College, London, for the discovery of argon, a new element of the atmosphere.

The second prize, of two thousand dollars, is not awarded, owing to the failure of any contestant to comply strictly with the terms of the offer.

The third prize, of one thousand dollars, to Dr. Henry de Varigny, of Paris, for the best popular treatise upon atmospheric air, its properties and relationships. Dr. de Varigny's essay is entitled "L'Air et la Vie."

(Signed), S. P. Langley,
G. Brown Goode,
John S. Billings,
M. W. Harrington.

August 9, 1895.

Supplementary Report of the Committee Appointed by the Smithsonian Institution to Award the Hodgkins Fund Prizes.

After having performed the function to which the Committee was called, as announced by the circular of the Secretary of the Smithsonian Institution, dated March 31, 1893, which function did not include the award of any medals, there remained several papers to which the

[1] Communicated by Dr. S. P. Langley, Secretary Smithsonian Institution.

Committee had been unable to give any prize, and to which they had felt desirous to give some honourable mention, and on their representing this to the Smithsonian Institution, they had been commissioned to do so, and also to give certain medals of silver and bronze which had been subsequently placed at their disposition.

The Committee has decided that honourable mention should be made of the papers, twenty-one in number, included in the following list, which also gives the full names, titles, and addresses of the authors, and the mottoes or pseudonyms which in four instances were employed. To three of the papers a silver medal is awarded, and to six a bronze medal.

Honourable Mention with Silver Medal.

Mr. A. L. Herrera and Dr. Vergara Lopez, of the city of Mexico: "La Atmosfera de las altitudes y el bienstar del hombre."

Mr. C. L. Madsen ("Geo"), Helsigör, near Copenhagen, Denmark.

Mr. F. A. R. Russell, of London, Vice-President of the Royal Meteorological Society of Great Britain: "The Atmosphere in Relation to Human Life and 'Health.'"

Honourable Mention with Bronze Medal.

Mr. E. Deberaux-Dex and Mr. Maurice Dibos ("Spes"), of Rouen, France: "Études des courents aériens continentaux et de leur utilization par des párostats long-courriers."

Dr. O. Jesse, of Berlin, "Die leuchtendon Nachtwolken."

Dr. A. Loewy, of Berlin: "Untersuchungen über die Respiration und cirkulation unter verdünnter und verdichteter Sauerstoffarmer und sauerstoffreicher Luft."

Mr. Alexander McAdie ("Dalgetty"), of Washington: "The known properties of atmospheric air considered in their relationships to research in every department of natural science, and the importance of a study of the atmosphere considered in view of those relationships: the proper direction of future research in connection with the imperfections of our knowledge of atmospheric air and the conditions of that knowledge with other sciences."

Mr. Hiram S. Maxim, of Kent, England: "Natural and Artificial Flight."

Dr. Franz Oppenheimer and Dr. Carl Oppenheimer ("E pur si muove"), of Berlin, Germany: "Ueber atmosphärische Luft, ihre Eigenschaften und ihren Zusammenhang mit dem menschlichen Leben."

Honourable Mention.

Mr. E. C. C. Baly, of University College, London: "The decomposition of the two constituents of the atmosphere by means of the passage of the electric spark."

Prof. F. H. Bigelow, of Washington: "Solar and Terrestrial Magnetism and their relation to Meteorology."

Dr. J. B. Cohen, of Yorkshire College, Leeds, England: "The Air of Towns."

Dr. F., J. B. Cordeiro, of Washington:—"Hypsometry."

Prof. Emile Duclaux, of the French Institute, Paris, France: "Sur l'actinométrie atmosphérique et sur la constitution actinique de l'atmosphère."

Prof. Dr. Gieseler, of Bonn, Germany: "Mittlere Tagestemperaturen von Bonn, 1848-88."

Dr. Ludwig Hosvay von Nagy Ilsova, Professor in the Royal Joseph Polytechnic School, Budapest, Hungary: "Ueber den unmittelbar oxydirenden Bestandtheil der Luft."

Dr. A. Magelssen, of Christiania, Norway: "Ueber den Zusammenhang und die Verwandschaft der biologischen, meteorologischen, und kosmischen Erscheinungen."

Dr. A. Marcuse, of the Royal Observatory, Berlin, Germany: "Die atmosphärische Luft."

Prof. C. Nees, of the Polytechnic School, Copenhagen, Denmark: "The Use of Kites and Chained Air-balloons for observing the Velocity of Winds, etc."

Surgeon Charles Smart, of Washington: "An Essay on the Properties, Constitution and Impurities of Atmospheric Air, in relation to the promotion of Health and Longevity."

Dr. F. Viault, of the Faculty of Medicine, Bordeaux, France: "Découverte d'une nouvelle et importante propriété physiologique de l'Air atmosphérique (action hématogene de l'air raréfié)."

(Signed), S. P. LANGLEY,
G. BROWN GOODE,
JOHN S. BILLINGS,
M. W. HARRINGTON.

August 9, 1895.

THE PERSEIDS OF 1895.

THE conditions have been very unfavourable for the observation of this meteoric display. The moon's presence in the firmament overpowered the smaller meteors, and unfortunately the weather was very unsettled, the first half of August being notable for its frequent rains and clouded skies.

It was intended to obtain some observations at the end of July before moonlight interfered, but the attempt failed at several stations. On July 25, however, Prof. A. S. Herschel, at Slough, availed himself of a pretty clear interval between 11h. and 12h. 40m. to watch for Aquarids and early Perseids. He found meteors rather bright and plentiful, and the chief radiants in Cassiopeia, Camelopardus, Perseus, Aquarius, and Capricornus. At 11h. 32½m. an Aquarid brighter than Jupiter was recorded in a position a few degrees north of the head of Draco, and at 11h. 55m. a bright Capricornid, equal to Jupiter, traversed a long slow course from the north-east region of Cassiopeia.

On August 2, Mr. E. R. Blakeley, of Dewsbury, watched the sky from 11½h. to 14½h., and observed thirty-one meteors, of which seventeen, or slightly more than one-half, were Perseids with a radiant about 3° in diameter at $35\frac{1}{2}^\circ + 52^\circ$. Mr. Blakeley regards the declination as rather uncertain; it is probably 3° S. of the real position. The brightest meteors seen were Perseids; very fine ones were noted at 13h. 33m. and 13h. 45m.

On August 7, between 10h. and 12½h., some meteors were observed at Slough, Bridgwater, and Bristol. Prof. Herschel at the former place found them very scarce, however, for though the sky was quite clear from 10h. 50m. to 12h. only four meteors were detected. Mr. Corder, at Bridgwater, noted twelve in a watch of 2½ hours. Three or four of the paths indicated a good radiant at η Persei, but others seemed to come from just below γ. At Bristol the writer recorded seven meteors in 1¾h., and of these five were Perseids with a radiant at $41^\circ + 57^\circ$, which agrees with the usual position on August 7. Three meteors were observed at more than one station, and the particulars are as follows:

10h. 12m.—A swift, streak-leaving meteor of 2-3 magnitude observed at Bridgwater and Bristol. Height at beginning 43 miles over Bromyard, Hereford, and it disappeared at an elevation of 28 miles near Crickhowell, Brecon. The real length of path was 42 miles, and the earth-point at Barnstaple, Devon. The radiant was at $45^\circ + 47^\circ$, so that it was not a true Perseid, but a member of a well-known contemporary shower near α Persei.

11h. 4m.—A fine moderately swift meteor variously estimated as first magnitude, equal to α Lyræ, and Jupiter by observers at Bridgwater, Slough and Bristol respectively. Height at beginning 74 miles, at end 45 miles. The meteor passed from above Newport, Mon., to Gellygaer, Glam. Real length of path 33 miles. Earth-point 5 miles north of Pontardawe. Radiant at $333^\circ + 36^\circ$ in the south region of Lacerta.

11h. 29m.—A swift, streak-leaving meteor of second magnitude observed at Bridgwater and Bristol. Height at beginning 105 miles over Stratford-on-Avon, at end 63 miles over Oldbury-on-Severn. Real length of path 64 miles. Earth-point near Chumleigh, Devon. Radiant at $38^\circ + 57^\circ$, so that the meteor was a true Perseid.

On August 9, Mr. Corder, at Bridgwater, watched from 10h. 34m. to 13h. 45m., and saw about 30 meteors, nearly all of which were Perseids. He found the radiant indefinitely marked. A certain proportion of the meteors

observed agreed with a centre at 43 + 57°, but others were directed from η Persei, and others again from the cluster at χ Persei. On August 10 the writer, at Bristol, watched the eastern sky from 13h. 46m. to 15h. 17m., and saw 19 meteors, of which 17 were Perseids from a well-defined radiant at 45° + 55°. This is about 2° S. of the correct place. More meteors would have been seen but for the interference of passing clouds.

On August 11, between 10h. and 11h. at Bristol, 11 meteors were observed, including 7 Perseids with radiant at 44° + 58°. Clouds were again very prevalent, and greatly restricted the view.

On the same night, Prof. Herschel, at Slough, had a clear sky from 9h. 50m. to 12h., and mapped twenty-six meteors, a great majority of them being Perseids. Many of the meteors were bright, and Prof. Herschel regarded the maximum frequency as occurring on this date. "Besides Perseids, a few bright meteors diverged from Pegasus, Pisces, and the head of the Lynx. A pseudo radiant probably of the Perseids presented itself at 46° + 63½°. But the body of the Perseid radiation is very scattered—only the tail end of the shower being here recorded very likely—and a large area enclosing γ, τ, η, χ Persei and H, B, C, D Camelopardi, with its centre at about 43° + 58°, near k Persei, is the best approximation that can be gathered from the tracks registered."

A fourth magnitude meteor, moving swiftly, was seen at 10h. 7m. both at Slough and Bristol. Height at beginning, 78 miles; at end, 62 miles. It passed from over Brackley (Northampton) to Farringdon (Berks). Real length of path, 30 miles; earth-point, 10 miles south-west of Portland, Dorset. The radiant was at 48° + 60°, the meteor being a true Perseid.

From the various reports already received, it appears certain that this year's display has been far from presenting a conspicuous character. This has probably not proceeded from any special weakness in the shower itself, but from the unsuitable circumstances which have attended its return. Moonlight is a most serious obstacle in the way of meteoric work, and when, added to this, the observer is confronted with skies more or less clouded, the chances of success become very remote. But, in spite of these untoward conditions, the shower has by no means passed unobserved; many of its brilliant meteors have been recorded, and the radiant point has been determined on several nights. Some of the chief contemporary systems have made their presence known by some fine objects, and the results on the whole may be regarded as very satisfactory.

W. F. DENNING.

SIR JOHN TOMES, F.R.S.

ANOTHER of the small band of histologists, who took up the subject when the field was almost untrodden, has passed away, at the age of eighty.

Sir John Tomes, after serving an apprenticeship to a medical man at Evesham, came to London in 1836, and entered at King's College and at the Middlesex Hospital, being at the former a class-mate with the late Sir William Bowman, with whom a life-long friendship thus began.

For two years (1839-40) he resided in the Middlesex Hospital as house-surgeon; and even at this early stage in his career his attention became turned towards the histology of bone and teeth, and we find him feeding a nest of young sparrows and a sucking-pig upon madder. From a somewhat fragmentary diary which he kept, we find, too, that he then bought from Powell (afterwards Powell and Leland) a microscope, and that he was often spending his evenings with Bowman, Quekett, Kiernan, Todd, Carpenter, and Edward Forbes.

He was an early member of the Microscopical Society, and over a long series of years his contributions to the histology of the hard tissues were numerous. Amongst his more important papers in the *Phil. Trans.* were those on bone (in conjunction with the late Campbell de Morgan), on the dental tissues of marsupials, of rodents, and upon the structure of dentine, this last establishing the existence in dentine of the soft fibrils, ever since known as "Tomes' fibrils."

Like that of his friend Bowman, almost all of his work has stood the test of time, and to this day remains undisturbed. A strong bent towards mechanical invention led him, while still house-surgeon, to revolutionise the construction of tooth forceps, which thenceforward supplanted the old "key" instrument; and at the advice of the late Sir Thomas Watson, he determined to devote himself to the practice of dental surgery, in which the busiest years of his life were spent.

Dr. Morton, a dentist of Boston, Mass., having introduced the use of ether in 1846, we find from Sir John's diary that he was early in the field as an experimenter with this anaesthetic. After sundry experiences with it for tooth extraction at the Middlesex Hospital, some successful and some not, we read: "Gave ether to Arnott's case of lithotomy eight minutes, and insensibility came—the operation then commenced and lasted twelve minutes." (Jan. 14, 1847.) And after notes of many administrations: "Gave ether to eight patients for operations with great success. Earl of Cadogan (a governor of the hospital) and many others present." (Feb. 23, 1847.)

His lectures on dental physiology and surgery were perhaps the first in which the subject was treated from a true scientific standpoint, and when published became quite a classic. But it is curious to read in his diary a resolve that he really will not deliver any more lectures unless he has a class of at least six students.

In 1883 the College of Surgeons, exercising their right to confer honorary fellowships of the College, elected Sir John Tomes and the late Prof. Huxley.

In 1886 he obtained the honour of knighthood, in recognition of his great services to the cause of dental education, and to the establishment of a dental diploma and its recognition by Parliament, his unbroken success in all that he undertook being largely due to his excellent business capacity, and to the respect, trust, and liking which he inspired in all with whom he came in contact.

NOTES.

WE understand that a Civil List pension of £200 has been granted to Mrs. Huxley.

THE following have been elected Associates and Correspondents of the Reale Accademia dei Lincei:—National Associates, Prof. L. Luciani and Prof. G. Tizzoni; Correspondents, Prof. E. Cesàro, Prof. A. Ricco, and Prof. Carlo de Stefani; Foreign Associates in Mathematics, Prof. C. Jordan and Dr. G. Salmon; in Astronomy, Prof. Simon Newcomb; in Physics, Prof. H. J. Wild; in Morphology, Prof. A. Kolliker.

The following are among the recent appointments abroad:—Dr. R. Behrend to be Professor of Chemistry in the Technische Hochschule of Hanover; Dr. X. Siefert to be Professor of Forestry at the Technische Hochschule of Karlsruhe; Dr. F. Richarz to be Professor of Physics in the University of Griefswald; Dr. P. Stackel to be Assistant Professor of Mathematics in Konigsberg University; Dr. O. Wiener to be Professor of Physics in the University of Giessen.

REUTER'S correspondent at Wellington reports that a severe earthquake shock was felt at Taupo, in the district of Tauranga, and at some other places in New Zealand, on Saturday last. An earthquake was also felt over the greater part of Peru, but principally in the south, on Monday.

WE learn from *Das Wetter* that the efforts which have been made during the last fifteen years for the re-establishment of a

meteorological observatory on the Brocken, have at last been crowned with success, and, if unforeseen difficulties do not arise, it is expected that this important station will be in working order during the coming autumn. This successful issue is mostly owing to the support given to the undertaking by the Ministry of Public Worship and the Meteorological Institute of Berlin, and by the Brunswick and Hanover sections of the German and Austrian Alpine Club. There can be no doubt that observations from this mountain observatory will be of considerable value for the progress of meteorological science.

As already announced in these columns, the sixty-seventh meeting of German physicians and men of science will take place at Lübeck on September 16 to 21. Members and visitors will be received at the Town Hall on Sunday, the 15th, at 8 p.m. Business will commence on Monday at 11 a.m. in the Gymnastic Hall with a presidential address, followed by some medical papers. At 3 p.m. the sections will be formed, and at 7 p.m. there will be a social gathering at the Tivoli. Among the entertainments of the following days, are a garden party given by the Senate of the Free Hansa City of Lübeck on Tuesday, a grand ball in the theatre on Thursday, and an excursion to the lakes of East Holstein on the Saturday. Medical papers are announced by Drs. Klebs, Behring, Riedel, and Rindfleisch, and general scientific papers by Drs. Victor Meyer, Ostwald, and others. Senator Dr. Brehmer and Dr. Theodor Eschenburg are the secretaries of the meeting.

The *Board of Trade Journal* reports that an industrial exhibition, to celebrate the jubilee of the recognition of Berlin as the capital of the German Empire, is to be held next year in the Treptow Park, near that town, from May to October. The exhibition will embrace the following groups:—(1) Textile industries; (2) Clothing industries; (3) Building and engineering; (4) Wood industries (cabinet-making, &c.); (5) Porcelain, glass and fire-brick industry; (6) Smallwares and fancy goods; (7) Metal industry; (8) Engraving, the decorative arts, and the book trades; (9) Chemical industry; (10) Food products (including tobacco, spirits, &c.); (11) Scientific instruments; (12) Musical instruments; (13) Machine-construction, shipbuilding, and transport trade; (14) Applied electricity; (15) Leather and india-rubber industry; (16) Paper industry; (17) Photography; (18) Hygiene, and sanitary dwellings; (19) Education and instruction; (20) Fishing and boating, as industries and sports; (21) Riding and racing, aquatic sport; cycling, shooting and hunting, pleasure-boating; (22) Horticulture; (23 German colonial exhibition; (24) Hotel and restaurant trades.

The Council of the Federated Institution of Mining Engineers have had for some time under their consideration the holding of meetings of the student members, and the first meeting of students was successfully held in the North of England district on August 13, 14, and 15. With a view to interest the students more especially in the proceedings of the first meeting, a prize was offered by the Institution for the best essay on "The Prevention of Accidents in Mines." The prize was obtained by Mr. Austin Kirkup, whose essay deals concisely with the commonest forms of mining accidents, and sets forth the results of the experience of practical men on their prevention. Mr. Kirkup has based his facts almost entirely on the knowledge which practical experience and observation have afforded him, so his essay possesses a real value, and we regret that pressure upon our space prevents us from doing more than refer to it. In order that the meeting in connection with which the paper was prepared might be of a thoroughly practical character, the students who took part in the proceedings made lengthy underground visits to the Wearmouth and Eppleton Collieries, and were given every information as to the mode of working, haulage, ventilation, &c., practised at these extensive collieries. The Institution is to be congratulated upon its new departure, which is certainly calculated to give the students a wider knowledge of mining than they would otherwise obtain.

We have received the official programme of the prizes offered for 1896 by the Société Industrielle de Mulhouse. A prize of 1250 francs is offered for a complete history of one of the principal branches of Alsatian industry, such as spinning and weaving cotton and wool, printing woollen and cotton fabrics, machinery, &c. The Hubner prize, represented by a *médaille d'honneur* and 1000 francs, is offered for the best memoir on the carding of spun textile materials during the period which has elapsed since the last publication on the subject, or for the improvement which, in the opinion of the Society, shall have contributed most to the development of carding operations. Similar prizes are offered for a substance which, in the coloured cloth industry, can replace the dry albumen of eggs, and is cheaper than this substance; and for a colourless blood albumen which does not colour on steaming. Silver medals and prizes of 500 francs each are offered for a new and simple means of determining the amount of priming in steam boilers; for a new and advantageous mode of constructing buildings suitable for cotton and wool spinning and weaving, or the manufacture of dyed cloth; new and practical researches on the movement and cooling of steam in long conduits; a registering pyrometer for steam boiler fires; a memoir on the spinning of carded wool; and for a complete memoir on the drying of tissues. Besides these prizes, medals of various grades are offered in some 140 subjects connected with chemical and mechanical arts, agriculture, commerce, history, and fine arts. The competitions are international, but it does not appear from the programme whether French is to be the only language permitted. The memoirs, designs, samples, &c., must be marked by a device or motto chosen by the author, and addressed to the President of the Society before February 15, 1896, together with a sealed envelope containing the exact name and address of the competitor.

Mr. T. H. Bickerton pointed out, at the recent meeting of the British Medical Association, that when the inquiry was arranged into the disastrous collision between the *Elbe* and the *Crathie*, it was stated that "the question of the powers of vision will be carefully borne in mind in the Board of Trade inquiry into the cause of the collision." The inquiry has now been concluded, but it appears that the witnesses were not examined as to their eyesight. This act of negligence will need a deal of explaining. The reading of Mr. Bickerton's paper was followed by the adoption, on the proposal of Dr. Farquharson, M.P., of a resolution that the matter should at an early date be brought to the notice of Parliament, which should be asked to insist that adequate tests should be compulsorily applied before a lad is apprenticed to the sea; that the Royal Society's recommendations should be acted on by the Board of Trade in their entirety; and that officers already holding certificates, and now by the institution of adequate tests found colour blind, should have shore berths given them in Government offices.

The morphological place of moulds and yeasts, respectively, has long been the subject of speculation and research, some authorities regarding yeasts as having an independent existence, others considering them as only transitory forms in the life-history of moulds. Most important and interesting contributions to this subject have recently been furnished by the experiments carried on in Dr. Jorgensen's laboratory in Copenhagen. In the course of some researches on the diastatic power of the well-known Japanese mould *Aspergillus oryzae*, Juhler found that in the flasks in which this mycelium had converted rice-starch into sugar, it had produced a growth of typical alcohol

producing saccharomyces cells. This most interesting observation was subsequently confirmed by Jorgensen, who has since endeavoured to ascertain if the various types of alcohol producing yeasts can be traced to particular moulds, and already he has succeeded in demonstrating the evolution of wine yeast cells from a particular mould extensively present on grapes. Dr. Jorgensen intends to continue these most suggestive investigations, and publish his results from time to time in the form of a separate *Bericht* exclusively devoted to the work carried out in his laboratory. In pursuing these researches Dr. Jörgensen will not only render great practical service to the science of fermentation, but he will also lay botanists under deep obligation to him for having rendered possible a more extended and accurate insight into the life-history of moulds.

THE annual address on "The Recent Evolution of Surgery," delivered before the Medical Society of London in May last, by Mr. A. Pearce Gould, has been published in the form of a dainty brochure by Messrs. Kegan Paul and Co.

THE *Transactions* have reached us of the Ballarat meeting (1894) of the Australasian Institute of Mining Engineers. Among the papers contained therein, we notice a review of past and present steam pumping in mines, by Mr. J. Tipping; an address on the mineral wealth of Victoria, by Mr. James Stirling; an account of the physiography and geology of the Wadnaminga Gold Field, by Mr. F. D. Johnson; notes on the White Cliffs Offal Fields, Wilcannia, by Mr. F. G. de V. Gipps; and a description of ore-dressing by automatic machinery, by Mr. H. W. F. Kayser.

WE have received from Dr. G. Hellmann, of Berlin, a revised edition of "Meteorologische Volksbücher," which first appeared in *Himmel und Erde* in 1891 (see NATURE, vol. xliv. p. 185). The work contains an account of the earliest popular German treatises on natural science and meteorology from the first encyclopedia, "Lucidarius," which was written more than two centuries before the invention of printing, to the "Hundred-year Calendar" of Dr. Knauer, for the years 1701 1801. Dr. Hellmann has embellished the work by further biographical notes and additions based upon his laborious researches since the appearance of the first edition.

THE forty-first annual report of the Trustees of the Australian Museum is not a pleasing one. We read: "The continued smallness of the income allowed to the Trustees by Parliament has practically stopped the acquisition of specimens by purchase or collection. The amount expended in the purchase of specimens [during 1894] does not exceed £20. No collecting expeditions have been sent out, all that has been done in this way being confined to flying trips around Sydney. . . . The staff still continues at its reduced strength, and the forced economies of late years are beginning to tell on the efficiency of the Institution." It is really time that something was done to alter this unsatisfactory state of things; for the present conditions hamper the usefulness of the museum, and are most detrimental to the interests of science. A few researches have been carried on by the officers of the museum, and the mention of them gives a little light to an otherwise rather discouraging report.

MESSRS. GEORGE PHILIP AND SON have published a school edition of the "Systematic Atlas." The atlas has been specially arranged for the use of students in higher schools and training colleges. Though an abridgement of the "Systematic Atlas," it contains as many as 170 maps—practically all the general ones—in forty-one plates, and a complete index of more than 12,000 names. The atlas will be very valuable for class work in physical and political geography, and is a useful introduction to the larger edition, which has already been reviewed in these columns.

Another atlas, of which Messrs. Philip have just published a new edition, is the "Handy-Volume Atlas of the World," by Mr. E. G. Ravenstein. This, however, is almost a new work, for the whole of the maps have been re-drawn and re-engraved, and the letterpress accompanying them has been rewritten. We reviewed the original edition when it appeared some years ago; and it is only necessary now to say that the present volume, like its predecessor, is a compact and an efficient pocket-atlas.

THE second part of the fifty-first volume of the *Verhandlungen des Naturhistorischen Vereins der Preussischen Rheinlande, Westfalens und der Reg. Bezirks Osnabrück* (Bonn, 1894), contains six memoirs and a series of shorter papers and notes. The first memoir is a list of the fossils derived from northern regions found in the diluvial deposits of Westphalia, which is contributed by Dr. W. von der Marck. Laspreyes has issued a detailed study of the meteorites in the museum of the University of Bonn, in which the literature is tabulated with great care. Stockfleth describes the iron-ore deposits in the Hill of Huggel, near Osnabruck, where it occurs in the Zechstein. C. Roettgen gives a "Contribution to the Coleoptera Fauna of the Rhine Province." H. Pohlig continues his study of abnormal deer antlers by a description of two pairs belonging to the great Irish elk. One of these has a brow tyne on the left side, but no trace of one on the right, whereas the first of the serial tynes on that side is branched. In the other case both brow tynes are present, but the second serial tyne on the left side has a rudimentary branch. Dr. Verhoff contributes a short paper on the biology of the fire-fly *Phosphaenus hemipterus*. Among the smaller papers, a note by Ludwig gives a brief account of Marchiafava and Celli's work on the malaria parasite; Philippson summarises the geological problems that still await solution in Western Turkey. Schenck gives a brief demonstration of the structure of the Brazilian lianas, or climbing stems.

THE additions to the Zoological Society's Gardens during the past week include a Ruffed Lemur (*Lemur varius*, ♀) from Madagascar, presented by Mr. J. H. Bingham; a Vervet Monkey (*Cercopithecus lalandii*, ♀) from South Africa, presented by Mrs. C. J. Humphrey; a Mozambique Monkey (*Cercopithecus pygerythrus*, ♀) from East Africa, presented by Mrs. John Mahon; a Sooty Mangabey (*Cercocebus fuliginosus*, ♀) from West Africa, presented by Mr. Davies; a Sykes's Monkey (*Cercopithecus albigularis*, ♂) from East Africa, presented by Mr. J. Watkinson Brown; a Cheetah (*Cynælurus jubatus*), a Blotched Genet (*Genetta tigrina*) from Somaliland, presented by Mr. J. L. Harrington; a Martial Hawk Eagle (*Spizaetus bellicosus*) from British East Africa, presented by Captain B. L. Sclater; two Ravens (*Corvus corax*), two Buzzards (*Buteo vulgaris*), two Greater Black-backed Gulls (*Larus marinus*), European, presented by the Hon. William Edwards; a Herring Gull (*Larus argentatus*), British, presented by Mr. George Hawes; two Orbicular Horned Lizards (*Phrynosoma orbiculare*) from Mexico, presented by Mr. Bernard Jackson; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, a Black-backed Jackal (*Canis mesomelas*) from South Africa, four Spiny-tailed Mastigures (*Uromastix acanthinurus*) from North Africa, deposited; two —— Octodons (*Ctenodactylus gundi*) from Egypt, purchased, three Dorcas Gazelles (*Gazella dorcas*, ♀ ♀ ♀) a Sœmmerring's Gazelle (*Gazella sœmmerringi*, ♂), an Egyptian Cat (*Felis chaus*), three Libyan Zorillas (*Ictonyx lybica*), ten Varied Field Rats (*Isomys variegatus*), thirty-five Hairy-footed Jerboas (*Dipus hirtipes*), forty-five Lesser Egyptian Gerbilles (*Gerbillus ægyptius*), eight Larger Egyptian Gerbilles (*Gerbillus pyramidum*), two Egyptian Kites (*Milvus ægyptius*), a Cerastes Viper (*Vipera cerastes*) from Egypt, received in exchange; a Spotted Pigeon (*Columba maculosa*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CŒLOSTAT.—The name *cœlostat* has been given by M. G. Lippmann to a modified form of siderostat which he has devised (*Comptes rendus*, No. 19, 1895, and *Observatory*, August). The special feature of the instrument is that it gets rid of the rotation of the field of view which disqualifies the siderostat for some purposes, such, for instance, as long-exposure photography. It consists simply of a mirror with its plane parallel to the earth's axis, and turning on a polar axis once in forty-eight hours in the same direction as the apparent diurnal motion of the heavens. It is easily demonstrated that the image of any star whatever will be seen stationary in a mirror so mounted, and a telescope pointed at the mirror in any direction will have a constant field of view. The telescope being directed to the cœlostat in a given position, to observe other objects having the same declination as that in view, it will only be necessary to turn the mirror; but for objects with different declinations the telescope must also be moved. If it be desired to use a horizontal telescope, it must be directed to the point on the horizon where the object rises, and the mirror must be started in a position suited to the hour-angle; but there is a limit to the use of a horizontal telescope. It is pointed out that the simplicity of the instrument makes it possible to turn it into one of great precision; stability being readily attained, while the possibility of flexure can be reduced to a minimum.

ADAMS' MASSES OF JUPITER'S SATELLITES.—A question having been recently raised by Mr. Marth as to the work of Adams on Jupiter's satellites, Prof. R. A. Sampson has stated the results of an inspection of the MSS. with reference to this subject (*Observatory*, August). It appears that when engaged upon a revision of Damoiseau's tables in 1875, with a view to their continuation, Prof. Adams determined the following revised values for the masses of the satellites:—

$$m = 0{\cdot}0000283113$$
$$m' = 0{\cdot}0000232355$$
$$m'' = 0{\cdot}0000812453$$
$$m''' = 0{\cdot}0000214880$$

"There is no reason to suppose that Adams attached any weight to the above determinations of the masses, seeing that he never published the values directly; the MS. appears to be little more than a study such as he was in the habit of making upon any work that he was examining, in order to test by cross verifications the accuracy and consistency of the whole. . . . Considerable expectations have been built upon the fact that Adams was engaged more or less closely for some years upon the theory of Jupiter's satellites. It will be well to say at once that the chief fruit of his attention was published in the *Nautical Almanac* of 1880; this, like all the rest of his published work, was the result of exhaustive labour, quite out of relation to the unpretentious form in which the outcome was presented, and only discoverable by searching tests."

ATMOSPHERIC REFRACTION.—The ordinary application of Bessel's expression for refraction requires that five quantities be taken from specially prepared tables, but Prof. E. C. Comstock, Director of the Washburn Observatory, has worked out a simple formula for computing the refraction without the aid of tables. A transformation of Bessel's formula, and the introduction of numerical constants from the Pulkowa refraction tables, leads to the following simplified form:

$$R = [2{\cdot}99215] \frac{BF}{455{\cdot}9 + t} \tan Z$$

$$\log F = -(42{\cdot}3 + 0{\cdot}12t)\tan^2 Z.$$

The number in brackets is a logarithm; B is the barometric pressure in English inches reduced to freezing-point; t is the temperature in degrees Fahrenheit, and Z is the zenith distance for which the refraction is required. The formula for F gives the logarithm in units of the fifth decimal place.

The computation by the formula is not more laborious than the direct use of the tables, and a comparison of the two methods shows that the differences in the results are far less than the uncertainty in the tabular numbers themselves. Prof. Comstock's paper forms one of a series of interesting "Studies in Spherical and Practical Astronomy," in the *Bulletin* of the University of Wisconsin (vol. i. No. 3).

ON THE ORIGIN OF EUROPEAN AND NORTH AMERICAN ANTS.

QUESTIONS belonging to zoogeography may be practical or theoretical, actual or genetic; ultimately the resolution of them, whatever they may be, takes its chief interest from their relations to genetical problems, that is, to the explanation of the origin of actual faunæ, and to the knowledge of the original home of phyletic groups, and of the ways followed in their gradual diffusion over the whole or part of the world. To this purpose, not only living animals, but also fossils, have to be determined, and their affinities exactly worked out; changes in the distribution of land and sea and in the shape of continental areas must be investigated, and analogies and differences in the diffusion of various groups of living beings taken in consideration, as far as they are known. The work involved is long and difficult, and its results will form the science of the future.

In a paper published in 1891, on the fossil ants of Sicilian amber,[1] I made out that at the beginning of the Miocene epoch, North and South Europe had very different faunæ of ants, the Sicilian amber containing genera which belong to the actual Indian and Australian fauna, but wanting the typical holarctic genera *Formica*, *Lasius*, *Myrmica*, which are found in the Baltic amber, some species of them being extremely common and abundant. A similar, but not such a striking, difference exists between recent Mediterranean and North European ants, the former including a greater percentage of Indian and cosmopolite forms, and an absolutely and relatively lesser number of typically holarctic ones, the most species of *Formica*, *Myrmica*, and *Lasius* not having reached Africa (*F. fusca*, L., and *M. scabrinodis*, Nyl., are introduced in gardens in Algeria), and these genera being scarcely represented in Mediterranean islands. After discussing these facts, I came to the conclusion that South Europe should have had in the Tertiary epoch an ant fauna compound of old Mesozoic cosmopolite genera (chiefly Ponerinæ), mixed with Indian-Australian forms. In North Europe these lived together with northern genera, which, after the emergence of the bottom of the middle European sea, invaded the South, being perhaps expelled from the North by gradual cooling of climate. Later, the glacial epoch destroyed in Europe nearly all the rest of tropical insects, their return being made impossible by the natural barriers of sea, deserts, and mountains, accumulated southward and eastward of our continent.

These studies I have carried a step further in a revision, now printed, of the Formicidæ of North America.[2] A great number of North American ants are specifically identical to European ones. My attention was directed to find differences between American and European specimens, and indeed but a few species were so similar to their European relatives as to be not distinguishable as sub-species or varieties. The one genus, *Epœcus* and two sub-genera are exclusively Nearctic; all the other genera of North American ants not represented in Eurasia (*Discothyrea* has two species only, one in North America, another in New Zealand) are Neotropical. The northern regions of Europe has the one peculiar genus *Anergates*, allied to *Epœcus*; middle and south Europe have two further genera not found in other parts of the world, and some others known from the Indian region. All these facts lead to the result, that the Palæarctic ant-fauna is made of cosmopolite + Arctic + Indian elements; that the Nearctic fauna is similarly composed of cosmopolite + Arctic + Neotropical ones.

The question that now arises is: how has such a mixture been effectuated—what changes have determined it? A complete and detailed answer I believe to be at present impossible; but the knowledge of the fossil mammals may help us greatly, supplying for the want of evidence taken from fossil ants, other than the Miocene fauna of European amber, the fossil prints of Formicidæ being too imperfectly known, and a careful revision of the existing collections from a trained specialist wanted. I believe that mammals and ants are both of the same age; their migrations took place by means of the same land connections, with the difference, that winged females of ants could, easier than terrestrial mammals, pass over sea-arms, being carried by winds.

I admit that in the Oligocene epoch, after Australia, Africa and South America had been cut off from a great northern

[1] C. Emery. "Le Formiche dell' Ambra Siciliana nel Museo Mineralogico della R. Università di Bologna." (*Memor. Accad. Bologna* [5], vol. v. 1. 1891).

[2] C. Emery. "Beiträge zur Kenntniss der Nordamerikanischen Ameisenfauna. (*Zoolog. Jahrbücher.* Abth. f. Syst. 7 Bd. pp. 633-682, Taf. 22; 8 Bd. pp. 257-360, Taf. 8. 1893-95)

system of dry land (such a system was rather an extensive archipelago than a continuous continent); this last was again divided into two systems: an Arctic and Occidental one, comprising North America, together with the northern parts of Asia and Europe, and an Indian one, communicating with South Europe. The former was the home of the Cervidæ, the rhinoceroses and most other Perissodactyls, the latter that of the Cavicorns and elephants. Very few mammals of Indian origin migrated into America; much more from the Arctic system into India. The same seems to be the case for ants. *Myrmecina* is perhaps the only North American genus of Indian origin (*Tetramorium cæspitum* being doubtless introduced by man), whereas a number of American-Arctic genera, sub-genera and species-groups, as *Myrmecocystus*, *Messor*, *Myrmica*, *Camponotus pennsylvanicus*, &c., are more or less far diffused in India and Africa, *Myrmica* reaching Borneo, and *Messor* the Cape of Good Hope.

In Europe, the study of the Baltic and Sicilian amber proves that the Arctic fauna went down from the north, as a host of conquerors, invading the territory formerly occupied by other people. I believe that, in Miocene times the North American fauna was much like the actual cosmopolite and Arctic part of the recent fauna, and might have included a number of forms actually extinct. As in the Pliocene a bridge was put between North and South America, an invasion of neotropical forms took place, walking from south to north. But it is not improbable that other forms migrated in the opposite sense, and descended from North America into the neotropical region. I suppose that such was the case for the genus *Pogonomyrmex*, perhaps also for *Dorymyrmex*, *Forelius*, and several species of *Camponotus*. It is not improbable that other genera from North America migrated southward, and later became extinct in their primitive home. The recent work of Mr. Scudder on Tertiary Curculionidæ of North America seems to confirm this view, some of these fossil beetles belonging to genera now living only in South America. It is probable that a number of insects, actually regarded as typical members of the neotropical fauna, immigrated from North America, as it is proved by palæontology for several mammals, as, for instance, the llama and alpacca of the Pampas.

The North American origin of some South American ants was suggested by Prof. H. von Jhering,[1] in a paper published last year. The author endeavours to sustain, by the study of the ants, his theory of the multiple origin of actual neotropical fauna. I agree in many points with him,[2] but I must recognise that the Formicidæ afford but little evidence in favour of his views. Actually, the ants of South America are distributed chiefly in relation to the climate and vegetation, no strong obstacles being put to the wide dissemination of the species, some of which range from Central America or from Mexico to Paraguay and Rio Grande do Sul. Chili is, however, an isolated country, which we may call "a continental island," although it is not surrounded by water. If we should take the Chilian fauna as a standard for the primitive fauna of v. Jhering's Archiplata, that should have been a very poor one, like the fauna of New Zealand, with which it offers a striking resemblance. The most characteristic feature of the Chilian ant fauna is the occurrence of peculiar species of *Monomorium*, like those inhabiting Australia and New Zealand, and of the genus *Melophorus*, found only in Australia and New Zealand. These facts corroborate the hypothesis of a Cretaceous or Eocene connection between South America and Australia.

New Zealand appears as a bit of old Australia, quite free from later Papuan or Indian intrusions, like Madagascar, which, as an isolated part of old Africa, has received but a few immigrants, when, at the Pliocene epoch, a stream of Indian life entered into the Ethiopian continent. Probably Chili may be considered as a part of ancient Archiplata, secured from Guyanean and Brazilian immigrants by the heights of the Cordillera, but having preserved only an incomplete set of the original Archiplatean fauna.

I state these facts for the purpose of making the main conclusions of a special work known to a wide public. Exact knowledge of the exotic faunæ, and especially of the fossils, may enable us in future to carry further these incomplete and in part hypothetical results. Similar studies made on single groups of animals and plants by specialists, which do not only accumulate by blind statistical work names of families, genera, and species, but deal with them, knowing the value of each, are highly desirable. Summarising and integrating the single results will build up an exact knowledge of palæogeography, and of the origins and interrelations of the faunæ and floræ of the world.

C. EMERY.

[1] H. von Jhering, "Die Ameisen von Rio Grande do Sul." (*Berliner entomolog. Zeit.*, Bd. [illegible], pp. [illegible]–446, 1894.)

[2] Other parts of v. Jhering's theories, which I cannot accept, refer chiefly to the [illegible] fauna. In these points I think that W[illegible]

A NEW FILM HOLDER.

NO outdoor photographer can take a rough survey of the past few years without feeling some astonishment at the rapid progress made in nearly every branch of his art. The amateur is no doubt indirectly responsible for much of this advance; for it is through him that other brains have been set to work to satisfy all his many and various wants, in the way of instruments and accessories, to lighten his task at every step.

The camera, which a few years back was a heavy, clumsy and awkward instrument, is now of a light and handy construction, capable of being used in many cases without the tripod. Stops are now more generally of the Iris type, thus eliminating all possibilities of loss or of leaving them behind; while plate-holders are now supplied capable of holding a dozen or more plates, and necessitating the use of only one dark shutter.

The introduction of the film has brought us, however, into a new era; but the full benefit of this improvement can only be best appreciated by those who make use of their cameras while travelling.

Hitherto it has been impossible to make satisfactory use of the enormous advantages of celluloid flat films over glass plates; but now we have before us a holder which seems to give satisfaction, and which should prove a boon to photographers in general.

A holder to be really efficient should be readily adaptable to any ordinary camera; it must contain a large quantity of films,

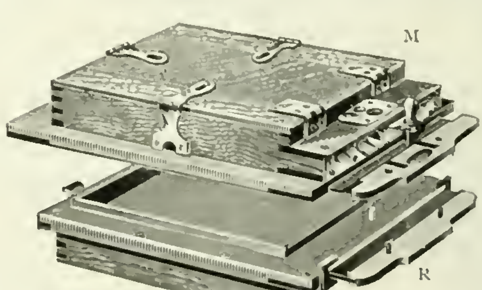


FIG. 1.—Magazine and receiver, separated.

and when complete and loaded should not be any larger or heavier than the three double backs (lighter if possible); and, finally, should be provided with some means of swiftly and automatically changing the positions of the exposed films.

Such a holder, if simple and of moderate price, would be much sought after by the photographic world. A very near approach to such an ideal film-holder will be found in that known as the "Frena," of which a short description follows.

Fig. 1 gives a complete view of the holder (the two parts are here shown separately), ready to be fitted to any camera. It consists of two parts: the magazine (M) and the receiver (R), each of these parts being about half as thick again as an ordinary dark slide. The exposure is made in precisely the same way as with an ordinary dark slide, namely, by inserting the magazine in the slide rails of the camera, and by withdrawing, and subsequently replacing, the shutter of the magazine.

The film changing is brought about simply by folding the magazine and receiver together until they interlock, drawing out the two shutters, pressing a change button to one side, and pushing the shutters back again.

The exposed films, stored in the receiver, may then be removed for development one by one, or as a complete pack, just as the operator desires.

An automatic counter upon the back of the magazine shows at a glance how many pictures have been taken.

The peculiarity of these films is that their edges are notched, and in their packing an alternate sequence is maintained as regards the position of these notches.

The films are supplied ready packed and arranged in the order in which they are to be inserted into the magazine.

To understand more clearly the position of the notches, it is best to take the empty magazine in hand, and entirely withdraw the black exposing shutter. It will then be seen that the front of the magazine is provided along its sides with two series of projecting teeth; it is upon these teeth that the films inserted into the holder are supported. At one end of the magazine, which we shall call the top, is a button; if this button be pushed from one side to the other, this movement will shift all the sorting teeth at the same time, so that they will occupy positions a little to one side of their former ones.

A film introduced into the magazine will then be supported by the sorting teeth, when these stand in the original positions; if this film be put into the holder with its notched corners towards the top end of the magazine. It will, however, fall past the sorting teeth, which pass through its notches, when the change button is moved to one side and the sorting teeth stand in the second position mentioned.

The process of filling the magazine is very simple, for the pressure-board has only to be removed, and the films inserted into the holder with the white film downwards, *i.e.* towards the pointers, and eventually towards the lens. Should there remain in the magazine any unexposed films, with their backings, and it is merely required to add to their number, the additional films with their backings may be dropped into the holder by twos or threes, due care being taken that the alternate arrangement be maintained.

The whole process, although somewhat lengthy to describe, is in itself very simple and neat, and can be at once grasped by an examination of the holder itself in daylight.

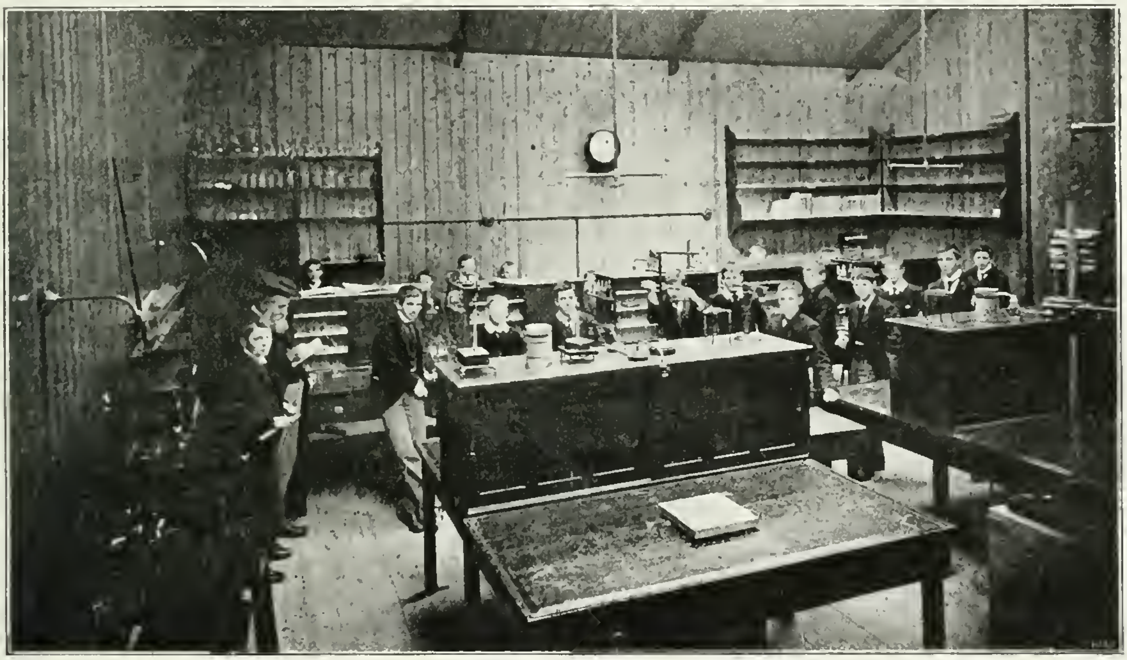
THE NEW NATURAL SCIENCE SCHOOLS AT RUGBY.

THIS new building for the physical part of natural science, which has recently been opened at Rugby School, is well worth a visit from any one engaged in teaching that subject. The building, owing to want of funds, is not at present of a permanent nature, being of the felt and matchboarding type, and in consequence has no pretensions to structural beauty; but when funds are forthcoming, no doubt the whole will be built in brick, and this will enable any alteration or improvement which may then be deemed necessary to be made. The building comprises a lecture room, a large laboratory, two small laboratories, an apparatus room, and workshop. At the back is another large room to be used for a natural history museum.

Every room is fitted with electric light and Ridge ventilation, which keeps the air pure even when filled with workers. The lecture theatre, which is capable of holding from 80 to 100 boys, is fitted with a solid slate table on brick piers, so that work can be done on it with the most delicate instruments without interference from the vibration of the floors. The fact that the rooms are all on the ground floor, gives the opportunity of putting all delicate instruments, such as balances, galvanometers, &c., on brick pillars, and thus to get rid of any vibration whatsoever.

The main laboratory contains ten tables for elementary physical measurements, two for calorimetry, two for magnetism, and two for heat experiments. Each table has a cupboard containing the necessary apparatus, and an electric lamp giving direct illumination on the tables without shadow or glare in the eyes of the worker.

Of the two smaller laboratories, one is an optical room, which can, of course, be completely darkened, and is fitted with two optical tables and a heliostat, so as to use direct sunlight as often as possible.



The other is the electricity room, containing two tables for frictional, and two for voltaic electricity, with cupboards, &c., as in the main laboratory.

Provision has been made for a small engine and dynamo for electrical work, and these will no doubt be added in time.

The whole is under the charge of Mr. L. Cumming, to whom the arrangement is due, and who is certainly to be congratulated on the result.

Every boy who takes up natural science at Rugby not only goes through a course of lectures, but has also to do experimental work himself in the laboratory. This enables him to grasp the subject much more thoroughly, and to remember it much better than if he attended the lectures only. That this method has had excellent results, will be seen by the number of successes in natural science that Rugby has gained of late years in scholarship and other examinations.

EVIDENCE OF A TWILIGHT ARC UPON THE PLANET MARS.

DURING last summer and autumn Mr. Douglass made at this observatory 341 micrometric measures of the diameters of Mars. In addition to their general value as micrometric measurements, these turn out to be of a peculiarly interesting

character. For on reducing them I find that beside furnishing, from their great number, relatively accurate values of the equatorial and polar diameters and of the polar flattening, they yield a by-product as unexpected as it is important. Their discussion reveals, in short, what appears to be unmistakable evidence of a twilight upon the planet, sufficiently pronounced to be visible from the earth, and actually to have been measured unconsciously by Mr. Douglass. That Mars possessed an atmosphere, we had what amounted to proof positive before; but that the fact should again be brought to light in this literal manner, as a silver lining to a cloud of figures, is a point of some curiosity. The measures had no such end in view; indeed, to detect the presence of an atmosphere by measures of the diameters had not suggested itself to any of the most adventurous of observers. Yet, as will be seen, the quantities upon which the evidence rests are so large as to be quite without the pale of accidental error, being ten times as great as the probable errors of observation, and twice as large as those that disclose the polar flattening. That they have hitherto escaped detection is due to their having been masked by another factor affecting the size of the polar diameter, as will appear in the course of this paper. To the unsuspected presence of these two causes, at times nearly offsetting each other, so far as relative values go, is attributable in all probability much of the discrepancy in the determinations of the polar flattening hitherto made.

The first measures were made on July 6, and the last on November 21, 1894. From October 12 they were taken nearly every night. Those here given were all made by Mr. Douglass. Later in the paper I shall introduce others by Prof. W. H. Pickering, which confirm the result. But here at the outset it may be well to point out that whether the results of many observers are to be preferred to those of one is, omitting discourteous personalities, a question entirely of what is to be determined. If the determination be one of absolute quantity, the more observers the better, provided they be good; but if, on the other hand, the determination be of relative magnitudes, one observer is better than many, as his personal equation obligingly eliminates itself, whereas two such equations can by no possibility, short of chance, eliminate each other. Now, in the present case, while the determination of the planet's size, and even to some extent of its polar flattening, are matters of absolute quantity, the evidence of a twilight upon it is one which rests upon relative results. The former, therefore, are subject to any systematic errors there may be; the latter, essentially free of them. In consequence, the by-product in this case is actually more trustworthy than the main results themselves.

Much care was taken in the matter of the Martian measures. In the ones I shall first discuss, those made from October 12 and November 21, Mr. Douglass adjusted the longitudinal thread of the micrometer, parallel or perpendicular, as the case might be, to the planet's polar axis, according to Marth's ephemeris, and then placed himself, so that the line joining his eyes was kept parallel to this thread or to the fixed transverse thread at right angles to it, during any one set of observations, the position being then recorded. As measures were taken in both positions for each diameter at various times, we have here a comparison of some eventual value. In eye-estimates such orientation in the position of the observer is absolutely essential in order to correct his possible astigmatism. Into measures, however, astigmatism enters only to cancel out. For if we consider the matter, it is at once evident that the whole field is distorted in the same proportion, the space between one turn of the micrometer and the next being reduced or expanded in the same ratio as the part of the image measured. The astigmatism thus eliminates itself.

From October 12 to November 21, Mr. Douglass made in all 275 measures; 140 of the equatorial, and 135 of the polar diameter. In the reduction of the measures, account has been taken of the place upon the micrometer screw at which the measures were made, and its appropriate value introduced. For by the forethought of Mr. Douglass in suspecting the possibility of variation, we measured the value of a micrometer turn at different points of the scale to confirm his conjecture.

Preliminary to the discussion of the results, it will be well to explain the corrections determined and applied. The first correction is that arising from refraction. This is the correction due to the differential effect of refraction upon the planet's opposite limbs at the extremities of the particular diameter measured. It depends both upon the altitude of the planet at the time of observation, and upon the inclination at that moment, of the particular diameter to the vertical. In many cases it was so small as not to make itself perceptible in the column.

The correction for aberration, similarly a differential effect, was so utterly insignificant throughout as not to appear at all.

The next correction is that due to irradiation. Toward its determination two different tests were made, in each case upon both Prof. W. H. Pickering and myself: in the one the effect should have been less than in the case of Mars, in the other greater. As in both cases the observers substantially agreed, the results may be accepted as having some impersonal value.

The first test was made upon a railroad switch-head, a white circular disc with a smaller black circle painted upon it. The size of these circles was unknown to the observers.

Their estimates were:

(W. H. P.) ... (white rim) ... 1 ; (diameter black circle) .. 1·3
(P. L.) ... ... ,, ,, ... 1 : ,, ,, ,, 1″·205

The discs and their distance were then measured and gave:

For diameter black circle ...	...	202 mm.
For radius white rim ...	...	126 mm.
For ratio	..	$1\frac{1}{4}$
For distance from eye ...	...	57 yds.

Therefore 1 mm. equalled 3″·9.

For the amount of the irradiation in seconds of arc, x, assume the amount of the irradiation of the white rim against the general background of earth of a brown colour to have been two-thirds that of the rim against the black circle. We have then, for the first observer, the following equation to determine x.

$$\frac{252 \text{ mm. } 10/3\,x}{212 \text{ mm. } 6/3\,x} = \frac{2\cdot0}{1\cdot3}; \text{ from which } x = 0\cdot2 \text{ mm. or } 36'';$$

for the second observer: $x = 40''$.

The second test was on the moon (November 22), when the old moon was seen in the new moon's arms. In this case the irradiation proved for both observers to be one-seventh of the radius of the old moon, or about 157″.

In the case of Mars, the value for the irradiation probably lies between these two limits. For the contrast between the Martian limb and the sky is pretty certainly greater than that of the white rim and the black circle of the switch-head, and less than that of the moon's bright limb and the sky, to which the contrast between the limbs of the old and of the new moon closely approximates.

It is to be noted that with a given illumination and a given eye, the irradiation correction is a personal constant, not depending upon the size of the disc measured and diminishing inversely as the magnification. In all the measures subsequent to and including October 15, the power used was 860; in those of October 12, it was 617. The correction, therefore, for all except those of October 12 was 0″·10; for those of October 12, 0″·14.

Such, then, is the correction for irradiation upon the planet's limb. The double of it, therefore, would need to be subtracted from the measures of a disc similarly placed to that of Mars when fully illuminated. But the disc of Mars was not fully illuminated even at the moment of opposition, and grew less so as time went on. Now it will be evident on consideration that the irradiation from the terminator must be very different from that upon the limb, inasmuch as the light fades away to nothing at the one, while it has its full value at the other.

To determine the amount of the correction needed at the terminator it is to be observed that if

γ = the areocentric angle between the sun and the earth;
α = the angle between the terminator and the point of the illuminated surface of which the irradiation is sought; and
m = the ratio of the irradiation at the limb to the radius of the disc, we have for the extent of the irradiation at the terminator

$$m\left(\frac{\sin\alpha}{\sin(\gamma+\alpha)}\right)^{\frac{1}{n}} - \left(\cos\gamma - \cos(\gamma+\alpha)\right)$$

where n denotes the ratio of the irradiation to the illumination, and is equal to about 2^6; that is, it takes 2^6 times the illumination to produce twice the irradiation effect. This value is got from inter-comparison of the above tests as limiting values, the

resulting value for Mars and the known decrease in illumination due to the telescopic magnification employed.

To deduce the resulting irradiation we must find the value of which renders the above equation a maximum, and then substitute this value in the equation. To do so directly leads to an equation of so high an order that approximation will be found the better, if indeed it be not the only, method of solution. By this means it appears that the necessary correction does not become insensible, to three places of decimals, till the phase angle, γ, somewhat exceeds 30°.

The formula must be used within the limits for which $\frac{\sin \alpha}{\sin (\gamma + \alpha)} = 1$; beyond them $\frac{\sin \alpha}{\sin (\gamma + \alpha)}$ must be taken as unity.

If the reflection from the disc followed the law of the cosines—that is, if the apparent illumination were always equal to the true one—we should have

$$m\left(\sin \alpha\right)^{\frac{1}{n}} - \cos \gamma - \cos\left((\alpha + \gamma)\right)$$

where α, γ, and n have their previous values, and m = a constant to be determined from the equation, from the value at the limb.

But although this is the formula for the case of a theoretical rough bare globe, it manifestly does not hold in the case of Mars, of which the limbs are not only as bright as the centre of the disc, but much brighter. The previous formula is, therefore, to be preferred to it, although even that formula makes the irradiation correction at the terminator too great as compared with that at the limb.

But it is to be specially noticed that no law of correction for irradiation at the terminator, however big it make that correction to be, is able to do away with the outstanding differences, presently to be noted, of the equatorial diameter at different times upon which the evidence of the twilight arc is based.

There is also the correction for phase. Inasmuch as the phase axis and the polar axis did not in general coincide, there entered into its determination beside the amount of the lacking lune, the angle of inclination of the two axes. So that the amount of the defalcation had to be calculated in accordance for each night. These corrections and their results reduced to distance unity have been calculated and tabulated.

Besides the above there is a fifth correction needed to reduce the diameter measured for the polar one, to the true polar diameter. The diameter measured perpendicular to this, or the apparent equatorial diameter, although not in fact an equatorial diameter, was always exactly equivalent to one, since its extremities were always each 90° distant from the pole. The other, however, was the diameter of the ellipse made by the plane passing through the polar axis, which was inclined to the polar axis by the angle of tilt, and needed, therefore, to be reduced to that ellipse's minor axis. This correction is best applied to the means, and appears in the subjoined table.

Polar Diameters.

	Cor. measures.	Cor. for inclination.	Further cor. for twilight band.
Oct. 15 to 23 inc.	9″·385	9″·379	9″·356
,, 15 to 1 of 24 ,,	9″·377	9″·371	9″·348
,, 15 to 24 ,,	9″·368	9″·362	9″·339
,, 15 to 29 ,,	9″·375	9″·369	9″·346
,, 12 to 30 ,,	9″·384	9″·378	9″·354
Nov. 2 to 21 ,,	9″·397	9″·390	9″·353

Equatorial Diameters.

Oct. 15 to 23 inc.	9″·420	—	9″·404
,, 15 to 1 of 24 ,,	9″·428	—	9″·402
,, 15 to 24 ,,	9″·424	—	9″·395
,, 12 to 30 ,,	9″·440	—	9″·396
Nov. 2 to 21 ,,	9″·545	—	9″·402

Twilight arc ... ... 10°
Polar flattening .. 1/191 of the equatorial diameter.

As previously explained, no correction is needed for astigmatism, as the measures themselves correct it.

So soon as the measures had been corrected and reduced to distance unity, two things became apparent, both so large as to be almost unmistakable before taking the means. The first was the polar flattening; the other an equally systematic difference in the size of the equatorial diameter according as the measures were made in October or in November. The November measures came out much larger than the October ones; while the corresponding polar measures, on the other hand, showed no corresponding increase. Struck by this fact, and suspecting its cause, instead of taking the mean of all the measures for each diameter, I divided them into sets according to their proximity in date to the time of opposition, and took the mean of these sets.

The means are as follows:—

Polar Diameter.

Mean October 15 to October 23, both dates inc.	9″·379
,, ,, 12 ,, 30, ,, ,,	9″·378
,, Nov. 2 to Nov. 21, ,, ,,	9″·390

Equatorial Diameter.

Mean October 15 to October 23, both dates inc.	9″·420
,, ,, 12 ,, 30 ,, ,,	9″·440
,, Nov. 2 to Nov. 21, ,, ,,	9″·545

Opposition occurred on October 20. The first set in each schedule, therefore, was made within four days of opposition; the second, within eleven days of it; the last, from fourteen to thirty-two days after it. That there is a systematic increase in the equatorial measures is apparent. That it is not paralleled by a corresponding increase in the polar ones shows instantly that it can hardly have been due to systematic error in the observer, since in that case both sets of measures should, in all probability, have been affected.

Now as all the measures had previously been corrected for refraction, irradiation, phase and tilt, the means of each diameter should have agreed with themselves. The polar did so in a very satisfactory manner; the equatorial not only did not, but they differed in proportion to their distance in time from the date of opposition. Now the only factor that increased in proportion to the distance in time from opposition was the phase. The direct effect in the way of decreasing the equatorial diameter had already, as we have seen, been allowed for; what is more, it is a correction susceptible of great accuracy, since it depends upon the motions and relative distances of the earth and Mars, quantities very accurately known. Besides these quantities, there is nothing which enters into the calculation but the position of the pole of Mars, and this would have to be, not only some 35 Martian degrees in error to explain the discrepancy, but would have had to have shifted obligingly to an opposite error during July and August to account for the measures taken then, as we shall see later. In other words, no such discrepancy exists.

In the case of a bare globe this direct effect would be the only effect phase could have upon the equatorial diameter; not so, however, in the case of a body not bare. If a planet possessed an atmosphere, that atmosphere would cause the phenomenon of twilight, and to an observer at a distance the effect of the twilight would be to prolong the terminator beyond what would otherwise be its limits. There would thus result a seeming increase in the equatorial diameter as the disc passed from the full to the gibbous phase. Now this increase is precisely the increase that the measures disclose.

It is furthermore worth noting that in the absence of an atmosphere, the measures of the equatorial diameter as the phase increased would not only have shown no increase, but would actually have shown a decrease, inasmuch as it would be impossible for an observer to see quite out to the edge under the diminishing illumination.

To determine the extent of the twilight thus disclosed by the measures, the angle between the radius to the sunset point and the radius prolonged to the point of the atmosphere last illuminated, had to be found. This enabled an equation to be developed, which gave for the visible twilight fringe an arc of 5°, the double of which, or 10°, is the angle which determines the duration of the twilight, or the twilight arc. On the earth this angle is 18°.

Applying the correction due to the twilight fringe, to the means previously obtained, we find the following close agreement between them:—

Polar Diameter.

October 15 to 22 inc.	9″·356
October 12 to 30 ,,	9″·354
November 2 to 21 ,,	9″·353

Equatorial Diameter.

October 15 to 23 inc.	9″·404
October 12 to 30 ,,	9″·396
November 2 to 21 ,,	9″·402

The value for the twilight band, deduced from these observations, does not measure the full breadth of that band. It gives rather a minimal value for it. For although Mars shows us a disc which is always more than half full, in which aspect an illuminated fringe of atmosphere would be more perceptible to an observer placed without than to one placed within it, provided both were at the same distance off, in the case before us the outsider is a great deal farther off. In consequence, what would be quite recognisable to one standing upon the planet's surface would be too faint to be seen by him at a distance of forty millions of miles away. The detection, therefore, of any twilight on Mars hints that the extent of that twilight is greater than appears; how much greater, we cannot at present say. A second possible cause affecting the extent of the twilight is the constitution of the Martian atmosphere. That atmosphere is practically cloudless; if, also, it be clearer than our own, the twilight would be relatively less for equal amounts of atmosphere, for the amount of twilight is, among other things, a question of the clearness of the air. In a perfectly transparent atmosphere there would be much less twilight than in one charged with solid or liquid particles.

It is to be noted that the evidence of a twilight is independent of any possible change in the value of the corrections. The only corrections that admit of uncertainty are those for the irradiation; and on examining them it will be seen that by no possible alteration can they be made equal to account for the observed increase in the equatorial diameter. Whatever alteration in them be assumed only affects somewhat the extent of the increase; it never does away with it. In other words, whatever these corrections, the fact of a twilight remains.

For the determination of the polar flattening, the measures of October 15 to 23 promise the best result, as the measures of the polar diameter on the 24th were so small, compared with those of the equatorial diameter, as to suggest error. Comparing, therefore, the polar and equatorial means of October 15 to 23, we get for the polar flattening 1/196. This, however, is probably too small; for though the polar cap was nearly non-existent during these observations, there were, on occasions, signs of its temporary reappearance, and an almost continuous brightness of the limb where it had previously existed. This by irradiation would increase the apparent polar diameter, and so decrease the resulting value for the polar flattening. If we compare each polar determination with its corresponding equatorial one, deduce the resulting polar flattening, and then take the mean of them all, we have for the polar flattening the value 1/191.

This is probably not far from the truth, although also probably a little too small, as the polar diameter was unquestionably still slightly increased beyond its real extent, by irradiation from the remains or consequences (vapour in the air, &c.) of the polar cap.

This value, 1/191, is also happily accordant with what theory would lead us to expect. Tisserand has found that with the known rotation of Mars and supposing homogeneity, the planet's flattening should be 1/175 of the equatorial diameter, while if the strata varied in density, after the manner of those of the earth, the polar flattening should be 1/227 of it. Now, assuming Mars to have been developed in general accordance with the nebular hypothesis, his strata would be neither homogeneous, on the one hand, nor, on the other, would they vary in density from the surface to the centre so markedly as is the case with those of the earth. For Mars being a smaller body, the pressure due to gravity would be less, somewhere between that of the earth and that of homogeneity, which is nothing, and the polar flattening should be somewhere between 1/227 and 1/175 of the equatorial diameter. 1/191 is, therefore, not far from the value probable *a priori*. It is interesting to have this result agree thus closely with theory, as it furnishes so much more reason for believing in the general evolution of our solar system.

Any value much less than 1/191 would require that Mars should have had at some time a much swifter axial rotation than he has now, which there is not only no ground for thinking, but much reason for thinking could not have been the case. For Mars lacks the tools for tidal friction, possessing insufficient satellites on the one hand and insufficient oceans on the other, so that even solar tides would be out of the question. Even had he possessed both requisites, it is more than doubtful if their slow action would have materially affected his form. For on the earth, which did possess them, we see that they were practically impotent to alter her shape. Any great change in Mars' period of rotation since he cooled must be looked upon, therefore, as unlikely.

For the final values of the diameters we have, allowing for a slight irradiation from the remains of the polar cap:—

True equatorial diameter	9″·40	″·007
True polar diameter	9″·35	″·007

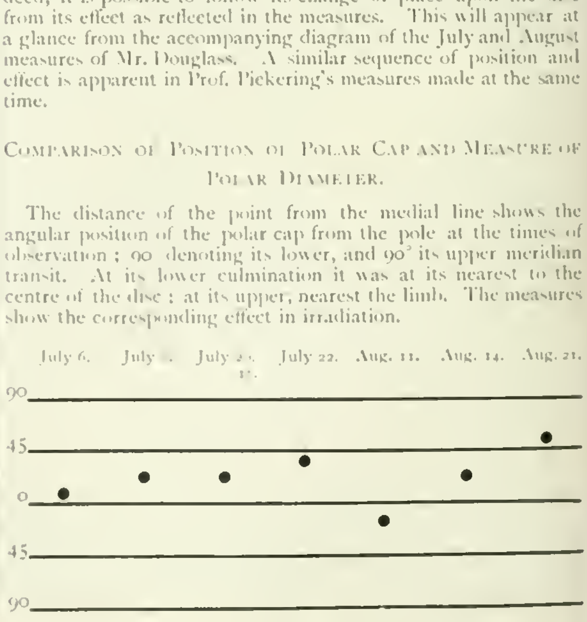
It will be noticed how near these values are to that found by Hartwig from his general discussion several years ago.

We will now consider the September observations and the first of the October ones, those taken upon the fifth of the month. The first thing we notice about them is the abnormal size of the polar measures, so large as to suggest error. On examination, however, we find that instead of mistake they give us our first recognition of the cause that has so long masked the effect of the twilight fringe. The equatorial measures, it will be seen, come out in fairly good accordance with the October and November determinations, being greater than those taken near opposition, although somewhat smaller than the November ones, the discrepancies falling probably within the errors of observation. The polar measure of October 5 is also much what we should expect, but the polar measures of September 20 and 23 are apparently unaccountably larger. If we consider, however, the dates at which they were taken, we shall at once perceive a cause capable of producing the apparent increase. For in September and early in October the polar cap was still in existence. Now the south polar cap is eccentric to the pole, being situated some 5° from it, and from Mr. Douglass's micrometric measures of its position in October, in longitude 59°. As during the observations the south pole was tipped towards the observer, the polar cap was carried, in consequence of the planet's rotation, now in upon the disc, now out upon the limb. Now, if it chanced to be upon the limb at the hour at which the measures were made, its excessive irradiation would produce just such apparent increase in the polar diameter as was observed. On calculating its position for the hours of observation on September 20 and 23, it appears that at those times it was in fact upon the side of the pole toward the limb. Here, then, we have the *deus ex machina* in the matter. To clinch the conclusion, we find on calculating its position for the observation on October 5, when it suddenly measured small again, that at that hour the polar cap was upon the hither side of the pole. Such was also the case on October 12. The discrepancy thus stands accounted for. On October 13, very obligingly, the polar cap practically vanished just in time not to interfere with the most valuable measures at and near opposition.

That such is the explanation of the change in the polar diameter, comes out still more markedly from the July and August measures. Turning to those measures we find that the position of the polar cap is an all-important factor in them. Indeed, it is possible to follow its change of place upon the disc from its effect as reflected in the measures. This will appear at a glance from the accompanying diagram of the July and August measures of Mr. Douglass. A similar sequence of position and effect is apparent in Prof. Pickering's measures made at the same time.

COMPARISON OF POSITION OF POLAR CAP AND MEASURE OF POLAR DIAMETER.

The distance of the point from the medial line shows the angular position of the polar cap from the pole at the times of observation; 90 denoting its lower, and 90° its upper meridian transit. At its lower culmination it was at its nearest to the centre of the disc: at its upper, nearest the limb. The measures show the corresponding effect in irradiation.



			Polar.			
9″·85	10″·29	9″·57	9″.46	9″·41	9″·40	9″·34
			Equatorial.			
9″·67	10″·08	9″·48	9″·33	10″·03	9″·75	9″·41
In relative values						
			Polar.			
1019	1021	1009	1014	938	965	993
			Equatorial.			
1000	1000	1000	1000	1000	1000	1000

At first sight it would seem that the later August measures do not support the rule. Closer consideration will, however, show that they do. For while in July the polar cap was still large, and in consequence reached to the limb, even when its centre was at some distance from it, by August it had dwindled to so small a patch as to be incapable of doing so when at the same angular distance away. Taking account of this fact, it will be seen that the effect is quite in accordance with the position, as comes out clearly in the relative values for the two diameters of August 14 and August 21.

It will now be evident why so large, and intrinsically so unmistakable, an effect as that of the Martian twilight should hitherto have escaped detection; the reason being that the twilight effect and the irradiation from the polar cap each increased their respective diameters to a simultaneous augmentation of both, conspiring each thus to mask the other.

Had measures been continued through a series of months, and been made in sufficient number, both causes must have made themselves evident. For both are periodic, and their periods are not the same. The irradiation from the polar cap has a primary period of thirty-seven days, a secondary one of a Martian year as well as a third depending on the tilt of the pole toward the earth; that of the twilight fringe a varying one of about thirteen months. But as previous measures have been made quite regardless of the twilight effect, and largely regardless of the polar cap, regardless, that is, of its varying position, the results have merely disagreed with each other, and the disagreements been credited to errors of observation. One result of this was discordance in the value of the polar flattening.

When we take both causes into account we find that the means of the July and August observations confirm the October and November ones.

For by comparing the values of the polar diameter when on and away from the limb, it is possible to deduce both the amount of the irradiation from the polar cap and the value of the twilight band from the measures themselves. The results in the case of Mr. Douglass agree with those of his October-November measures. In the case of Prof. Pickering, there is the same relative difference between the determinations, although the absolute values are all smaller.

That in the table the corrections to the July and August measures differ from those applied to the later ones, comes from the different manner of their taking; in the July and August measures the longitudinal thread of the micrometer having been set to the phase axis or perpendicular to it, instead of to the polar one.

In Mr. Douglass' determinations the value for the twilight arc comes out 8°. This is somewhat smaller than the result from the November measures. But a smaller value is precisely what should have been found. For the greater the phase angle, the less the foreshortening, which foreshortening by massing the illumination lets the fringe of light become evident farther out. Now the average phase angle was 43° in July and August, as against $18\frac{1}{2}$° in November.

From Prof. Pickering's measures the twilight arc comes out greater, or 11°, and by inference would have come out greater still in November.

Thus it appears that measures made by separate observers, and measures made before and after opposition, all confirm each other to the existence of a twilight band upon the planet.

PERCIVAL LOWELL.

THE FOUNDATIONS OF ENGINEERING EDUCATION.[1]

LET us consider what is the education which a young man needs to fit him for the profession of engineering, whatever be the special line of engineering which he proposes to follow. And, before discussing the details of what he ought to study, let us consider what it is that we desire to accomplish by giving him an engineering education. Naturally, we wish, as far as any education can accomplish it, to put him in the best condition to meet and grapple with the duties, the problems, and the responsibilities of his profession, as they arise.

There are two things which are absolutely necessary to make a successful engineer: first, a knowledge of scientific principles and of the experience of the past; and second, his own experience. The last cannot be given in a school, and each one must gain it for himself in his practice.

But the greater his familiarity with scientific principles and the experience of the past, the more able will he be to advance in his profession, and to be trusted to assume responsibility; indeed, if a man is ignorant of certain details and knows he is ignorant, he can—and if he is the right kind of a man, he will—take pains to learn them, if they bear on the work he has in hand; but if he is ignorant of scientific principles, it is very likely that he does not know he is ignorant, or, if by good luck he becomes aware of the fact, it is next to impossible for him to devote the time and study necessary to correct his ignorance while his mind is busy with his daily work.

Moreover, a man who is not familiar with the scientific principles which concern his work is not a safe man to trust with responsibility; for scientific principles are merely the laws of nature, as far as known, as shown by the experience of the past.

Hence it is that the first and most important thing to be done for the student is to give him a thorough drill in the scientific principles which find their application in his profession. It is in the school that this knowledge may best be acquired, since it is only with great difficulty that principles can be mastered after the student begins practice, and then as a rule but very imperfectly; and this view is borne out by those engineers who have been successful, and who have had to acquire their knowledge of scientific principles little by little, and as best they could, during the practice of their profession. Too much cannot be said by way of insisting that a thorough mastery of such scientific principles far outweighs in importance anything else that can be done for the student; and this is so true, that it is a decided mistake to neglect it in order to impart to him greater skill in such processes as will probably engage his attention the first year after he goes to work, as, for instance, to make him a skilful surveyor, a finished machinist, or an elegant draughtsman. Greater skill can far more easily be acquired after he goes to work than can scientific principles, and if this mistake is made the consequences will probably pursue him throughout his professional life.

The two fundamental sciences upon which the scientific principles of engineering are especially dependent are mathematics and physics, and no proper course in engineering can be arranged without insisting upon these fundamentals.

Let us begin with the subject of pure mathematics, and consider what portions should be studied, how they should be studied, or rather how they should be known, and of what service they are to the engineer after they have been mastered; bearing in mind that, in accordance with the opinions already expressed, the course of study should be laid out with direct reference to the needs of the engineer; and that when it is so laid out, it will, by the very fact that it leads to a definite end, subserve best the purpose of true education, and hence of developing the powers of the mind. Probably the best definition of mathematics is that given by Prof. Benjamin Pierce, who defined it as "the science of drawing necessary conclusions." This definition, of course, includes formal logic, and hence embraces more than is ordinarily understood by mathematics. We may assert, however, that the only function of mathematics is to draw necessary conclusions from the assumed data. Mathematics has nothing whatever to do with the correctness or incorrectness of the data. If these are correct, the conclusions deduced by mathematics will also be correct; whereas, if the data are false, the conclusions deduced by mathematics will be false.

Thus, if we require the sum of a certain set of numbers, the process of addition will give the correct result, provided the numbers added are the right ones; but if the numbers added are not the right ones, the result of the addition will not be the one desired. Indeed, we might compare pure mathematics to a mill; it will only produce good meal when the corn furnished to it to grind is of good quality; and if the corn is poor, the meal pro-

[1] Extracted from a course of lectures delivered in the Lowell Institute, Boston, by Prof. G. Lanza, Professor of Theoretical and Applied Mechanics, Massachusetts Institute of Technology, and published in the *Journal* of the Franklin Institute.

duced will be poor. With the selection of the corn which it is to grind, the mill has nothing to do.

No natural law can be discovered or proved by mathematics alone; the discovery or proof of natural law requires experiment and observation in all cases.

Just as arithmetic is a means of making calculations of certain kinds, so there are other kinds of calculations that can only be performed by the use of mathematics higher than arithmetic: some kinds require algebra, some geometry, some trigonometry, some descriptive geometry, some analytic geometry, and some the differential and integral calculus; while others yet require higher mathematics. Now, inasmuch as every one can easily understand the necessity of arithmetic for the purpose of making the calculations, and drawing the conclusions which come within its province: so, it follows that the engineer should have a thorough working knowledge of whatever portions of pure mathematics he needs, to make the calculations that are liable to arise in his work, and also to draw the necessary conclusions which concern the engineering and scientific subjects with which he must deal in his profession. This latter is an all-important matter; for, if our prospective engineer is to be fit to assume responsibility at some portion of his career, before he allows himself to use a formula in practice, he ought to know just how it is deduced, and what are the assumptions that were made in deducing it.

The rule-of-thumb engineer ignores this matter, and allows himself to risk the money, the safety, and the lives of his fellow men by making use of constants and mathematical formulæ found in some hand-book or elsewhere; using these constants and formulæ blindly, without knowing how they were deduced, or whether they have any reasonable foundation to stand on; or, in other cases, contents himself with merely guessing at what should be the dimensions of the various parts of a structure or machine. The natural result of such a course is poor work, and often disaster; and the world is rapidly waking up to this fact, so that important engineering work is being less and less entrusted to these rule-of-thumb engineers.

Now, I may say that knowledge of at least all these subjects mentioned in my communication—through the differential and integral calculus—is necessary for our prospective engineer.

As to descriptive geometry, that is classed by many, not as mathematics, but as a branch of drawing. It is the mathematical work upon which the making of engineering drawings of all sorts is based, and hence I have put it in this list.

So general has the conviction become that the engineer needs some knowledge of the differential and integral calculus, that it is not necessary for me to cite cases where he must use it if he is to perform his work intelligently and not by rule-of-thumb. Differential equations is a subject which is sometimes classed with the differential and integral calculus, and sometimes as a separate subject. It is one that should, if possible, be learned at least to a small extent, though the more that is known about it the better.

As to the special work to be done in each of these subjects, it is a matter of judgment with the one who lays out the course, and I shall not weary you with these details; but I must explain what ought to be the result aimed at, in other words, how the student should know his mathematics.

I might express my idea by saying that he should acquire the ability to use it as a tool, but, when I say that, I mean not merely as a tool for making computations, but also as a tool for drawing necessary conclusions of the kinds that apply to his engineering work; and this last is the feature which is most frequently lacking in the mathematical instruction given to engineering students.

By one method often pursued in teaching mathematics, the student is made to grind through a certain round of operations over and over until he has been so drilled in performing them mechanically that he can *perform* a similar problem. By this method, he is only taught to use it as a tool for making computations.

Another method, often pursued, is to exercise the student's ingenuity in performing a variety of (sometimes puzzling) problems which are of purely abstract interest, and are not planned in such a way as to bear upon the class of problems liable to arise in engineering work or study. This course probably tends to make the student do more thinking, but does not direct his thinking in the channel most useful for the prospective engineer. To accomplish the desired object in the teaching of mathematics, it is, of course, necessary that the teacher should be able to grasp the requirements of the engineering courses, and should know the special kind of use that the prospective engineer will have for his mathematics in later life.

Another important matter, the accomplishment of which concerns the treatment of the subjects of a mathematical nature that follow in his course, rather than the treatment of the pure mathematics itself (though the mathematical department can help in this matter), is that the student should be taught to distinguish between the mathematics of the work, and the assumptions made at the beginning, or in the course of it, respecting the proposition he is dealing with.

Perhaps I might sum up a part of the foregoing by saying that the student should be taught to think, and that the attempt to teach him to think should begin as early as possible in his course, and be kept up throughout. It is much easier for the average student to learn a lesson to recite by rote even if it contains a lot of formulæ, than it is to do a little solid thinking himself, and yet the more we can make him think the more successful in every way will he be.

Perhaps those of you (if there be any) who teach mathematics may think that the standard I have set is high. I admit that it is, and also that it requires hard work, good judgment, and the qualities of a good and efficient teacher, not only in laying out the course, but even more in teaching the class. Nevertheless, this standard is the one that is needed, and good judgment, and good teaching can at least approach near to it within the time that can be afforded in our engineering courses, even with such previous mathematical preparations as can be obtained to-day by the students before they enter the engineering schools; and as fast as it becomes possible to raise the standards of admission, the standard I have set can be even more fully realised.

The other fundamental science which I have mentioned is physics. It may be defined as that department of natural science which treats of the laws governing the various manifestations of energy (as gravitation, sound, heat, light, electricity, &c.). It deals with the natural law as it applies to just those classes of bodies, and substances with which the engineer does his work. Indeed, physics is a very general term, and might be made to include a great many subjects that are usually called by some more special name. For instance, mechanics is sometimes spoken of as a separate science, and sometimes as forming a part of physics, and, moreover, under any definition physics includes a part of mechanics.

Practically, a course in physics is the suitable preparation for a proper understanding of the scientific principles of most of the engineering work with which the student will come in contact. Treating, as it does, of the laws of nature, the more thoroughly an engineer knows it, the more successful will he be, and an ignorance of these laws can only result in failure.

Mechanics, light, sound, heat, and electricity, are all matters that concern the profession of the engineer so intimately that he cannot afford to neglect a careful study of their first principles. It is unnecessary for me to say, therefore, that there is no portion of the work usually treated in the best and most thorough courses of general physics but what should be included in the course of our prospective engineer.

Then, a certain amount of work in the physical laboratory is of great importance for the student, for it teaches him how to ask questions of nature, and how to get correct answers; in other words, how to make careful and accurate experiments, and this is a matter that intimately concerns the engineer. It is true that the greater part of his experimental work will have to be performed on a considerably larger scale than that usually carried on in a physical laboratory; but, on the other hand, some of his most important and delicate work involves the doing of just such experimental work as he is taught to carry on in a well-organised and well-equipped physical laboratory; and also the performance of these physical laboratory experiments is a proper introduction to his later course of experiments on the large scale, drilling him in accuracy and care while working on small amounts of material.

Indeed, I might mention quite a number of experiments which are all-important to the engineer, and in regard to which, it would be difficult to decide whether they should be called physical laboratory or engineering laboratory experiments, since they often have to be performed in both. Thus, the calibration of thermometers is a matter that is properly taught in the former, and yet the engineer who is to do delicate engineering work is liable to have to calibrate his thermometers, or at least to make a careful and accurate comparison with a standard which he or

some one else has calibrated. Again, the determination of the mechanical equivalent of heat is a matter of vital importance to the engineer, but the best and most accurate work thus far upon the subject has been done by Prof. Rowland, a physicist, in his physical laboratory.

As a rule, when experiments are to be performed on the large scale they get beyond the possibilities of a physical laboratory. In this category we may place such experimental work as the testing of steam engines and steam boilers, the testing of the strength of materials of construction on a practical scale, &c.; but, in order to carry out these tests with proper accuracy, we have generally to perform delicate measurements, as, for instance, measurements of temperatures, &c., in the first, and measurements of very small elongations or shortenings in the second case, and consequently have to use the suitable apparatus with the necessary degree of accuracy.

Since we have just been considering mathematics and physics, which may be called general sciences, perhaps a few words should be said in regard to chemistry. I cannot claim for it a similar position of fundamental importance in the engineering part of an engineering course that belongs to mathematics and physics. Nevertheless, a certain amount of chemical knowledge is of great importance to all engineers; but when they have passed this point, although a farther knowledge would be useful, it is not one of the most important things. The chemical composition of fuels, of steels and irons, of cements, of oils, and of other materials, is a matter that directly concerns the engineer. It is true that he can usually have his chemical analyses made for him, and generally would better do so; but he must know enough of chemistry to understand the bearing which the chemical composition of his materials have on their use in engineering work. Some knowledge of industrial chemistry is also desirable, so that he shall understand the nature of the processes performed in manufactories in which chemical processes on a large scale are performed.

The instruction in chemistry should, if possible, be given very early in the student's course. In the case of the Massachusetts Institute of Technology, and also, I think, in that of several other schools, both lectures and laboratory work in chemistry are given in the first year, and when this is done the instruction in chemistry fulfils another important function, viz. it introduces the student at the very threshold of his course to a species of scientific work that obliges him to think, and this, in a direction in which, as a rule, he has not been trained in the preparatory schools. Especially is this true of the laboratory work, for, by observing the results of experiments which he himself makes, he must learn how to interpret the replies of nature; and as chemistry, unlike mathematics, is an experimental science, it trains the thinking powers of the student even more than do his algebra, geometry, and trigonometry.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

A PROSPECTUS of the course in practical chemistry at the Polytechnic Institute of Brooklyn has been received, and it indicates that very efficient work is carried on at the Polytechnic. The course, which is under the direction of Prof. P. T. Austen, appears to be adapted in every way to meet the wants of the day, and to train competent analytical and technical chemists. The claims of pure chemistry are also recognised, facilities being given for post-graduate work in it, as well as in applied chemistry and chemical engineering.

THE Department of Science and Art has issued the following lists of Scholarships and Exhibitions just awarded:—Whitworth Scholarships (tenable for three years), £125 a year each: Arthur H. Barker (24), engineer; George W. Shearer (21), apprentice engineer; Percy Nicholls (24), engineer; Harold R. Cullen (21), engineer. Whitworth Exhibitions (tenable for one year), £50 a year: Charles E. Goodyear (21), shipwright; George M. Brown (23), draughtsman; Norton Baron (22), engineering student; Harry Jackson (20), engineering student: Edward M. Leflufy (22), engine-fitter apprentice; Arthur E. Hyne (21), fitter apprentice; Robert McMillan (20), engineer apprentice; John W. Roebuck (23), fitter; George Follows (24), engineer; Arthur J. Baker (19), engine-fitter apprentice; William D. Ross (21), fitter; Frank H. Phillips (20), engineer apprentice; Henry T. Hildage (20), fitter; William P. Jones (25), marine engineer; John W. Milner (20), mechanical engineer; William Bayliss (20), apprentice fitter; John B. Shaw (21), engineer; James Walker (22), engineer; William H. C. Kemp (21), engineer apprentice; William J. Talbot (23), engineer; Henry C. Trigg (24), draughtsman; Duncan R. McLachlan (24), engineer; George A. Robertson (21), engineering student; Charles H. Imrie (22), engineer; William McG. Wallace (20), apprentice fitter; William J. Gow (20), apprentice fitter; William Lauder (20), draughtsman; Samuel A. Clarke (25), draughtsman; Edmund B. Ball (21), engineer student; Jabez W. Ashdown (20), engineer apprentice.

THE list of successful candidates for Royal Exhibitions, National Scholarships, and Free Studentships (Science) is as follows:—National Scholarships for Mechanics: Edmund R. Verity (19), student; George Patchin (17), engineering student; Harry Jackson (20), engineering student; William Ditchburn, jun. (19), teacher. National Scholarships for Chemistry and Physics: Thomas S. Price (19), student; Franz E. Studt (21), tailor; Herbert Bailey (18), student; William Bennett (16), student; John W. Barker (18), laboratory assistant. National Scholarships for Biological Subjects: Thomas G. Hill (19), student; Ernest A. Scott (17), student. National Scholarships: Charles E. Goodyear (21), shipwright; Edward M. Leflufy (22), engine-fitter apprentice; William H. James (22), student; William T. Clough (18), student; Herbert Halliday (22), student; William Cameron (18), laboratory assistant; Ernest Hibbert (15), student; Sidney E. Lamb (21), engine-fitter apprentice; Joseph Lister (19), teacher; William Parker (19), student; Ernest T. Harrison (18), laboratory assistant. Royal Exhibitions: George E. Clarke (17), student; Edward C. Hugon (16), student; Thomas G. Procter (19), engine-fitter apprentice; John A. Tomkins (20), scientific instrument maker; William T. Swinger (20), engineer; John W. Roebuck (23), fitter; Robert L. Wills (21), shipwright apprentice. Free Studentships: William D. Ross (21), fitter; Leonard W. Cox (21), student; Edgar R. Sutcliffe (20), engineer; William P. Jones (25), marine engineer; Percy M. Hampshire (19), lecture assistant; William J. Talbot (23), engineer.

SCIENTIFIC SERIALS.

Bulletin de l'Académie des Sciences de St. Pétersbourg, 5th series, t. ii. No. 4, April 1895.—Proceedings, in which we notice the discovery, by G. Schneider, in Prof. Kovalevsky's laboratory, of lymphatic glands in the earth-worm, *Dendrolæna rubida* (Crimea), and in *Perichæta*; as well as a communication by E. Burinsky, on his method of restoring by means of photography the writing in old documents which time has rendered invisible. A number of good negatives having been taken on collodion pellicules, they are superposed, and the visibility of the faintest markings is rendered still greater by means of a "contrast positive" obtained with regulated artificial light. Definitive researches into the variations of latitude at Pulkova, on the ground of older observations made with the great vertical circle, by A. Ivanoff (in French). The previous memoirs of the author on the same subject being considered as first approximations only, the definitive formulæ are now given. The observations of the years 1863–1875 and 1842–1849 are treated for that purpose separately. Both series lead to formulæ which agree very well with the formula given by Mr. Chandler in the *Astronomical Journal*, No. 322; however, the Pulkova observations of the first-named period seem to point to the necessity of slightly reducing the half-amplitude of the yearly term in Chandler's formula. Two long series of Pulkova observations thus fully confirm Mr. Chandler's conclusions. On the measurements and calculations of some photographic charts of the stars, by F. Renz (in German). A catalogue of all stars, down to the magnitude 11·0, which were occultated by the moon during the last eclipse, was given in the *Astronomische Nachrichten*. It appeared, however, that occultations of stars down to the twelfth magnitude could be observed at several observatories. Accordingly, the corresponding region of the sky was photographed by Prof. Donner with such an exposure (25 minutes) as to obtain the stars of twelfth magnitude as well, and F. Renz measured their positions with the Pulkova Repsold apparatus. The Potsdam photographs of the same region, made in 1891, were also re-measured, while the right ascensions of thirty-five fundamental stars were accurately determined at Pulkova with the meridian circle. The agreement between the different plates is quite satisfactory; and no distortion of the field could be detected. However, there are certain small systematic errors which cannot yet be well

explained. Thus, the right ascensions on plate i. are on the average by 0·047s. greater than the values deduced from plate ii.—The Arachnides collected by G. Potanin in Mongolia in 1876-1879, by E. Simon (in Latin). Part i. Araneæ and Opiliones: forty-one species are mentioned and described, nineteen being new species.—Do the spurs of the Carpathians penetrate into European Russia? by General A. Tillo (in Russian). The question is answered in the negative. Supan and Lehman, in Kirchhoff's "Landerkunde von Europa," trace the limits of the Carpathians outside the boundaries of Russia: so also the Russian geologists, Barbot-de-Marny and Karpinskiy, did not see continuations of these mountains either in Poland or in Russia. The new hypsometrical map, now compiled by the author on a larger scale (27 miles to the inch), confirms this view.—New or little known Ixodidæ in the museum of the St. Petersburg Academy, by A. Birula (in Latin). Eight new species are described and figured on two plates.

Memoirs (Trudy) of the Kharkoff Society of Naturalists, vol. xxvii., 1892-93.—Obituary of I. Th. Levakovsky, by A. Guroff, with a portrait.—Researches into the crystals of kermesite and uranotil, by P. P. Piatnitzky.—The Algæ of the bays and peat-bogs of the Dnieper, in the government of Poltava, by M. Alexenko. This flora is poor, the *Cladophora*, *Confervæ*, *Enteromorpha*, and *Ulotrix* prevail, while Desmidiaceæ and Protococcoideæ are very rare; 371 species are mentioned.—The flora of the Central Caucasus, by I. Akinfieff, part i. (see Notes, vol. lii. p. 304).—On the part played by hydrocarbons in the inter-molecular respiration of higher plants, by W. Palladin. It had been shown by Diakonoff (*Ber. d. deut. bot. Ges.*, 1866) that certain fungi give up carbonic dioxide during their inter-molecular breathing, only when the surrounding feeding medium contains a substance capable of fermenting. It was desirable to verify whether the same is true with higher plants, but the difficulty was in the fact that the cellular sap always contains glucose, which itself is capable of fermenting. By a series of experiments on etiolated leaves, the author now confirms Diakonoff's conclusions for higher plants as well.—Short preliminary notes in the Addenda. Vol. xxviii., 1893-1894.—Geological description of Kharkoff town, with map and profiles, by P. Poustovitov.—On the part played by the secondary parallel chains in the grouping of forests and steppes in West Caucasus, by A. Krasnoff. An answer to G. Akinfieff's criticisms.—Materials for the Algæ flora of the government of Kharkoff, by M. Alexenko; 407 species are described.—Preliminary report on a geological excursion in the government of Kherson, by P. Piatnitzky.—Biological observations, by W. Taliev. A series of various observations of facts relative to the life of plants, which have hitherto attracted but little or no attention, chiefly relative to fertilisation, colouration, movements of plants, and heliotropism in connection with the affluence of sap.—On the flora of the basin of the Chakva, by A. Krasnoff, being a preliminary report of a botanic excursion into the province of Batum, containing an excellent general description of the vegetation, poor in species, but attaining a luxurious development of the individuals.—On the lichens of the neighbourhoods of Kharkov, by W. Tschernov; fifty-five species are described. Chemical studies on the seeds of *Myristica fragrans*, by W. Palladin, being a note on a special substance which is found in several seeds, but neither in the leaves or in the twigs, and which is now studied in Prof. Schultze's laboratory at Zurich. Preliminary report on botanical researches in the Verkhnedniepróvsk district of Ekaterinoslav, by I. Akinfieff; twenty-six species, new for South Russia, have been discovered.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, August 12.—M. Marcy in the chair.—Observations of planets made at Marseilles Observatory, by M. Coggia. The observations were made with the 0·26 m. equatorial, and for the planets BZ and CA (Charlois).—On algebraical surfaces which admit a continuous group of birational transformations, by M. Paul Painlevé.—On a special microscope for the observation of opaque bodies, by M. Ch. Fremont. The novelty in the microscope described, consists essentially in the method used for obtaining vertical illumination of the object, applicable with high powers. A concave mirror is arranged obliquely *inside* the microscope tube to reflect downwards a beam of light entering at a side aperture in the tube. The light passes through a prism which reduces the rays to parallelism with the axis of the microscope and then through the lenses of the objective to the object. The concave mirror and the prism are pierced centrally by a conical tube along which travel the rays of light from the object, the image being formed and magnified by the eye-piece in the usual way. M. Marey remarked on the great use the new modification would have in the chrono-photographic study of the movement of microscopic beings.—On some melting and boiling points, by M. H. Le Chatelier. From the experiments made, it is probable that the melting point of gold determined by M. Violle to be 1045°, is a little low. The error is certainly not more than 20°, and the results so far obtained would not justify the alteration of the pyrometer scales in actual use.—On certain potassium derivatives of quinone and hydroquinone, by M. Ch. Astre. A number of potassium derivatives are described, concerning which it is stated: the action of metals on quinone, together with the existence of oxy-potassium compounds yielded by quinone and hydroquinone (to be described in a coming paper) confirm the diketonic nature of quinone. The formation of these compounds and the passage of some of them from the hydroquinone to the quinone series, allow a formula to be given to quinone clearly expressing its diketonic character and accounting for its numerous reactions.—A theorem concerning the separation of the roots of numerical equations of every degree, by M. Teguor.—A white rainbow, by M. E. Kern. A lunar rainbow observed at 10 p.m. August 5.

BOOKS, PAMPHLET, and SERIALS RECEIVED.

Books.—British Birds: W. H. Hudson (Longmans).—Lectures on Elementary Navigation: Rev. J. B. Harbord (Potter).—Polyphase Electric Currents and Alternate-Current Motors: Prof. S. P. Thompson (Spon).—Transactions of the Australasian Institute of Mining Engineers, Vol. 2 (Adelaide).

Pamphlet.—The Recent Evolution of Surgery: A. P. Gould (K. Paul).

Serials.—Journal of the Chemical Society, August (Gurney).—Proceedings of the Physical Society of London, August (Taylor).—Bulletin of the American Mathematical Society, July (New York, Macmillan).—Natural History of Plants: Kerner and Oliver, Part 15 (Blackie).—Bulletin de L'Académie Royale des Sciences, &c., de Belgique, 65e Annee, No. 6 (Bruxelles).—Astrophysical Journal, August (Chicago).—Royal Natural History, Part 22 (Warne).

CONTENTS.

THURSDAY, AUGUST 29, 1895.

SIR SAMUEL BAKER AND NORTHERN AFRICA.

Sir Samuel Baker: a Memoir. By T. Douglas Murray and A. Silva White. 8vo. Pp. xii. 447, with six illustrations and nine maps. (London: Macmillan and Co., 1895.)

North Africa. Stanford's Compendium of Geography and Travel. (New series). *Africa.* Vol. i. By A. H. Keane. 8vo. Pp. xvi. 639, with seventy-seven illustrations and nine maps. (London: E. Stanford, 1895.)

A SUMMARY of our present knowledge of Northern Africa, and a memoir of the late Sir Samuel Baker, may be appropriately considered together, for Baker's main title to fame rests on the work he did in that region: and had his experience been properly utilised, the most interesting part of it might not have been lost to civilisation and closed to scientific inquiry.

Samuel White Baker came of an old Devonshire family, members of which have done good work for their country since the time when Sir John Baker served Henry VIII. as Attorney-General, Chancellor of the Exchequer, and Speaker of the House of Commons. Baker was born in London on June 8, 1821, and spent most of his early life at Enfield. He was destined for a commercial career, and in 1842 placed in his father's office in Fenchurch Street. But the work was utterly uncongenial to him. His marriage kept him quiet for a time, but not for long; for next year he gave up business and went to Mauritius, where the family had estates. In 1846 he went for a shooting expedition to Ceylon, and was so impressed by the possibilities of the island, which then had a very bad reputation, that he resolved to found a colony in it. In 1848 he led a party of settlers to Newera Eliya, where 1000 acres of land had been bought from the Government. This was cleared, and a settlement made. Baker remained there till 1855, and during his stay did a good deal of big-game shooting. In 1856 his wife died, and as he had previously lost three of his children, he became very depressed, and actually resolved to enter the Church. This scheme came to nothing, and Baker accepted instead the post of manager of the Dobruscha Railway, the construction of which had been just begun. This kept him busy in 1859 and 1860, and raised in him the keen interest he afterwards felt in the Eastern question. It was in the next year, when Baker was forty years of age, that he resolved on an expedition into Africa to try to meet Speke (whose sister had married Baker's father) and Grant, and carry out some explorations to supplement theirs. In order to gain experience of the people and to learn the languages required, he made a preliminary excursion up the Atbara to some of the Abyssinian sources of the Nile. He left Khartum on his main expedition on December 18, 1862, reaching Gondokoro in the following February. Here he met Speke and Grant, who returned northward in Baker's boats, while he and his heroic wife continued their journey southward along the Nile valley, and through Unyoro till they reached the Albert Nyanza at Bakovia. The discovery of this lake was the greatest achievement of the expedition; but it was only the accident of the condition of the weather, that robbed them of the discovery of the snow-clad peaks of Ruwenzori. They had reached a point whence, in clear weather, the mountain ought to have been as visible "as St. Paul's dome from Westminster Bridge," as Stanley said. They returned to Europe in 1865, and in 1869 went back to the Soudan on an expedition to suppress the slave trade. Baker had all a Devonshire Quaker's horror of this trade. The view that slavery was a kind of secondary larval structure, necessary in a certain stage of national progress, and later on to be absorbed or thrown off, was not then recognised. Baker simply regarded it as an unholy thing, which was to be crushed by any means or at any cost. He accordingly went for it with the pluck of a bull-dog, and just about as much judgment. He was given a commission to go to the Soudan to break up the gangs of slave raiders. He had an independent command, but could do little of permanent value without the assistance of his colleague, the Governor of Khartum; but this worthy official, as well as Baker's native assistants and the supreme authorities in Cairo, all believed in the slave trade in theory, and carried it out in practice. Ismail Pasha alone seems to have been sincere, and not to have endeavoured to thwart the efforts he was ostensibly supporting. Thanks, however, to Baker's indomitable pluck and energy, and his tact with the men, this Quixotic expedition was carried through with a certain measure of success. Its commander alone benefited much by it, for he secured a great reputation as a leader of men, and learnt better to understand both the Soudan and the slave trade. He returned to Europe in 1873, recognising the futility of trying to effect a social revolution over several millions of square miles by shooting a few score of the agents in a trade, of which the principals lived unpunished in Cairo and Khartum. He realised that the only useful course was to improve the industrial conditions, so as to render slavery unnecessary. Had Baker been sent back to the Soudan, and allowed to work on these lines, the subsequent revolt might have been avoided. But the task was entrusted to other hands, and unfortunately Gordon's peculiar genius was less successful with Mohammedan fanatics than it had been with the stolid Chinese.

After Baker's return he settled at Sandford Orleigh in Devonshire, where he lived till his death, except that every winter he made expeditions to some warmer clime. He was always ready, like a knight-errant of old, to rush forth to relieve the inhabitants of some village on the Brahmapootra from the tigers that preyed upon them. He was fond of sport to the last; even after he had become too unsteady to be a match for anything worse than the worn-out old tigers who have had to turn "man-eaters."

The story of Baker's life is pleasantly told, and even in less competent hands could not have failed to be interesting. The editors have wisely left Baker to relate most of it by quoting copious extracts from his letters. Explanatory chapters help the reader to understand the condition of African geography at the time of his journeys, and to appreciate the relative importance of his work. These chapters seem to be judicious and well informed. Our main regret is that we do not hear enough of Baker as

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a sportsman and a naturalist. One chapter is devoted to this, but we doubt if it does full credit to Baker's work in this field. His valuable contributions to natural history are barely referred to; his important services to gunnery and his improvements in cartridges are not mentioned. We should have been glad to have seen more space devoted to this, at the cost of condensation of the political writings, some of which are hardly likely to add to his reputation. For when we remember the conditions under which he shot, the clumsy old muzzle-loaders and the badly-mixed powders he used, and the accuracy and fulness of his observations upon the habits of animals, we cannot but reckon Baker as the greatest of English sportsmen.

While Baker's memoir gives an account of the political conditions of the Soudan from 1860 onward, Prof. Keane's admirable summary of the present knowledge of North African geography completes the sketch in other departments. He divides North Africa into six divisions, viz. the Atlas (including Morocco, Algiers and Tunis), the Sahara, the Soudan and the Niger Basin, Egypt and Nubia, and Italian North-East Africa (including Abyssinia and Somaliland). Each of these districts is described separately, an account being given of its general physical geography, of its history, as far as this is known, of its ethnography, and natural history. The ethnographical sketches are especially well done, while the political histories are the most detailed. The natural history is the least satisfactory part of the book. The geology is mostly quoted second-hand, or is taken only from geographical instead of from geological papers. Some of the botanical records are certainly quite untrustworthy, as when on p. 533 *Casuarina* is reported on the banks of the Webi Shebeyli, whereas it occurs only on the ends of the promontories on the eastern coasts. The nine maps are admirably clear, while full of information. The volume is in every way a great improvement on the preceding editions. The immense increase in the material to be summarised, has made the task a difficult one. This enormous growth of knowledge applies, however, to five out of the six districts described. It is only in one that progress has been stopped, and of which the new edition has nothing fresh to report, except paper delimitations in Europe and reaction in Africa. All Junker's collections, the greatest ever made in the equatorial provinces of Egypt, were lost by the closing of the Soudan. It is to be hoped, however, that European officials will not much longer prohibit our representatives in the field from taking action, and again opening to progress the lands where Gordon's death and Baker's life-work added their names to the roll of our national heroes.

J. W. G.

BIO-OPTIMISM.

(*The Evergreen. A Northern Seasonal.* Published in the Lawnmarket of Edinburgh by Patrick Geddes and Colleagues. London: Fisher Unwin, 1895.)

It is not often that a reviewer is called upon to write art criticism in the columns of NATURE. But the circumstances of the "Evergreen" are peculiar; it is published with a certain scientific sanction as the expression of a coming Scots Renascence of Art, and it is impossible to avoid reference to its æsthetic merits. It is a semi-annual periodical emanating from the biological school of St. Andrews University. Mr. J. Arthur Thomson assists with the proem and the concluding article ("The Scots Renascence"), and other significant work in the volume is from the pen of Prof. Patrick Geddes. It may be assumed that a large section of the public will accept this volume as being representative of the younger generation of biological workers, and as indicating the æsthetic tendencies of a scientific training. What injustice may be done thereby a glance at the initial Almanac will show. In this page of "Scots Renascence" design the beautiful markings on the carapace of a crab and the exquisite convolutions of a ram's horn are alike replaced by unmeaning and clumsy spirals, the delicate outlines of a butterfly body by a gross shape like a soda-water bottle; its wings are indicated by three sausage-shaped excrescences on either side, and the vegetable forms in the decorative border are deprived of all variety and sinuosity in favour of a system of cast-iron semi-circular curves. Now, as a matter of fact, provided there is no excess of diagram, his training should render the genuine biologist more acutely sensitive to these ugly and unmeaning distortions than the average educated man. Neither does a biological training blind the eye to the quite fortuitous arrangement of the black masses in Mr. Duncan's studies in the art of Mr. Beardsley, to the clumsy line of Mr. Mackie's reminiscence of Mr. Walter Crane, or to the amateurish quality of Mr. Burn-Murdoch. And when Mr. Riccardo Stephens honours Herrick on his intention rather than his execution, and Mr. Laubach, rejoicing "with tabret and string" at the advent of spring, bleats

> "Now hillock and highway
> Are budding and glad,
> Thro' dingle and byway
> Go lassie and lad,"

it must not be supposed that the frequenters of the biological laboratory, outside the circle immediately about Prof. Patrick Geddes, are more profoundly stirred than they are when Mr. Kipling, full of knowledge and power, sings of the wind and the sea and the heart of the natural man.

But enough has been said of the artistic merits of this volume. Regarded as anything more than the first efforts of amateurs in art and literature (and it makes that claim) it is bad from cover to cover; and even the covers are bad. No mitigated condemnation will meet the circumstances of the case. Imagine the New English Art Club propounding a Scientific Renascence in its leisure moments! Of greater concern to the readers of NATURE than the fact that a successful professor may be an indifferent art editor, is the attempt on the part of two biologists (real responsible biologists) writing for the unscientific public, to represent Biology as having turned upon its own philosophical implications. Mr. Thomson, for instance, tells his readers that "the conception of the Struggle for Existence as Nature's sole method of progress" "was to be sure a libel projected upon nature, but it had enough truth in it to be mischievous for a while. So zoologists honour their greatest! "Science," he says, has perceived "how false to natural fact the theory was." "It has shown how primordial, how organically imperative the social virtues are; how

love, not egoism, is the motive which the final history of every species justifies." And so on to some beautiful socialistic sentiment and anticipations of "the dominance of a common civic ideal, which to naturalists is known as a Symbiosis." And Prof. Geddes writes tumultuously in the same vein—a kind of pulpit science—many hopeful things of "Renascence," and the "Elixir of Life."

Now there is absolutely no justification for these sweeping assertions, this frantic hopefulness, this attempt to belittle the giants of the Natural Selection period of biological history. There is nothing in Symbiosis or in any other group of phenomena to warrant the statement that the representation of all life as a Struggle for Existence is a libel on Nature. Because some species have abandoned fighting in open order, each family for itself, as some of the larger carnivora do, for a fight in masses after the fashion of the ants, because the fungus fighting its brother fungus has armed itself with an auxiliary alga, because man instead of killing his cattle at sight preserves them against his convenience, and fights with advertisements and legal process instead of with flint instruments, is life therefore any the less a battle-field? Has anything arisen to show that the seed of the unfit need not perish, that a species may wheel into line with new conditions without the generous assistance of Death, that where the life and breeding of every individual in a species is about equally secure, a degenerative process must not inevitably supervene? As a matter of fact Natural Selection grips us more grimly than it ever did, because the doubts thrown upon the inheritance of acquired characteristics have deprived us of our trust in education as a means of redemption for decadent families. In our hearts we all wish that the case was not so, we all hate Death and his handiwork; but the business of science is not to keep up the courage of men, but to tell the truth. And biological science in the study still faces this dilemma, that the individual in a non-combatant species, if such a thing as a non-combatant species ever exist, a species, that is to say, perfectly adapted to static conditions, is, by virtue of its perfect reactions, a mechanism, and that in a species not in a state of equilibrium, a species undergoing modification, a certain painful stress must weigh upon all its imperfectly adapted individuals, and death be busy among the most imperfect. And where your animal is social, the stress is still upon the group of imperfect individuals constituting the imperfect herd or anthill, or what not—they merely suffer by wholesale instead of by retail. In brief, a static species is mechanical, an evolving species suffering—no line of escape from that *impasse* has as yet presented itself. The names of the sculptor who carves out the new forms of life are, and so far as human science goes at present they must ever be, Pain and Death. And the phenomena of degeneration rob one of any confidence that the new forms will be in any case or in a majority of cases "higher" (by any standard except present adaptation to circumstances) than the old.

Messrs. Geddes and Thomson have advanced nothing to weaken these convictions, and their attitude is altogether amazingly unscientific. Mr. Thomson talks of the Gospel of the Resurrection and "that charming girl Proserpina," and Baldur the Beautiful and Dornröschen, and hammers away at the great god Pan, inviting all and sundry to "light the Beltane fires"—apparently with the dry truths of science—"and keep the Floralia," while Prof. Geddes relies chiefly on Proserpine and the Alchemy of Life for his literary effects. Intercalated among these writings are amateurish short stories about spring, "descriptive articles" of the High School Essay type, poetry and illustrations such as we have already dealt with. In this manner is the banner of the "Scots Renascence," and "Bio-optimism" unfurled by these industrious investigators in biology. It will not appeal to science students, but to that large and important class of the community which trims its convictions to its amiable sentiments, it may appear as a very desirable mitigation of the rigour of, what Mr. Buchanan has very aptly called, the Calvinism of science. H. G. WELLS.

THE GLYPTODONT ORIGIN OF MAMMALS.

Studies in the Evolution of Animals. By E. Bonavia, M.D. (London: Constable, 1895.)

IN his preface the author writes that: "Having completed the 'Flora of the Assyrian Monuments and its Outcomes,' I was looking about for something to take up next as a subject of study. In the furriers' windows I was attracted by the leopard and tiger skins, which by degrees became objects of interesting study and speculation." In the true interests of zoology, it is to be deplored that his attention was not attracted by some other subject.

The key-note to the startling theory propounded in this volume is to be found in a sentence on page 131, where it is stated that: "The Glyptodonts, or other armoured animals of a similar nature, were the *originals* from which all existing mammals, including marsupials, descended."

This astounding statement is largely based on the belief that the rosettes on the skins of the jaguar and leopard are the remnants of the rosette-sculpture on the bony carapace of the glyptodonts, the author stating (p. 124) that these markings "are *inherited* from ancestral plate-impressions of some extinct glyptodontoid form, and have *not* been evolved by a process of natural selection."

How the author can conceive that the *Felidæ* are descended from any glyptodont-like form (by which it may be presumed an edentate is meant) will pass the comprehension of any anatomical zoologist; but all will endorse his remark (p. 163) that "one would indeed require to have lived a good bit of time to witness a Glyptodon changing into a Jaguar." This, however, is by no means all. Later on the author finds evidence of glyptodont affinities in the bosses on the skin of Rhinoceroses, and remarks (p. 217) that "the giant armadillo has its hind feet ungulate, its hoofs are almost exactly like those of the Malayan Tapir; and in some rhinoceroses the incisor teeth are wholly wanting, and that part of the jaw is contracted, not unlike that of the Glyptodon." If this means anything, it means that rhinoceroses are evolved from a veritable edentate glyptodont; and it is thus a pity the author did not enlighten us how the full dentition and claws of a jaguar were also to be derived from such a type.

It would be mere waste of space to state how mar-

supials enter the scheme, but it may be mentioned that the loss of the primeval carapace of ordinary mammals is attributed (p. 209) to a deficiency of carbonate of lime in the water and plants on which they subsisted. It will also be a surprise to zoologists to learn (p. 142) that the coloration of the Indian black-buck is due to its having lost its armour on the ventral sooner than on the dorsal surface. And equal wonderment will be experienced when they read (p. 300) that dolphins are near relatives of Plesiosaurs, and that the author doubts whether "there are any good reasons for supposing that Ichthyosaurs were *not* mammals"!

In another chapter the author is led, from the study of monstrosities, to the conclusion that horses are more nearly allied to the Artiodactyla than they are to either rhinoceroses or tapirs!

Many more similar instances might be quoted, but it will suffice to say that if the author be right, all zoologists are hopelessly in the wrong in their views on mammalian affinity.

Among the redeeming features in the book will be found many interesting observations on the coloration of cats and horses, and the author appears to have made out a fairly good case for the derivation of the striping of the tiger from the spots of a leopard-like type. Many of the figures of animals, especially the skins of leopards, are admirable examples of photography, and would be well worth reproduction in other works.

R. Lydekker.

OUR BOOK SHELF.

Le Cause Dell' Era Glaciale. By Luigi de Marchi, Libero Docente di Meteorologia nella R. Universita di Pavia. (Pavia: Fratelli Fusi.)

This work does not fulfil the expectations raised by its title. It is a prize essay of 220 large octavo pages, divided into three sections. The first treats of the climatic conditions of a glacial invasion, and here the author agrees with a number of German writers whom he quotes, in considering that a glacial epoch is due to a lowering of mean animal temperature and a diminution of the annual range, accompanied by an increased rainfall in summer. The next section treats of the temperature of the air. We find a large collection of empiric formulæ, taken for the most part from German authors, some of which are based on assumptions which appear to be far from satisfactory, and which certainly cannot be verified in the exhaustive way which one would wish before applying them to find the temperature in the Glacial Age. Among these there is one more important than the others, in which *t*, the mean annual temperature at any given locality, is expressed in terms of no less than *fifteen* physical quantities, such as the supposed temperature of an ideal sky,[1] the absolute radiating power of this sky, the transmissive powers of the atmosphere for radiation from earth and water, and for sun-heat, and last, but not least important, "a term of correction which expresses the effect of the physical and meteorological conditions of the locality," and this term may, according to the author, oscillate between −6° C. and +6° C.

The third section, entitled "The Cause of a Glacial Age," contains the author's deductions from this formula. He uses it to disprove the hypothesis that the Ice Age was due to a change in the obliquity, but he cannot apply it to discuss Croll's theory, because it only takes account of the *total* annual heat received. Hence he refers to previous writers for his criticism on Croll. Similarly the geographical hypothesis is dismissed as insufficient, so that the way is cleared for the author's own hypothesis, viz. that the Ice Age was caused by a general lowering of temperature which arose from a diminution of the atmospheric transparency, which can only be explained (p. 183) as the effect of a general diffusion into the atmosphere, over the whole surface of the earth, of a gas, vapour, or dust which absorbs, or reflects towards space, a part of the heat which comes from the sun. "But since the glacial epoch also presupposes an extraordinary rainfall, among the many hypotheses which may be framed, one spontaneously presents itself, viz. that a great mass of aqueous vapour was launched against and diffused into the atmosphere." Owing to the lowering of temperature due to want of transparency, the vapour would fall as snow, and this precipitation would go on until the mass of vapour injected into the atmosphere is entirely or in great part eliminated.

The author quotes an Italian writer, who suggests that the action of volcanos in the age preceding the Ice Ages affords a possible explanation of the (supposed) launching of these vast masses of aqueous vapour into the atmosphere.

[1] Not [illegible] temperature of space, but (following Pouillet [illegible]) the temperature of a [illegible] surface, of which the radiat- [illegible] of all the celestial [illegible] taken as equal to [illegible] C. for [illegible] the equator.

Leitfaden für histologische Untersuchungen. By Bernhard Rawitz. Second edition. (Jena: Gustav Fischer, 1895.)

Histological methods have become so perfected, microscopic appliances so modified, and staining reagents so numerous, that it is necessary to have good reference books for use in laboratories. Although there are a number of such works, amongst which we may mention Lee's "Vade Mecum," Sims Woodhead's "Manual," and Fletcher's edition of Von Kahlden's "Practical Pathological Histology," the appearance of a new edition of Rawitz's compendium will be welcomed by all who were familiar with the first edition, which was published six years ago. It resembles Von Kahlden's book in arrangement, but while this latter has been compiled specially for pathological investigations, Rawitz's "Leitfaden" is essentially intended for the biologist and physiologist, and forms a suitable supplement to its morbid counterpart. When reviewing Dr. Fletcher's translation of Von Kahlden's book, some time back, we regretted the omission of various matters relating to section-cutting, embedding and staining, an omission which is excusable on the ground that in a work on practical pathological histology a sound knowledge of these subjects might be taken for granted. Rawitz gives excellent descriptions of all our recognised modern methods, and a careful account of paraffin embedding and paraffin cutting, which will prove useful to all who wish to become familiar with what is undoubtedly the best method for general histological purposes. His directions for working with celloidin are equally good, and since this method is somewhat neglected in this country the beginner will find a number of hints which Dr. Fletcher might well have included in his translation. The completeness with which the various methods of fixation, hardening, and staining have been enumerated is admirable, and we gain the firm conviction that the author has only included what is sound, and in careful hands certain to give good and trustworthy results. Chapter xi. (part 1) contains some useful information on the art of drawing and "reconstructing" microscopical objects. The "Leitfaden" may be recommended without hesitation to the histologist as a book of reference for use in the laboratory; it will save time, and seldom cause disappointment.

A. A. K.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The University of London.

I AM anxious to make it clear that what Sir John Lubbock has sprung upon us is a radical change in the procedure of Convocation.

The object can only be, it appears to me, to obtain a reversal of its policy. As a political expedient it is, therefore, very similar to the action of those politicians who for analogous reasons would change the constitution of the House of Lords.

Sir John now defines what he calls his "suggestion" in the following words :—"That in voting on the new Charter, members of Convocation should do so 'as at a senatorial election,' *i.e.* by voting papers." I call this a radical change in the procedure of Convocation.

I put aside the not immaterial point that as a Statutory Commission is a delegation from Parliament, the result of its labours will not be embodied in a Charter, but will be virtually in effect an Act of Parliament when approved by that body.

Sir John has made the following statements about his "suggestion" :—

(1) "I am not asking that any privilege which they do not at present possess should be conferred upon my constituents, but only supporting what is now their *legal right* . . . This right I know they highly value" (NATURE, July 18, p. 269).

(2) "It is the *law* at present" (NATURE, August 8, p. 340).

The words which I have put in italics are definite and explicit, and are, of course, in flat opposition to my repeated statement that Sir John's suggestion amounts to a fundamental and, indeed, revolutionary change of procedure. This change consists in extending the mode of voting in a senatorial election to other matters. Now the mode of voting at a senatorial election is prescribed by the 21st clause of the Charter, which is printed in NATURE for July 25, p. 296. It embraces two very important points. First, the right of absent members to vote at all is not absolute but only *permissive*. The words are: "Power to the Convocation, *if it shall think fit*, to enable absent members of the Convocation to vote on such nominations . . . by voting papers." Secondly, this permissive right is strictly limited by the words "*but not so to vote on any other matter.*"

It is upon this vital discrepancy between Sir John's statements quoted above and the provisions of the Charter that I think it is imperative that he should give some explanation. This demand on my part he is pleased to call an "attack." Well, however that may be, he at least owes it to himself to meet it.

I trust, however, that I have now made it clear, and even to Sir John, that his "suggestion" is not the law, but that, further, it involves the abrogation of a portion of the Charter. I think as a member of Convocation that in making such a proposal without consulting that body he has exceeded his functions as our Parliamentary representative. At any rate it must, I think, be admitted that he is making short work of the "right" which his "constituents highly value." (NATURE, August 8, p. 340.)

I am unwilling to prolong a painful discussion. But as Sir John is pledged to bring forward his "suggestion" in Parliament, which of course can incorporate it in the Bill, if it thinks proper, it seems to me of extreme importance to dissipate his contention that it is already the "law." W. T. THISELTON-DYER.

Kew, August 23.

The Nomenclature of Colours.

THE interesting article of Mr. J. H. Pillsbury, published in your last number, recalls to me a passage in my autobiography, which, though it is already in print, will not be issued until after my death. As bearing on the question Mr. Pillsbury raises, this passage may, perhaps with advantage, be published in advance. The plan suggested aims at no such scientific nicety of discrimination or naming as that he proposes, but is one which is applicable with the means at present in use. It is, as will be perceived, based on the old theory respecting the primary colours; but whatever qualification has to be made in this, need not affect the method described. The passage is as follows :—

"I mention it here chiefly for the purpose of introducing an accompanying thought respecting the nomenclature of colours. The carrying on of such a scheme would be facilitated by some mode of specifying varieties of tints with definiteness; and my notion was that this might be done by naming them in a manner analogous to that in which the points of the compass are named. The subdivisions coming in regular order when 'boxing the compass,' as it is called, run thus :—North, north by east, north-north-east, north-east by north, north-east ; north-east by east, east-north-east, east by north, east. Applying this method to colours, there would result a series standing thus :—Red, red by blue, red-red-blue, red-blue by red, red-blue (purple) ; red-blue by blue, blue-red-blue, blue by red, blue. And in like manner would be distinguished the intermediate colours between blue and yellow and those between yellow and red. Twenty-four gradations of colour in the whole circle would thus have names ; as is shown by a diagram I have preserved. Where greater nicety was desirable, the sailor's method of specifying a half-point might be utilised—as red-red-blue, half-blue ; signifying the intermediate tint between red-red-blue and blue-red by red. Of course these names would be names of pure colours only—the primaries and their mixtures with one another ; but the method might be expanded by the use of numbers to each : 1, 2, 3, signifying proportions of added neutral tint subduing the colour, so as to produce gradations of impurity.

"Some such nomenclature would, I think, be of much service. At present, by shopmen and ladies, the names of colours are used in a chaotic manner—violet, for instance, being spoken of by them as purple, and other names being grossly misapplied. As matters stand there is really no mode of making known in words, with anything like exactness, a colour required ; and hence many impediments to transactions and many errors. In general life, too, people labour under an inability to convey true colour-conceptions of things they are describing. The system indicated would enable them to do this, were they, in the course of education, practised in the distinguishing and naming of colours. If, by drawing, there should be discipline of the eye in matters of form, so there should be an accompanying discipline of the eye in matters of colour."

Were some authoritative body to publish cards representing these various gradations of colour, arranged as are the points of the compass, each division bearing its assigned name, as above given, such cards might serve as standards ; and any one possessing them would be able to indicate, within narrow limits, to a shopkeeper or manufacturer, the tint he or she wanted. Of course to complete the method it would be needful that there should be a mode of indicating gradations of intensity, and if the numbers 1, 2, 3, were appended to indicate the degrees of impurity by mixture with neutral tint, *a*, *b*, *c*, might be used to signify the intensity or degree of dilution of the colour.

Very possibly, or even probably, this idea has occurred to others, for it is a very obvious one. HERBERT SPENCER.

The Mount, Westerham, July 23.

Clausius' Virial Theorem.

THE above-named theorem, which appeared in the *Phil. Mag.* for August 1870, much as it is now used in connection with the kinetic theory of gases, received little, if any, attention in England for some time after its introduction. Apparently the theorem was accepted without hesitation or discussion, and, as far as I can learn, neither on its first introduction or since has it received any adverse criticism, or, in fact, any criticism whatsoever. My object in writing this letter is, in the first place, to direct attention to the arguments used by Clausius to establish his theorem, which appear to me to be unsound, and secondly, by applying a simple test case, to show that the theorem itself is not true.

Clausius first proves the following equation.

$$\frac{m}{4t}\int_0^t \frac{d^2(x^2)}{dt^2}dt = \frac{m}{2t}\int_0^t x\frac{d^2x}{dt^2}dt + \frac{m}{dt}\int_0^t \left(\frac{dx}{dt}\right)^2 dt.$$

If for the moment, for the sake of simplicity, we divide bot sides of the equation by $\frac{m}{2t}$, we get

$$\tfrac{1}{2}\int_0 \frac{d^2(x^2)}{dt^2}dt = \int_0^t x\frac{d^2x}{dt^2}dt + \int_0^t \left(\frac{dx}{dt}\right)^2 dt,$$

and this may be written

$$ux = \int_0^t x\,du + \int_0^t u\,dx.$$

In this form it is easy to see that each term may be graphically represented by an area, and the equation simply expresses the fact that the rectangular area xu is equal to the algebraic sum of the areas $\int_0^t u\,dx$ and $\int_0^t x\,du$. It is obvious that for periodic motion the rectangle xu will vanish when a suitable value is given to t; but so also will the areas $\int_0^t u\,dx$ and $\int_0^t x\,du$. So that when $xu = 0$ we get, either

$$\int_0^t u\,dx = 0 \text{ and } \int_0^t x\,du = 0; \text{ or } \int_0^t u\,dx = -\int_0^t x\,du.$$

Again, in what Clausius calls "stationary motion" when xu does not vanish periodically, although we can make the expression $\frac{m}{2t}ux$ vanishingly small, by taking t very great, it is obvious that if the areas $\int_0^t u\,dx$ and $-\int_0^t x\,du$ are not equal before multiplying them by $\frac{m}{2t}$, the expressions so obtained are not so afterwards. Moreover, and finally, it should be observed that the expression $m\int_0^t u\,dx$ *does not represent kinetic energy*; to represent which the expression should be $m\int_0^t u\,du$. The above considerations seem to me to entirely upset Clausius' demonstration.

In the tenth edition of Maxwell's "Heat" (p. 323), Lord Rayleigh has given an illustration of the manner in which he supposes the "virial" to act in opposition to kinetic energy, and we may take his illustration as a simple test of the theorem. He supposes two bodies, each of mass m, to revolve in a circular path with a constant velocity about their centre of gravity. Here, as there is no pressure, the so-called virial equation takes the form

$$\Sigma\tfrac{1}{2}mv^2 = \tfrac{1}{2}\Sigma Rr.$$

In the above equation v, the velocity, is constant, and $R = mf$. If we take ρ as the radius of the circle, then $r = 2\rho$, and the equation becomes

$$\tfrac{1}{2}v^2\Sigma m = \tfrac{1}{2} \times 2\rho f\Sigma m.$$

Hence

$$\tfrac{1}{2}v^2 = \rho f;$$

which equation *does not represent the ordinary law of centrifugal force*. Lord Rayleigh omitted to notice that

$$\Sigma R = \Sigma mf = f\Sigma m = 2mf.$$

When, however, we throw overboard all ideas of "virial," and look upon the term $\frac{1}{2}\Sigma Rr$ in the so-called "virial equation" as simply representing work and equal to $\frac{3}{2}pV$, also an expression for work, then the equation

$$\Sigma\tfrac{1}{2}mv^2 = \tfrac{3}{2}pV + \tfrac{1}{2}\Sigma Rr$$

is certainly true. But there seems no possible advantage to be obtained in splitting the right-hand member into two equal terms, instead of writing the equation

$$\Sigma\tfrac{1}{2}mv^2 = 3pV\text{ ; or }\Sigma\tfrac{1}{2}mv^2 = \Sigma Rr\text{ ;}$$

in either of which forms—the first for preference—it is applicable to ideal gases. For natural permanent gases the equations become, either

$$\Sigma\tfrac{1}{2}\beta mv^2 = 3pV\text{ ; or }\Sigma\tfrac{1}{2}\beta mv^2 = \Sigma Rr,$$

and not

$$\Sigma\tfrac{1}{2}\beta mv^2 = \Sigma\beta Rr.$$

[illegible] my letter (p. 221) on "Argon and the Kinetic Theory." C. E. BASEVI.

London, W., August 14.

Incubation among the Egyptians.

ARTIFICIAL incubation, like many another practice supposed to be peculiar to modern civilisation, is but a revival from very ancient times. Diodorus, an author who wrote about forty years before the commencement of the Christian era, tells how the [illegible] of [illegible], with their own hands, bring eggs to maturity, and how the young chickens thus produced are not inferior to any [illegible] hatched by the usual means.

The process, probably with methods differing little from those of ancient times, survives to the present day among the fellahs of Egypt. In suitable places ovens are erected, and the proprietors go round the neighbouring villages collecting eggs. A sufficient number having been collected, they are placed on mats strewed with bran, in a room about 11 feet square, with a flat roof. Over this chamber, which is about 4 feet high, there is another built about 9 feet in height. The roof, which is vaulted, has a small aperture in the centre to admit light during the warm weather; below it another opening of larger dimensions communicates with the oven below. In the cold weather both are kept closed, and a lamp is kept burning within. Entrance is then obtained from the front of the lower chamber. In the upper room fires are made in troughs along the sides, and the eggs are placed on the mats below in two lines, corresponding to and immediately below the fires. The fires are lighted twice a day, the first time to die about midday, the second to last from about 3 p.m. to 8 p.m. The first batch of eggs are left for about half a day in the warmest situation, after which they are moved to make room for others, until the whole number in hand have had the benefit of the position. This is repeated for six days. Each egg is then examined by a strong light. All eggs that at this stage are clear are rejected, but those that are cloudy or opaque are restored to the oven for another four days. Then they are removed to another chamber, where there are no fires, but the air is excluded. Here they lie for five days, after which they are placed separately, about one or two inches apart, and continually turned. This last stage generally takes six or seven days. During this time a constant examination is made by placing each egg to the upper eyelid, when a warmth greater than that of the human skin is a favourable sign. The duration of the process generally extends over twenty-one days, but thin-shelled eggs often take only eighteen days. The average heat required is 86° F. Excessive heat is prejudicial. In Egypt the best time is from February 23 to April 24.

J. TYRRELL BAYLEE.

Mountain Sickness.

I HAVE just come back from a journey in the region of the Andes, and in looking over the numbers of NATURE, which had accumulated during my absence, I came across the extract, which you make in your notes of February 21, from the *Revue Scientifique*, on the subject of mountain sickness. I cannot agree with M. Kronecker's statement that beyond three thousand metres mountain sickness attacks all persons as soon as they indulge in the least muscular effort, as I made the acquaintance of many people, mostly railway men, living and working at altitudes of fourteen or fifteen thousand feet on the Oroya line and the Southern Railway of Peru, who had never experienced *soroche*, or mountain sickness. As far as my own experience goes, in three journeys across the Andes and several mountain ascents, including one to the top of the crater of the Misti, 19,300 feet above sea level, I had only one attack of *soroche*, and that was at the end of a ride on an oil engine from sea level to fourteen thousand feet in nine hours. But this was so complicated with suffocation by the oil fumes and scorching by the heat of the furnace while running through the fifty-seven tunnels on the line, that I cannot say how much was mountain sickness and how much was not. At any rate, I was perfectly well the next morning, and rode over a pass nearly seventeen thousand feet high without the slightest inconvenience. As regards the danger of a prolonged sojourn, my experience teaches me that it is almost entirely due to personal idiosyncrasy and unwise eating and drinking. A healthy person whose lungs and heart are all right, who does not over-eat and is very moderate in the use of stimulants, will not suffer from mountain sickness after the first few hours, and in many cases will not suffer at all if the ascent is sufficiently gradual. Of course very violent exertion produces distress by reason of the deficiency of oxygen. I do not think that there need be any difficulty about the officials of the proposed Jungfrau railway, if steady men, not of a full habit of body, are selected. I never heard of any trouble from mountain sickness among the Peruvian railway men unless they over-stimulated, and yet they are accustomed to go in a day from sea level to 15,764 feet on the Oroya line, and to 14,666 feet on the Southern line, and return to sea level on the following day. I may add that I have made both these journeys myself without the slightest inconvenience, and have been able to walk and ride without any trouble at the end of them.

London, August 20. GEORGE GRIFFITH.

How was Wallace led to the Discovery of Natural Selection?

THE reviewer of Osborn's "From the Greeks to Darwin" (*antea* p. 362) says that Marshall quotes the fact of Wallace's being led "to the discovery of natural selection as he lay ill of intermittent fever at Ternate," and refers one to the abridged form of the "Life and Letters of Charles Darwin" for this statement. Having only the original edition in three volumes, from the year 1887, at my disposal, wherein I cannot find it, I would draw attention to my having published the fact as far back as 1870 ("Charles Darwin and Alfred Russel Wallace. Ihre ersten Publicationen uber die Entstehung der Arten, nebst einer Skizze ihres Lebens und einem Verzeichniss ihrer Schriften." Erlangen, E. Besold, 8vo, pp. xxiii. and 56, on page xviii.) The remarks to be found there are based upon a letter of Mr. Wallace's dated November 22, 1869, and now before me, a passage of which runs thus:—

"The paper No. 9 ['on the law which has regulated the introduction of new species' A.N.H. 1855] should be read along with No. 19 ['on the tendency of varieties to depart indefinitely from the original type' P.L.S. 1858]. When I wrote it I was firmly convinced of the derivative origin of species, but had not arrived at an idea of the process. When I wrote No. 19 at Ternate [in the year 1858] I did not [know] what were Mr. Darwin's views or the nature of the work he was engaged on, except generally that it was on 'Variation.' I hit upon the idea of 'Natural Selection' (though I did not give it that name) while shivering under the cold fit of ague, and I was led to it by Malthus' views on population applied to animals. As soon as my ague fit was over I sat down, wrote out the article, copied it, and sent it off by the next post to Mr. Darwin. It was printed without my knowledge, and of course without any correction of proofs. I should, of course, like this fact to be stated."

This I did in my pamphlet of 1870 on the page quoted, and on page 39, and I hope Dr. Wallace will forgive me for now making known the whole of his highly interesting statement *in his own words*. Of course I am not sure whether he did not tell or write the same to some one else, though I am not aware that it has been published.

Ordinary mortals dream nonsense in their fits of fever, a philosopher of Dr. Wallace's standing conceives original ideas!

A. B. MEYER.

Zoological Museum, Dresden, August 19.

THE letter to Prof. Newton, published in the abridged "Life of Darwin," was written in 1887. I had entirely forgotten that I had written on the same subject to Dr. Meyer in 1869, or that he had published anything in reference to it. That letter probably contained my earliest statement on the subject, and it agrees substantially with my later statements.—A. R. WALLACE.

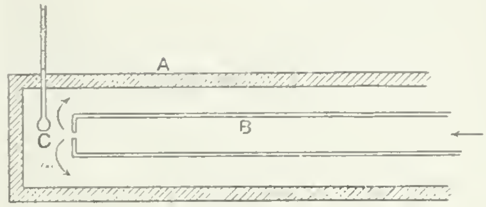
A Problem in Thermodynamics.

SIEMENS taught us how, by using the heat of the gases escaping from a furnace to heat the gas and air before entering the furnace, we could obtain temperatures limited only by the fire-resisting quality of the materials of which the furnace is constructed. Now, it occurred to me whether on the same principle very low temperatures might not be reached. My idea is this: If compressed air is expanded to atmospheric pressure, the gas does work in overcoming the resistance of the atmosphere, and is cooled to a corresponding amount.

Suppose, for instance, the gas is compressed to 1/100 of its volume, then 1 cubic metre would perform, in expanding against the atmospheric pressure of 1 kil. per 1 square centimetre, or 10,000 kilos per square metre, an amount of work equal to $10{,}000 \times 0{\cdot}99 = 9900$ kilgr.-metres, and absorb $\frac{9900}{424}$ units of heat. Now, 1 cubic metre of air weighs 1·24 kil., and, having a specific heat of 0·24, the temperature of the expanded air would be lower 78° than before expanding.

Now suppose A is a tube of a material impervious to heat—that is, a perfect non-conductor—and B a tube made of a perfect conductor of heat; the tube A being closed at one end, and B having a small opening in the end.

Now, if a continuous supply of compressed air is kept up in tube B, this air will come down in temperature, and, passing along between A and B, cools the compressed air before it expands.



I should be glad if any of your readers could give me the theoretical minimum of temperature produced at C.

Essen-Ruhr, Germany. E. BLASS.

A Remarkable Flight of Birds.

ON September 30, 1894, about 3 p.m., I was observing the sun through an 8-inch telescope. I noticed some dark figures of birds passing, like shadows, across the sun. I was using a dark glass, and the birds were, consequently, only visible when seen against the bright solar disc. The silhouettes of the birds were very sharply and clearly cut. Every few seconds a bird would emerge from the darkness, pass slowly across the sun and disappear on the other side. I watched them for over ten minutes without any decrease in their numbers. The whole number of birds must have been enormous, otherwise it would have been impossible for some of them to have passed as frequently as they did between my telescope and the sun. The birds were flying in a southerly direction, and were quite invisible to the naked eye. I was, therefore, unable to determine their distance, but should think they must have been two or three miles away, for the telescope was in focus for the birds and sun at the same time. I do not know what birds they were. Comparing the spread of their wings with the solar disc, I should say their wings subtended an angle of about two minutes. The place from which I observed them was Shere, a village between Guildford and Dorking. I am told that such a flight of birds has not before been recorded in this country, and have been urged to publish an account in the hope that other astronomers, who may have seen a similar thing, may be led to mention the fact.

Shere, Guildford. R. A. BRAY.

THE IPSWICH MEETING OF THE BRITISH ASSOCIATION.

IN our last article we gave a general outline of the local arrangements for the Meeting. The programme, as a whole, is now fairly complete. A slight alteration has been made with reference to the soirées; the first will be given by the Ipswich Scientific Society and the Suffolk Institute of Archæology jointly, and the second by the Mayor of Ipswich (Mr. J. H. Bartlet). The fitting up of the Section Rooms is proceeding rapidly, and arrangements are being made for the darkening of those in which a lantern will be used. In the case of Sections A and B, which meet in the same building, only the room allotted to Section B will be fitted up with dark blinds and a lantern screen, and the Sections will be asked to exchange rooms on days when papers requiring lantern illustration are read in Section A. The same arrangement will be made as to Sections D and K, which meet in the two rooms at the Masonic Hall. For the President's address in these Sections, the Lyceum Theatre, which is a short distance off, will be placed at the disposal of the Sectional Committees, as the Masonic Hall rooms may be hardly large enough to contain all those who would probably wish to be present on these particular occasions. For a similar reason, Section G, which meets in the Co-operative Hall, will be asked to allow the President's addresses in Sections A and B to be delivered there. A spacious room adjoining the main

street, and within two minutes' walk of the reception room, will be set apart for a ladies club-room.

The excursions will be of a more varied character than usual. On the Saturday afternoon the geologists will visit the well-known crag district, including Orford, Sudbourne, and Chillesford. This will give an opportunity for the examination in the field of many of the deposits to which the previous days' discussions have been devoted. On the same afternoon, there will be a dredging excursion down the Orwell, whilst other parties will go to Bury St. Edmunds (on the invitation of the Mayor), to Helmingham Hall, and to Southwold (where also the Mayor and a Local Committee will act as hosts). On the Thursday afternoon after the meeting, there will be another dredging expedition, and also an excursion to Colchester (on the invitation of the Mayor), to the Flint Napping Works at Brandon, and to the Broads, on which occasion the party will be entertained *en route* by the Mayor of Yarmouth. The geologists on this day will go to the Norfolk coast to examine the Glacial and Pliocene deposits in the neighbourhood of Cromer, where arrangements will be made so that those, who wish, may stay the night. Other short afternoon excursions will be made near Ipswich whenever time allows.

The programme of work in the Sections is rapidly filling up. In Section A, the President, Prof. W. M. Hicks, will take as the subject for his address, "The Fluid Theories of Ether and Matter." On the Friday a joint sitting will be held with Section B, when Prof. A. Schuster will open a discussion, in which Lord Rayleigh and Mr. Crookes are expected to take part, on the evidence to be gathered as to the simple or compound character of a gas from the constitution of its spectrum. On the same occasion, Captain W. de W. Abney and Mr. C. H. Bothamley will read papers on orthochromatic photography. There will also be important discussions in Section A, on the question of a new practical unit of heat, introduced by a paper from Mr. E. H. Griffiths, and on the objective character of combination tones, opened by Prof. Rücker. Other papers to be read in the Section will be on the teaching of geometrical drawing in schools, by Prof. O. Henrici, on the electrification and diselectrification of gases, by Lord Kelvin and Messrs. Maclean and Galt, on vertical (earth-air) electrical currents, by Prof. Rücker, on the events that go on within molecules, by Dr. Johnstone-Stoney, on the velocity of light in a rarefied gas through which a current is passing, by Messrs. Edser and Starling, on a dynamical top, by Mr. G. T. Walker, and on Boltzmann's minimum theorem, and the question of reversibility in the kinetic theory of gases, by Mr. E. P. Culverwell.

In Section B, the President, Prof. R. Meldola, will deal in his address with the relations of physiology and chemistry. The Monday will be devoted chiefly to papers dealing with the relation of chemistry to agriculture, which are already anticipated locally with considerable interest, on account of the large stake the district has in agriculture. Prof. Warington will be amongst those to read papers on the question. The Tuesday will be given up to papers on organic chemistry.

In Section C, the address of the President, Mr. Whitaker, will be devoted to the subterranean geology of the Eastern Counties, as exhibited in various deep borings and wells. Mr. Whitaker will also have a paper on the latest results in the boring for coal, now being made at Stutton. The other papers on local questions will probably deal mainly with newer Tertiary geology; Ipswich being a capital centre for the study of our Pliocene and Pleistocene deposits. Besides the local papers, communications have been promised from certain of the foreign visitors, on the correlation of our British Tertiary deposits with their continental equivalents. A paper by M. Gustave Dollfus, of Paris, on the extent of the Tertiary seas of Western Europe, will give his views of the physiography of the south and east of England in Pliocene times, and is likely to lead to some discussion. Glaciation, as was to be expected at Ipswich, will occupy a good deal of time. Prof. Sollas will exhibit the "pitch-glaciers," by which he has produced in the laboratory many of the obscurer phenomena of glaciation. Mr. Robert White communicates a paper on the glaciation of tropical South America.

Of the miscellaneous communications likely to be brought forward, we can only mention a few. Mr. Joseph Francis, the engineer to the New River Company, will have one on the method adopted to ascertain the direction of the dip in the Palæozoic rocks met with in the deep borings at Ware and Cheshunt. It may be observed that while there is no difficulty in obtaining the amount of the dip, when a solid core is brought up, it has always been a difficult problem how to obtain the far more important data as to its direction. Papers are also expected from Prof. Nicholson and Mr. Marr, on the phylogeny of the graptolites; from Messrs. Garwood and Marr, on zonal divisions of the Carboniferous system; from Mr. T. V. Holmes, on the ancient physiography of South Essex; from Messrs. Reid and Ridley, on the Arctic and Palæolithic deposits at Hoxne. Others, on American palæontology, have been promised by Profs. Claypole and Marsh.

Section D meets this year under the presidency of Prof. W. A. Herdman, and, for the first time in the history of the Association, it will be a section of zoology alone. Botany now forms a separate section, and although physiology is nominally attached to Section D for this meeting, it will in fact be unrepresented. The work of Section D will be largely devoted to questions of marine fisheries and marine zoology. On the Friday of the meeting, Prof. McIntosh will open a discussion on fishery questions, and an interesting debate is expected. Prof. Haddon will read a paper on the Royal Dublin Society's Fishery Survey; Dr. Bashford Dean, of New York, will give a paper on apparatus for catching oyster spat and its failure in practice, and will also exhibit an interesting collection of eggs and larvæ; Prof. Herdman will give an exhibition of lantern slides illustrative of fishery problems, and will explain the method of "zoning" of shores, &c., and, in conjunction with Prof. Boyce, will give a paper on oysters and typhoid. Other papers will be read by Prof. Miall, on pupation in insects; by Prof. Ritter, of New York, on budding in Tunicata; by Prof. Lloyd Morgan, on experiments on instinct in young birds; by Dr. H. O. Forbes, on the Antarctic continent, and on seals; and by Dr. Otto Maas, of Munich, Prof. Gilson, of Louvain, Prof. Howes, Mr. Moore, Mr. Hoyle, Dr. Hurst, and others on various subjects.

The following is the provisional programme for Section G:—Thursday, 12.—Address by the President, Prof. Vernon Harcourt; light railways in agricultural districts, by Major-General Webber; congelation of soil for foundation purposes, by M. Gobert; Bentley coal borings (a local work), by R. C. Rapier. Friday, 13.—The growth of the port of Harwich, by W. Birt; notes on improvement of Maas in connection with Hook of Holland route, by the President; Snowdon tram-road, by Sir Douglas Fox; notes on autumn floods of 1894, by W. H. Symons; river weirs and flood prevention, by F. G. M. Stoney. Saturday, 14.—Dredging operations at Mersey Bar, by A. G. Lyster; carbonic anhydride refrigerating machinery, by E. Hesketh; deodorising sewage by Herzite process at Ipswich, by J. Napier.—Monday, 16, will be devoted to electrical papers, among which will be the following:—Induction telegraphy, notes on further advance, by W. H. Preece; glow lamps, by W. H. Preece;

modern applications of electricity to traction, by P. Dawson; the chloride battery, by W. H. Earle; extension and development of the telephone in agricultural districts, by Major-General Webber; telephony, by A. R. Bennett; the field telegraph in Chitral campaign, by P. V. Luke; a new portable photometer, by W. H. Preece and A. P. Trotter. Tuesday, 17.—Interim report of committee on standardising; modern flour-milling machinery, by F. W. Turner; paper-making machinery, by Mr. Mason; printing without use of movable types, by J. Southward; incandescent gas lamps, by C. Cooke; B.A. Standard small screws, by R. B. Compton; uniform factor of safety in steam boilers, by J. Key.

The provisional programme for Section H is as follows:—Thursday, September 12.—Address by Prof. Flinders Petrie; skulls of the aborigines of Jamaica, by Sir W. H. Flower; skulls of the Neolithic invaders of Egypt, by Dr. J. G. Garson; Andamanese, by Morris Portman; Neolithic invaders of Egypt, by Prof. Flinders Petrie. Friday, September 13.—Worked flints from South Africa, by H. W. Seton Karr; flint and metal working in Egypt, by Prof. Flinders Petrie; flints found at Thebes, by Gen. Pitt Rivers; plateau flints of North Kent, by B. Harrison; graving tools from terrace gravels of the Thames valley, by H. Stopes; Palæolithic projectiles, by the same; megaliths of Tripoli, by Swainson Cooper; kitchen midden at Hastings (report), by W. J. Lewis Abbott. Saturday, September 14.—North-west tribes of Canada (report), by Prof. E. B. Tylor; Samoyedes of the Arctic tundras, by A. Montefiore; language illustrating primitive warfare, by Rev. Hartwell Jones; ethnographical survey (report), by E. Sidney Hartland; deviations of children (report), by Dr. Warner. Monday, September 16.—Cannibalism, by Captain Hinde; folk-lore of Ipswich, by Miss Layard; ethnographical conclusions, by G. Laurence Gomme; general conclusions, by Edward Clodd; folk-lore illustrated, by Prof. Haddon; religious origin of dances, by Mrs. Grove. Tuesday, September 17.—On interference with the civilisation of other races, by Lord Stanmore, Prof. Douglas, Prof. Haddon, and Dr. R. N. Cust, and letters of the late R. L. Stevenson; southern Arabians, by Theodore Bent; the Eskimo, by F. Linklater and J. A. Fowler. Wednesday, September 18.—Lake village of Glastonbury (report), by Dr. R. Munro; prehistoric Greek idols, by Arthur Evans; Neolithic station of Butmir, by Dr. R. Munro.

A SOUVENIR OF "CHALLENGER" WORK.

A MEDAL has been prepared as a souvenir of the scientific work connected with the *Challenger* expedition. The medal, which is in bronze, is three inches in diameter, and was modelled by Mr. Birnie Rhind, sculptor, from designs by Mr. William S. Black, both of Edinburgh. It was cast in Paris, and is being presented by Dr. John Murray to the naval officers of the expedition, the contributors of memoirs to the report on the scientific results of the expedition, and to members of the civilian scientific staff, as a souvenir of *Challenger* work.

The accompanying illustrations have been reproduced from two photographs of the casts forwarded to us by Mr. Black, and show the two sides of the medal. On the front of the medal, the head of Athena with owl occupies the centre, and is placed on the globe, which in turn is surrounded by a border of water indicating the voyage of the expedition around the world. Out of the water rises Neptune, with trident and a trawl disclosing the treasures of the deep-sea. The decoration of the border is completed with a dolphin and two mermaids supporting a ribbon with the words "Voyage of H.M.S. *Challenger*, 1872-1876."

The back of the medal bears the crest of the *Challenger*—a mailed warrior throwing down the gauntlet to Neptune, whose trident appears above the waves. This central figure is surrounded by a scroll bearing the words, "Report on the Scientific Results of the *Challenger* Expedition, 1886-1895." The name of the recipient of each medal is engraved around the edge.

It is hardly necessary to say that the medal has been very much appreciated, and appears to have been received with special satisfaction by foreign contributors to the *Challenger* Report, who regard it as a pleasing recognition of their assistance in the great work which has now been completed.

DR. FRIEDRICH W. G. SPÖRER.

IN a recent number of NATURE we unfortunately had to record the loss of an astronomer, Dr. Friedrich Tietjen, who devoted himself to computation, or, we should say, to that branch of astronomy which deals with

the methods of calculation, and with the reduction of the observations themselves.

It is our lot to-day to say a few words about another hard worker in astronomical science, whose end has followed too soon after that of Dr. Tietjen. This devoted student of astronomy has been an energetic observer in the same degree that Dr. Tietjen was an ardent computer. We refer to Dr. Friedrich Wilhelm Gustav Sporer, the former chief assistant of the Astro-Physical Observatory at Potsdam, and who died on July 7 last.

Dr. Sporer was born in Berlin on October 23, 1822, and after spending some time at the Friedrich-Wilhelms Gymnasium, he entered the University of Berlin, making mathematics and astronomy his chief studies. On December 14, 1843, he gained his doctor's degree, the subject of his thesis being the comet of 1723. In the following years he worked under Encke's direction at the Berlin Observatory, and in 1846, after having made his Staats exam., went as a teacher of mathematics and natural science to the Gymnasium at Bromberg. In 1847 he proceeded to Prenzlau, and two years later to Anclam, at which latter place he taught for twenty-five years, and became eventually Pro-rector.

It was during his leisure hours there that Dr. Sporer was able to turn his attention to astronomical observations, his instrumental equipment being of a very inferior kind. Notwithstanding this hindrance, he was able, however, by great diligence and perseverance, to make useful observations with regard to the statistics of the solar spots, which have made his name known to every worker of solar physics. Through the attention of Prof. Schellbach, who was the teacher of the then Crown Prince Friedrich Wilhelm, afterwards Kaiser Friedrich, Dr. Sporer was equipped with a good 5-inch telescope, with which he continued to make his solar observations by the known method of projection. His Anclam observations appeared from time to time in numerous articles contributed to the *Astronomischen Nachrichten*, and also in two larger papers which came out in the years 1874 and 1876 in the *Publicationen der Astronomischen Gesellschaft*. The chief value of these pieces of work lies in the careful determination of the elements of rotation of the sun, and also in the more accurate settlement of the then empirically known law of Carrington, namely, the decrease in the velocity of rotation of the sun-spots according to increase of solar latitude.

In the year 1868, accompanied by Prof. Tietjen and Dr. Engelmann, Dr. Sporer took part in the astronomical expedition to observe the total eclipse of the sun visible in the East Indies. Six years later (1874) he received the appointment as observer at the Potsdam Astro-Physical Observatory, and in the same year continued his solar observations from the top of the tower of the Military Orphan Asylum, until the completion of the observatory.

Here Dr. Sporer, with untiring energy and with the same ardour that he displayed in Anclam, did a great amount of work in collecting data on the subject of sun-spots. The publications of the Astro-Physical Observatory (years 1879–1894) contain four valuable papers by him, giving a rich quantity of accurate observations that will remain a classical work for the study of the proper motion of the solar spots.

In 1882 Dr. Sporer became chief assistant, and this position he held until October 1894, when he retired for a well-earned rest.

From Dr. Sporer's observations of solar spots, the most important deductions that have been made may be summed up as follows:—

(1) That the period of rotation of the apparent surface of the sun about its axis, is not the same for every part.

(2) That the velocity of the spots is greater nearer the equator than further away from it, and that this velocity can be approximately represented by a formula.

(3) That the variation in latitude is periodical, and that there are two series of spots. We learn thus that the true sun-spot cycle is one extending over twelve to fourteen years, and that another begins in high latitudes before the former has ceased.

(4) His observations of the quantity of spotted area between the years 1856–1880, show a length of period of eleven years, this being the time between two consecutive maxima.

The maximum is reached when the mean latitude of the spots is about 16° north and south. A retreat then takes place from about 30° to 16°, that is, 14° in four years, and a further retreat from 16° to 8°, that is, 8° in eight years; or, in other words, we get a change of latitude of over 3° a year to begin with, and one of $\frac{1}{2}$° a year to end with.

Such results as these, which have here only been briefly summarised, are of fundamental importance, and form valuable data for those attempting to investigate the conditions of atmospheric circulation at the surface of our sun. Since the observations have been made consecutively by such a diligent observer, and extend over a considerable period of time, they are strictly of a uniform nature, and in consequence they are comparable *inter se*.

Happy in his work, and endowed with a strong constitution, Dr. Spörer was free from the ailings of old age up to his last day. It was when on a journey to visit his children that he was suddenly seized with paralysis of the heart, without ever having had any previous sign of illness, and died quietly and without pain.

His loss not only affects the astronomical world, but his large circle of friends, all of whom will mourn deeply such a sudden and unexpected bereavement.

W. J. S. L.

NOTES.

Considerable activity has been displayed at the Plymouth laboratory of the Marine Biological Association during the present summer, and general satisfaction has been experienced by the naturalists who have visited the station for the purpose of research. Progress has been made with the series of dredging operations in the outlying grounds of the neighbourhood. The unsettled weather of the past two months has been a somewhat unfavourable condition in these expeditions; but it is expected that these operations may be carried on regularly and with increased success during the autumn months. The following naturalists have occupied tables at the laboratory during the summer: Prof. Weldon, F.R.S., Mr. G. P. Bidder, Mr. W. Garstang, Mr. T. H. Riches, Dr. Albrecht Bethe, Mr. W. J. Beaumont, Mr. Gilchrist, and others.

A well-marked earthquake disturbance was felt at Zermatt on Wednesday, August 21. Many houses were severely shaken.

Among the deaths of eminent scientific men abroad, we notice the name of Dr. F. Hoppe-Seyler, professor of physiological chemistry in Strassburg University, and also that of Dr. S. Moos, professor of otology in Heidelberg University.

We regret to record the death of Dr. J. S. Bristowe, F.R.S., whose work on the "Theory and Practice of Medicine" is recognised as a classic, while his other contributions to scientific literature give him a high place among medical worthies. Dr. Bristowe had filled the offices of President of the Medical Society, of the Pathological Society, and of the Neurological Society. He was elected into the Royal Society in June 1881.

THE Assistant Clerk to the Geological Society, Mr. F. E. Brown, died suddenly on Sunday, August 4. The Society loses in him an invaluable official, who was ever rigid in the exact performance of all his duties, and combined with strict business-like habits a courtesy and patience which endeared him to his colleagues and to the Fellows generally.

THE eleventh Congress of Americanists will be held in the City of Mexico, on October 15-20. The meeting has for its principal object the progress of ethnographical, linguistic, and historical studies of the two Americas, especially with reference to the period prior to the discovery of the New World. Among the matters which will be discussed at the forthcoming gathering are the following:—The relations existing between different American peoples before the discovery; maps of the Atlantic and Pacific Oceans in the sixteenth century; medical natural history of the Ancient Mexicans; public instruction in Mexico in early times, and from the conquest of Mexico to the middle of the sixteenth century; mines and metallurgy before the conquest of Mexico; interpretation of the symbolic dances of the Azetics; different forms of arrows and their use among the natives of Central America; recent researches with regard to the first appearance of man in America; relationships between the Esquimaux and other native races of North America; prehistoric man in Mexico; the stone carvings in Central America; the pottery of Nicaragua and Costa Rica; the chronological classification of the monuments of Mexico and Central America; the human inhabitants of caves and grottos; Indian hieroglyphics; names of animals in the native languages of Central America; the decipherment and comparison of the hieroglyphics of ancient races of Mexico; the use of hieroglyphic writing since the conquest of Mexico, and the importance of its study in connection with the Mexican and Mayan languages. The President of the Congress is Sr. J. Baranda, and the Secretary, Sr. T. S. Santos, to whom all memoirs and other communications should be addressed at the Bibliothèque Nationale, Mexico.

DURING the latter part of last week the area of high barometric pressure that had prevailed over the greater part of the British Islands gave way to small disturbances, which either approached from the Atlantic, or were formed immediately over this country, causing severe thunderstorms over England and Ireland, while lightning was also visible in Scotland. In the storm of Thursday night (22nd inst.) the lightning was extremely brilliant in London, the flashes during part of the time being almost continuous. Considerable quantities of rain fell in many localities, and in some of the English districts much damage was done by hail.

THE problem solved by Edison's kinetoscope has been successfully attacked along a different line by MM. A. and L. Lumière. The film which in the kinetoscope takes the impressions of moving objects is passed before the eye with a continuous motion, and it is only illuminated for about a 7000th of a second at the instant at which each successive picture is fully in view. Hence the total illumination is exceedingly feeble. A very bright object is necessary; the eye has to be brought close to the moving film, and the number of impressions per second must be at least thirty in order to give continuity. MM. Lumière's "kinematograph," which is not subject to these disadvantages, is described in the *Revue Générale des Sciences*. The principal features of this instrument are a mechanism whereby the film is at rest during illumination, and an arrangement for projecting the images upon a screen, so as to be visible to a large meeting. Under these circumstances, fifteen images per second are all that is necessary. The film is at rest for two-thirds of the time of passage of each image. During the remaining third the film is grasped and pulled forward as far as the next image by a set of teeth attached to a frame whose motion is governed by a cam worked by a revolving handle. The same apparatus also serves as a camera for taking the photographs, and for printing transparencies from the negative film. For this purpose two films are passed over the rollers, the negative and the film to be printed on, and exposure is made for a very short time as each negative image is placed in the field. An exhibition was given on July 11, at the offices of the *Revue Générale des Sciences*, at which the evolutions of cuirassiers, a house on fire, a factory, street scenes, and a dinner-party were shown on the screen, and were much admired.

A NUMBER of observations referring to a shower of dust in connection with snow in Indiana and Kentucky, are brought together and discussed in the *Monthly Weather Review*. The dust does not appear to have been the nuclei of snowflakes, but was intermingled in the air with the snow, and fell during an interval between two snow-storms. An examination of numerous samples showed that the dust was made up largely of silt, mixed with organic matter. A number of freshwater algæ were distinguished, though they appear to have been dead and dried for some time. There were also groups of diatoms, fungi, animal and plant hairs, fibres of grasses, shreds of woody tissue of some shrub or tree, and many other objects in the samples examined. Everything indicated that the material came from the bottom of some dried-up lake, pond, or marsh, or some river-bottom. To afford information upon the belief that this fine material is very valuable as a fertiliser, an examination of the dust was made from that point of view. The analyses showed that the material is no better fertiliser than any other good surface soil. The dust was almost identical with the so-called "loess" formation, which covers very extensive areas in Illinois, Indiana, Nebraska, and other adjoining States; its depth in some places amounting to a hundred feet or more. This is interesting, because there is a long-standing controversy as to the origin of the loess formation of the North-west. Certain portions of the loess formation of Asia are known to be wind deposits, and there is very strong presumptive evidence, now borne out by the examination of the samples of dust, that much of the loess of the Western States is also a wind deposit. Special interest is thus attached to the dust-storm referred to, on account of the bearing of the observations on the question of the formation of agricultural soils, and especially the loess, which is the lightest and finest of all. This light soil is easily raised and carried by the strong winds of the western plains of America; instances have occurred in which six inches of surface soil have been blown away from freshly cultivated fields in the course of a single wind-storm. Prof. Cleveland Abbe is of the opinion that the dust caught between the two layers of snow in Indiana, probably did not differ materially from that which is daily present in the atmosphere of that region, but its presence on the top of a layer of snow rendered it easy to gather the dust-fall without contamination with the soil already existing. So this dust formation, or loess, when it has once settled upon the ordinary soils, becomes a new ingredient in their composition, and is therefore well worth further study.

A USEFUL bulletin, on the pasteurisation of milk and cream for direct consumption, has been issued from the Agricultural Experiment Station of the University of Wisconsin. It is drawn up by Dr. H. L. Russell, the bacteriologist attached to the station, and contains much interesting matter. There can be no doubt whatever that the pasteurisation of milk is a most important hygienic measure, destroying as it does an average of about 99·7 per cent of the microbes present in milk, amongst which are the diphtheria and typhoid microbes, as well as those organisms associated with gastric and intestinal disturbances so common in young infants during the summer. It is claimed

that the introduction of pasteurised milk among the poor people of New York, through the philanthropic efforts of Mr. Nathan Straus, has done much to reduce the infant mortality in that city during the hot summer months. The practical side of the question has not been lost sight of by Dr. Russell, and the results of his experiments on the efficient production and distribution of pasteurised milk on a commercial scale are carefully brought together. The subject is one of great importance, both from a hygienic as well as commercial point of view, and we may surely hope that before long our dairy authorities will take the matter up, and that we shall follow, though tardily, the example already set us by our neighbours in France and Germany, where pasteurised milk may be purchased across the counter.

THE volume of "British Rainfall" for 1894, compiled by Mr. G. J. Symons and Mr. H. Sowerby Wallis, from observations made at more than three thousand stations in the British Isles, has just been published. As in previous years, the volume contains articles upon various branches of rainfall work, and upon rainfalls of exceptional interest.

DR. TH. WOLF has contributed to the *Verhandlungen der Gesellschaft fur Erdkunde zu Berlin* (Bd. xxii. Nos. 4 and 5, 1895, pp. 246–265, pl. iii.) a detailed sketch of the Galapagos Islands, describing their geology, in some detail, with shorter accounts of the botany and zoology. He denies that there are any grounds for Dr. Baur's theory that the islands were once connected with the mainland of South America.

WE have on our table the *Journal* of the Royal Agricultural and Commercial Society of British Guiana, containing two papers of scientific interest, viz. "Cane Cultivation in the Straits Settlements," by Mr. F. Campen, and "A Journey to the Summit of Roraima," by Mr. J. J. Quelch; also the *Journal* of the Institute of Jamaica, which, though mostly taken up with matters of historical interest, contains several notes on local natural history topics, and a note on the discovery of aboriginal Indian remains in the Port Royal Mountains, already described in these columns by Mr. J. E. Duerden (p. 173).

THE report of the Royal Prussian Meteorological Institute for the year 1894 draws attention to two points: the completion of the arrangements for magnetic observations at the Potsdam Observatory, and the conclusion of a number of balloon ascents made during the year. The results of these ascents will be made the subject of a special investigation; one of the balloons, sent up with registering instruments only, reached an altitude of over sixty thousand feet. The report shows that many important publications have been issued, both officially, and in various periodicals, by members of the staff; some of these papers have been noticed in our columns. The laboratory experiments carried on by the Institute are of a high order, and have attracted the attention of scientific men in various countries.

THE Royal Horticultural Society's *Journal* for August has in it several important papers. There is a report of the Primula Conference, held a short time back with the idea of increasing and improving the culture of the various species of Primula by procuring new plants from remote regions; by practising the most successful methods of culture; and by producing hybrids. A paper on the botanical work done on the genus Primula since the last conference in 1886 was contributed by Mr. J. G. Baker, F.R.S., and this is printed with one on the culture and classification of Primulas, by Mr. H. Selfe-Leonard, and another on the Auricula, by Mr. J. Douglas. Among the other papers in the *Journal*, we notice a long and very valuable description of the plants and gardens of the Canary Isles, by Dr. Morris, C.M.G., and a paper on the culture of roses under glass, by Mr. F. Cant.

DR. K. SAPPER has supplemented his recent memoir, "Bemerkungen über die raümliche Verteilung und morphologischen Eigentümlichkeiten der Vulkane Guatemalas" (*Zeit. deut. geol. Ges.*, Bd. xlv. 1893), by a further account of the topography of some of the less-known volcanos. (*Petermann's Mitth.* Bd. xli. No. 5, 1895, pp. 105–109, pl. vii.) In spite of the fact that the volcanos of Guatemala have been repeatedly examined during the last half-century, and described in Dollfus and Montserrat's classical work, many of them were almost unknown. Dr. Sapper now describes the volcanos of Acatenango, 3950 m., which consists of five craters in line; San Pedro, 3050m., on which no trace of recent volcanic action remains, for the mountain is wooded to the summit, and the crater has been destroyed; and a group of western volcanos. He was anxious to explore the previously unknown Lacandon, which if proved to be volcanic would fill up a gap in the chain. He was unable to ascend the mountain, but saw sufficient to render it almost certain that Lacandon is a volcano of the first order.

THE Madras Government Museum is, to judge from the Administration Report for the year 1894–95, a very progressive institution. Mr. Edgar Thurston, the superintendent, appears to be sparing no efforts to make the museum more valuable for educational purposes, and for reference in connection with natural history, economic, and other subjects, and also more attractive to the ordinary sight-seer. The increase in the number of visitors to the museum during the year—from 311,112 to 368,282—shows that his efforts are appreciated. We notice with interest, that an entirely new departure was made, during the year covered by the report, by the commencement of a detailed anthropological survey of the races, castes, and tribes which inhabit Southern India. The Madras Government express in the report their satisfaction that the survey has been set on foot. Mr. Thurston has already collected sufficient evidence to make it clear that his investigation will prove of great interest and value.

WHEN Mr. Alfred Daniell's "Text-Book of the Principles of Physics" (Macmillan) appeared, eleven years ago, it was at once hailed as an original work, and a decided acquisition to the literature of physics. The third edition, which is now before us, maintains the characteristics of the original issue. At the time when the work was designed, it was possible for a medical student to obtain the degree of Doctor of Medicine without any adequate knowledge of physics. "That arrangement," Mr. Daniell then wrote, "is self-evidently opposed to common-sense, and to the exigencies of physiological study and of medical practice; such an anomaly cannot, it may be anticipated, endure much longer. Before many years are over, it will be universally acknowledged in practice, as it already is in theory, that knowledge of natural philosophy is an essential part of the mental equipment of the medical student and of the properly-trained medical man." It is satisfactory to be able to record that Mr. Daniell's prognostication was fulfilled in 1892, when the new regulations of the General Medical Council came into force, and it is also gratifying to know that medicine is every day becoming more truly scientific in its methods and objects. Mr. Daniell's work is by no means only suited for a medical class-room; it is alike useful to all students of science. The leading principles of physical science are set forth in the pages of the book in language the precision and accuracy of which make the volume welcome to all who study physics.

WE have received from the Deutsche Seewarte the first supplement to the principal catalogue of its valuable library, which now contains some seventeen thousand works relating to meteoro-

logical and kindred sciences, and includes the important collection of the late Prof. H. Dove. It is arranged under subjects, with the titles under each entered according to authors or institutions, while an alphabetical index at the end facilitates the reference to the subject catalogue. Opinions differ as to the best method of publishing such a work, the strictly alphabetical arrangement, such as followed by Prof. G. Hellmann in his excellent *Repertorium der Deutschen Meteorologie*, or the Royal Society's catalogue of scientific papers, possesses great advantages, and obviates the necessity of indexing one book under several sections; but as the Seewarte originally adopted another method, it has perhaps done well to keep to the same plan, and has rendered good service to science by its careful preparation and timely publication of the catalogue. The first part was issued in the year 1890.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♂) from India, presented by Mr. Hugh H. Collis; a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. E. Laundy; a Vervet Monkey (*Cercopithecus lalandii*, ♀) from South Africa, presented by Mrs. Edward Webb; two Brown Capuchins (*Cebus fatuellus*) from Guiana, presented by Major W. S. D. Liardet; two Black-eared Marmosets (*Hapale penicillata*) from South-east Brazil, presented by Mrs. H. V. Friend; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Mr. J. Lewis; a Purple-capped Lory (*Lorius domicella*) from Moluccas, presented by Mr. T. Bailey; two Tarantula Spiders (*Mygale*, sp. inc.) from Trinidad, presented by Mr. J. Hoadley; six Grey Parrots (*Psittacus erithacus*) from West Africa, deposited; a Collared Fruit Bat (*Cynonycteris collaris*), a Ypecha Rail (*Aramides ypecaha*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

REAPPEARANCE OF SWIFT'S COMET.—The *Edinburgh Circular*, No. 44, publishes a telegram from Kiel announcing that Comet Swift was seen by Mr. E. E. Barnard, at the Lick Observatory, on the 20th and 21st inst. The comet is described as faint, and its position and daily motion are given as follows:—

Local Mean Time.	R.A.	Decl.
h. m.	h. m. s.	° ′ ″
1895, August 21, 11 23·7 ...	0 30 11·4. ...	+5 38 55
Daily Motion ...	+2 48 ...	+ 10

THE LATITUDE VARIATION TIDE.—One of the most interesting outcomes of the recognition of the variability of the earth's axis of rotation has been the search for the tide, corresponding to the latitude variation. The separation of the axis of rotation from the axis of figure must cause at any point on the earth's surface successive divergences of the sea-level, from that which would exist if the figure of the earth remained a fixed ellipsoid of revolution. This consideration naturally led to the inquiry whether a small oscillation in the mean sea-level could be actually detected, having the same period as the displacement of the pole. The earliest results published were those obtained by Dr. Bakhuyzen (*Astr. Nach.* No. 3261), who used the tidal observations for the years 1855 to 1892, registered on a mareograph at the Helder, and these results showed a satisfactory agreement with those deduced from astronomical observations.

In the meantime Mr. A. S. Christie has been at work on the records made at the United States Coast Survey mareograph stations, and his results, embodied in a paper read before the Philosophical Society of Washington, are now before us. The paper is divided into two sections, the first of which is devoted to the derivation of the formulæ necessary for the elimination of the effects of other tides, and the second contains the results of the application of these formulæ.

The observations employed are obtained from two series, made at stations in the vicinity of San Francisco, namely, at Fort Point (1856–70) and Sausalito (1877–91). Mr. Christie has also used a similar series made at Pulpit Harbour, Penobscot Bay, Maine (1870–88). It will be sufficient to give here the final result arrived at by combining the results at San Francisco and Pulpit Harbour. The period deduced is 431 ± 4 days, and the value of the half-range tide is 15 ± 2 mm.; while the dates at which the critical phases of the tide occurred are:—

	San Francisco.	Pulpit Harbour.
Min.,	1872, July 15 ± 15 days ...	1878, August 22 ± 10 days.
Max.,	1873, Feb. 15 ± 15 ,, ...	1879, March 25 ± 10 ,,

Dr. Bakhuyzen's value of the half-range is 8·2 mm., a result that does not differ greatly from the mean here given, 15 mm., or from either of the two results, 17·4 mm. and 12·5 mm., on which this value rests.

Reduced to the latitude of Berlin, we have another comparison between the investigations of the American and Dutch astronomers, and the results are still fairly satisfactory, as shown below:—

Julian Date of Maximum Latitude of Berlin.

Bakhuyzen,	from astronomical observations	2405141 Julian
,,	from discussion of Helder tides ...	201
Christie,	from San Francisco tides	153 ± 16

It seems possible, therefore, that this difficult question of the motion of the earth's pole may be attacked by two quite separate processes.

THE SOLAR PARALLAX FROM MARS OBSERVATIONS.—With the view of making a new and trustworthy determination of the solar parallax, a scheme was suggested in 1892 by the authorities of the Washington Observatory for the observation of the difference of declination at the time of meridian passage between Mars and a number of selected stars. The horizontal equatorial parallax of Mars reached in that year a maximum of 23″·4, a sufficiently favourable condition, though the small altitude of the planet in the northern observatories was likely to introduce considerable uncertainty in the amount of refraction. Among the observatories that replied to the invitation of Washington to take part in this scheme are those of Gotha and the Cape of Good Hope. The result of the combination of the two sets of observations has recently been published by Dr. Paul Harzer, and are of especial interest, since Gotha lies nearly on the northern limit of the region in which observations of Mars could be made with sufficient accuracy.

It was a part of the original suggestion—to which some exception was taken at the time—that in addition to the method of fixing the declination of the centre of Mars by the employment of a pair of wires, separated by about 16″ to cut off equal segments from the northern and southern limbs of the disc, a reflecting prism should be mounted outside the eyepiece, and that half the observations should be made with, and half without the use of this additional apparatus. The result of the precaution is shown in the following figures, in the case of the two observers who took part in the series:—

	Dr. Harzer.	Dr. Rohrbach.
Mars stars ...	+0″·253 ± 0·039 ...	−0·383 ± 0·129
Mars	−0·270 ± 0·091 ...	−0·523 ± 0·262

These figures imply that Dr. Harzer placed the stars too low and the planet too high with reference to the threads, Dr. Rohrbach, in both cases, too high.

The observations were continued from June 22 to September 23, and when combined in three groups, formed on the assumption that the error of the ephemeris is constant throughout each group, the resulting values of the solar parallax are—

Group I.	...	$\pi = 8''\cdot680 \pm 0\cdot081$
,, II.	...	$= 8\cdot890 \pm 0\cdot089$
,, III.	...	$= 8\cdot828 \pm 0\cdot065$

or combined into one, $\pi = 8''\cdot799 \pm 0''\cdot044$.

The complete combination of the whole series formed into 20 normal places, and in which the possible variation of the error of the ephemeris is also sought, gives $\pi = 8''\cdot800 \pm 0''\cdot039$, and the value of $d\delta$ is expressed in the form

$$d\delta = \frac{-1''\cdot147 + 0''\cdot288t}{\Delta},$$

where t and Δ are reckoned from August 7·000, and the unit for t is 50 days.

THE SUN'S PLACE IN NATURE.[1]

X.

The New Classification of the Stars.

I NOW pass to the new classification of stars which has been suggested by the totality of the facts which I have so far brought before you.

Although the first observations of stellar spectra were made by Fraunhofer, we owe to Rutherfurd the first attempt at classification. In December 1862 he wrote as follows:[2]

"The star spectra present such varieties that it is difficult to point out any mode of classification. For the present I divide them into three groups. First, those having many lines and bands and most nearly resembling the Sun, viz. Capella, β Geminorum, α Orionis, Aldebaran, γ Leonis, Arcturus, and β Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the Sun, and are white stars. The third group, comprising α Virginis, Rigel, &c., are also white stars, but show no lines; perhaps they contain no mineral substance, or are incandescent without flame.

"It is not my intention to hazard any conjecture based upon the foregoing observations; this is more properly the province of the chemist, and a great accumulation of accurate data should be obtained before making the daring attempt to proclaim any of the constituent elements of the stars."

This classification was followed up by Secchi, who practically adopted Rutherfurd's three groups, changing, however, the word group to type, and adding a fourth. On this point Dr. Gould, in his memoir[3] of Rutherfurd, writes as follows:

"I cannot forbear calling attention to the classification, [illegible] the same, subsequently published by Secchi without reference to this or to any of the other labours of Rutherfurd, and which is generally cited under Secchi's name." (See "[illegible]," p. 258, and "Translation," pp. 235 236.)

In [illegible] other subsequent classifications—and of course [illegible] our stars if we are to speak about them with [illegible], and to understand the relations of one body or [illegible] to another—it has been taken for granted that [illegible] whatever to do with stars, and that all the stars lie along one line of temperature, the highest temperature being at one end, and the lowest at the other. Such, at all events, is Vogel's view. Now we have to conclude that nebulæ are stars to be, and that some apparent stars are really nebulæ; and I think I have shown you sufficient justification for the idea that the undisturbed nebulæ are of relatively low temperature; hence we have bodies getting hotter as well as bodies getting cooler, and both must be provided for.

In 1873 Dr. Vogel brought out a new and much more detailed classification considerably extending the number of groupings employed by Rutherfurd and Secchi. This classification is based on the assumption that all stars began by being very hot, and that the various changes observed in the spectra are due to cooling,[1] and the presence of bright lines is considered as a matter of secondary importance only, and gives rise to sub-groupings only.

Dr. Scheiner has quite recently accepted this statement. He appeals to his new observations of the spectrum of magnesium as a "direct proof of the correctness of the physical interpretation of Vogel's spectral classes, according to which Class II. is developed by cooling from I., and III. by a further process of cooling from II." (*Astronomy and Astro-Physics*, 1894, p. 571.)

Pechüle was the first to object to Vogel's classification, mainly on the ground that Secchi's types 3 and 4 had been improperly brought together.[2] Now the views I have brought before you cut at the root of such a classification as this.

It is perhaps worth while in passing to point out that in the course of lectures I gave here in 1880 I stated, taking the then classification as a basis[4]:

"On the nebular hypothesis, supposing . . . that we started with ordinary cometary materials, then, on the beginning of a central condensation which in time is to become a star, as Kant and Laplace suggested, such central condensation should then give us a star of the fourth class. As the energy of condensation increased and the temperature got higher, the spectra would change through the third and second classes, till ultimately, *when the temperature was highest*, the first class spectrum would be reached. *On the slackening down of the temperature* of the now formed star, the spectra of the second, third, and fourth classes would then be reproduced, but, of course, now in the direct order."

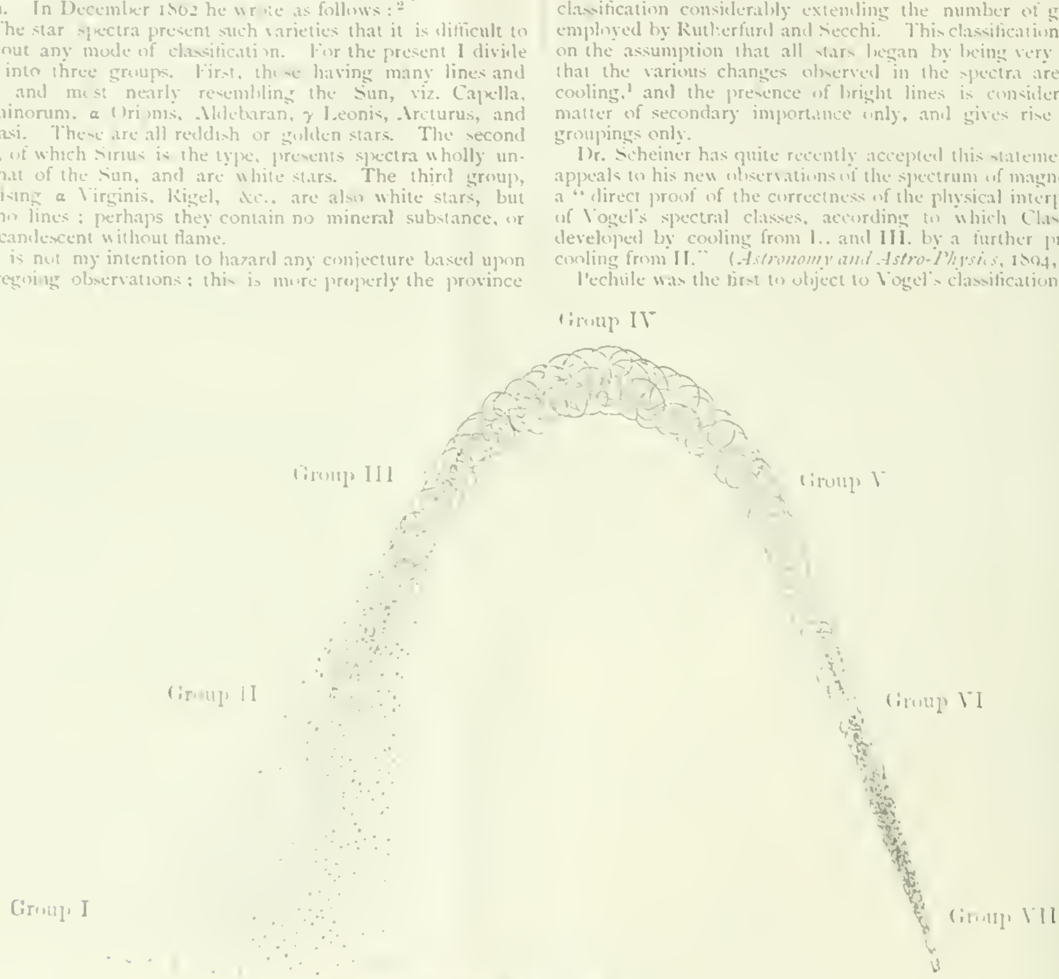


Fig. [illegible]. Temperature curve.

[1] [illegible] to Working Men [illegible] November and December, 1894 [illegible]

[2] [illegible]

[3] [illegible]

[1] "Selon [illegible] de la première [illegible]." Duner.

[2] [illegible] are given in my "Meteoritic Hypothesis," pp. [illegible]

[4] Lockyer, Nature, vol. xxxiv. [illegible]

We now know that this classification will not do, since all reference to bright lines is omitted, and every one now agrees that they must take the first place, and this is one of the great teachings of the views I have been bringing forward for the last ten years.

The idea which one arrives at by a discussion of all the spectroscopic facts is that we begin with a condition in which meteorites in swarms and streams are very far apart, and we get from the collisions of these a spectrum which gives us bright flutings and lines, in other words the spectrum of the nebulæ; when they get a little more dense, we get the bright-line stars: and as they get still more dense, we find the star with a mixture of bright and dark flutings. Then we get still more condensation and dark lines, and then the highest temperature of all; after which begins a descent on the other side, till at last we end in cool, dark bodies like the earth and moon.

This seems to be the classification which is necessitated by the consideration of all the facts, and it is, moreover, one which seems to give us possibilities of an explanation of the phenomena of new stars and variable stars, and many other things without going into the region of the unknown and impossible.

It also lands us in the so-called temperature curve along which I ventured to place the various classes of nebulæ and stars some time ago. I am glad to say that so far no valid objection has been made to it.

It will be noticed that in the classification I have suggested I use the word "group," first employed by Rutherfurd; it is one which ought never to have been changed.

With regard to this subject, Prof. Keeler, one of our most important authorities in this matter, agrees that a classification which depends on this temperature curve certainly has advantages over other systems. He writes [1]:—

"Prof. Lockyer's system of stellar classification provides for both an ascending and a descending branch of the temperature curve, and in this respect it certainly has advantages over other systems which claim to have a rational basis."

I am also more glad than I can say that Prof. Pickering, who has now given many years, with the aid of appliances beyond all precedent, to the study of these questions, has arrived at conclusions strikingly similar to my own.

In the first place he includes the nebulæ as well as the stars in his system: but it is right that I should add that he does not commit himself to any statements relating to the relative temperature of the different groups, although he distinctly accepts the idea of evolution, or what he terms an order of growth.

He writes (*Astronomy and Astro-Physics*, 1893, p. 722):

"In general, it may be stated that, with a few exceptions, all the stars may be arranged in a sequence, beginning with the planetary nebulæ, passing through the bright-line stars to the Orion stars, thence to the first type stars, and by insensible changes to the second and third type stars. The evidence that the same plan governs the construction of all parts of the visible universe is thus conclusive."

Prof. Pickering's results may be shown in tabular form, but first it will be well to show the general differences between the more recent classifications:

	Secchi.	Vogel.	Lockyer.
Nebulæ	Not classified.	Not classified.	Group I.
Bright-line stars ...	,,	,,	
Mixed fluting stars ..	Type III.	Class III*a*.	,, II.
Dark line stars (ascending)	,, II.	,, II.	,, III.
Broad hydrogen stars ...	,, I.	,, I.	,, IV.
Solar stars	,, II.	,, II.	,, V.
Carbon absorption stars...	,, IV.	,, III*b*.	,, VI.

In his classification, Prof. Pickering begins with the earliest stages, taking the planetary nebulæ and such nebulæ as that of Orion; he then comes to the bright-line stars, and then to such stars as those of Orion, and ultimately places the Sun, as I also do, after the spectrum of such a star as Sirius. There are practically two departures in his classification from that given by myself. One is that what I call the bright and dark fluting group of stars, represented by several of the red, and brightest, stars in the heavens, he makes cooler than the Sun. And the class of stars which I group together and call Group VI., in which we get mainly the absorption of carbon in the atmosphere, he omits altogether, possibly for a very wise reason, as they are certainly the most difficult stars to tackle; but you see the divergences in his classification from mine are small as compared with those between Dr. Vogel and myself, and he, I repeat, like myself, attributes the variation to an "order of growth."

[1] *Astronomy and Astro-Physics*, 1894, p. 60.

This premised, the differences of sequence between Prof. Pickering and myself may be shown as follows:

Lockyer.	Pickering.
I.	I.
II.	
III.	III.
IV.	IV.
V.	V.
VI.	II.

Prof. Pickering, in the Draper Catalogue, combines like stars under the different letters of the alphabet. The distribution of these letters in relation to my Groups is as follows:—

	Lockyer.	Pickering. (Draper catalogue.)
Nebulæ	I.	P. (Planetary Nebulæ.)
Bright-line stars...		O.
Mixed fluting stars	II.	M.
Dark-line stars (ascending) ...	III.	B. H. I. K. (?)
Broad hydrogen stars	IV.	A.
Solar stars	V.	F. G. K. L.
Carbon absorption stars ...	VI.	N.

It will be seen that certain groups are represented by more than one letter, but it is to be noted that here again Prof. Pickering and myself have arrived at very nearly similar results, for generally a different letter with him represents a sub-group with me. This will be gathered from the subjoined table.

Table showing the subdivisions of Groups III. and V.

Group.	Pickering.
III. α	H.
III. β	I. (some Q.)
III. γ	B.
V. α	F.
V. β	G.
V. γ	K. L.

With regard to Prof. Pickering, then, I have chiefly to justify the place I have given to the stars of my Group II., which I place after the nebulæ and bright-line stars, and he places after the Sun.

I fancy that one of the reasons which has led Prof. Pickering to this conclusion is to be found in the assumption that strong indications of calcium and iron can only mark one stage of growth, while I think it is certain they must mark two.

We know they mark the present stage of the Sun's history, and taking meteorites as we find them, a relatively low temperature would provide us with more calcium and iron vapours to act as absorbers round each one than anything else.

Now we have strong indications of calcium and iron absorption in such stars as α Herculis as well as in the Sun, but the general appearance of the spectra of these stars is so different that both Secchi and Vogel have classified them apart, and so indeed does Prof. Pickering.

But the reason that I classified these stars also in different groups, and one on the rising and the other on the descending arm of the temperature curve, was that in those like α Herculis we have enormous variability as well as bright lines and flutings indicative of sparse swarms, while in those like the Sun the production of such phenomena is almost unthinkable. The special variability of stars of my Group II. (Secchi's type III.) and the production of bright lines at maximum is now freely acknowledged. On this point Prof. Pickering remarks [1]:—

"Long period variables in general are of the third type, and have the hydrogen lines bright when near their maxima, as stated above. This property has led to the discovery of more

[1] *Astronomy and Astro-Physics*, 1893, p. 721.

than twenty objects of this class, and no exception has been found of a star having this spectrum whose light does not really vary. Of the variables of long period which have been discovered visually, the hydrogen lines have been photographed as bright in forty-one, the greater portion of the others being too fainter too red to be studied with our present means."

As said before, it seems impossible to imagine how our Sun, as it proceeds along its "order of growth," should change into a body with such characteristics as these. But on this point we must wait for more large scale photographic spectra; in other words, more facts.

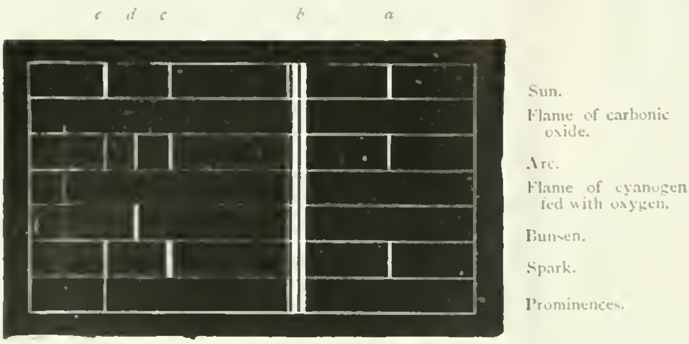


FIG. 39.—Showing the various intensities of the lines of magnesium as seen under different conditions.

Associated with this change in the order of evolution, Prof. Pickering classes the chief stars in Orion, such as Bellatrix, characterised by spectra containing hydrogen and a few other dark lines of unknown origin, as early forms. On this point I may also quote the following from Prof. Campbell (*Astronomy and Astro-Physics*, 1894, p. 475):—

"In conclusion, I think we can say, from the foregoing observations, that the spectra of the Wolf-Rayet stars are not closely related to any other known type. They appear to have several points in common with the nebular and Orion type spectra; but the last two appear to be much more closely related to each other than to the Wolf-Rayet spectra. It is therefore difficult to place these stars between the nebulæ and Orion stars. They certainly do not come *after* the Orion stars, and one does not like to place them before the nebulæ. We can probably say that the bright lines are chromospheric, owing their origin to very extensive and highly-heated atmospheres, but showing very little relation, in constitution and physical condition, to that of our own Sun. For the present, at least, this type of spectrum must be considered as distinct from every other known type, just as the nebular spectrum is distinct, and like the nebular spectrum containing lines whose origin cannot now be assigned."

Although Dr. Vogel and others apparently still hold in the main to the classification which assumes that all stars were created hot, and that nebulæ have nothing to do with them; that, in short, every star began in the highest stage of temperature, so that the whole history of every star in the heavens has been a process of cooling, there are signs of wavering here and there. Some of the definitions are being "edited" and re-edited to fit the facts which the photographic record is pouring in upon us. I may take, as an instance, the following statement made by Dr. Scheiner with reference to α Cygni, which is classified by Dr. Vogel as a solar star.

"These figures plainly show that the spectrum of α Cygni, in spite of the large number of its lines, has no resemblance with that of the sun. While it is possible to identify most of the lines with solar lines in respect to their position, yet the total lack of agreement as to intensity of the lines makes many of these identifications worthless."

The "figures" referred to are micrometer measures of a photograph. My experience in these matters is that it is a pure waste of time to measure a photograph until it has been compared with others to which it is important to refer it, enlarged up to the same scale. In this I think I carry Prof. Keeler with me (*Astronomy and Astro-Physics*, 1894, p. 485). "The coincidence of . . . lines is shown more beautifully by inspection of . . . photographs than by any process of measurement." Thus a comparison of the spectra of α Cygni and of the Sun *which Dr. Vogel classes together*, shows at once the dissimilarity pointed out above without any measurement whatever. I am glad, however, to find that Dr. Scheiner now regards the identification as "worthless," because it is such differences as these which have compelled me to reject Dr. Vogel's classification.

Dr. Scheiner then goes on:—

"The magnesium line at λ 4481 is the strongest in the entire spectrum. The other strong lines coincide for the most part with the fainter solar lines. The presence of numerous iron lines can be scarcely doubted, but here again we have the peculiar phenomenon that the fainter, instead of the stronger, lines occur. We may conclude from all these facts that very different conditions as to temperature must prevail in α Cygni from those in the stars of class Ia." (Scheiner's "Astronomical Spectroscopy," Frost's translation, p. 247.)

Much of the work of the future, which eventually must smooth down all differences between stellar classifications, must consist of the study of single lines in the spectra of different stars, and I am rejoiced to find that the Potsdam observers are at length beginning to take this matter up. Dr. Scheiner, one of the Potsdam assistants, has, as seen above, called attention to the behaviour of the line 4481 of magnesium, and agrees that the variations in the line observed are due to differences of temperature, and that therefore it may be used as a stellar thermometer.[1]

But for this work an acquaintance with the literature of the subject is desirable. Had Dr. Scheiner been acquainted with it, I am certain he would have done me the honour to quote, or at all events to refer to, a communication I made to the Royal Society (16 years ago!), pointing out that the line in question was visible only at high temperatures, and that such work would help us in the study of "the atmospheres of the hottest stars."[2] In the same connection, in the "Chemistry of the Sun," published in 1887, I gave the diagrams, here reproduced, indicating the lines, visible at various temperatures in the laboratory, and in the Sun and prominences.

Having said so much on the different classifications of stars, and indicated, I trust judicially, that the one suggested by the meteoritic hypothesis is so far holding its own, I now pass on to some recent work which was undertaken to test it by a limited photographic survey. In the first instance I had used the eye observations of others; a study of spectra, entirely photographic, it was hoped would enable an independent estimate to be formed as to the validity of the hypothesis.

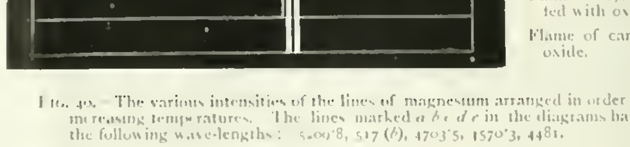


FIG. 40.—The various intensities of the lines of magnesium arranged in order of increasing temperatures. The lines marked *a b c d e* in the diagrams have the following wave-lengths: 5209·8, 517 (*b*), 4703·5, 4570·3, 4481.

The conclusions I came to in the first instance were necessarily based on observations made by others, for the reason that my own work up to that time had been chiefly directed to the Sun.

So soon, however, as my solar work rendered it necessary to determine the Sun's true place among the stars in regard to its

[1] "Astronomical Spectroscopy," p. viii.

[2] *Roy. Soc. Proc.* vol. xxx. p. 22, 1879.

temperature and physical conditions, arrangements were made to photograph the spectra of stars and nebulæ, in order to test the view, employing a quite new basis of facts; this new basis of the inquiry consists of 443 photographs of 171 of the brighter stars.

Having this new and accurate basis of induction, the objects were to determine whether the hypothesis founded on eye observations is also demanded by the photographs, and in the affirmative case to discover and apply new tests of its validity, or otherwise.

The results as yet obtained are not sufficient to permit a discussion of all points bearing upon the new classification, but most of the crucial ones are certainly covered by the photographs already obtained.

The main instrument employed in the work has been a 6-inch refracting telescope, with an object-glass made and corrected for G by the Brothers Henry. This was at first used in conjunction with a prism of $7\frac{1}{2}°$ of dense glass by Hilger. The object-glass and prism are fixed at the end of a wooden tube, which is attached to the side of the 10-inch equatorial, at such an angle that the spectrum of a star falls on the middle of the photographic plate when its image is at the centre of the field of the larger instrument. The camera is arranged to take plates of the ordinary commercial size, $4\frac{1}{4} \times 3\frac{1}{4}$ inches. The spectra obtained with this instrument are 0·6 inch long from F to K. An excellent photograph of the spectrum of a first magnitude star can be obtained with an exposure of five minutes. Afterwards a 6-inch prism, with a refracting angle of 45°, obtained from the Brothers Henry, was used with the Henry 6-inch object glass. The spectra obtained with the latter are two inches long from F to K, and the definition is exquisite. In some photographs the calcium line at H is very clearly separated from the line of hydrogen, which occupies very nearly the same position. It is unnecessary to swing the back of the camera in order to get a perfect focus from F to K. The deviation of the prism is so great that it would be very inconvenient to incline the tube which supports it at the proper angle to the larger telescope. When photographing the spectrum of a star, therefore, the star is first brought to the centre of the field of the large telescope, and the proper deviation is then given by reading off on the declination circle. This method has been found to work quite satisfactorily.

FIG. 41.—Objective prism fitted to object-glass.

With this combination the exposure required for a first magnitude star is about twenty minutes. The method of mounting the prism is shown in Fig. 41.

For the fainter stars, the 6-inch prism of $7\frac{1}{2}°$ has been adapted to a Dallmeyer rectilinear lens of 6 inches aperture and 48 inches focal length. At times prisms of $7\frac{1}{2}°$ have been used on a 10-inch equatorial.

Since the spectrum of a point of light such as a star is a line so fine that the spectral lines would not be measurable, it is necessary to give it breadth. This is done by adjusting the prism so that the spectrum lies along a meridian of R.A. and altering the rate of the clock.

J. NORMAN LOCKYER.

(*To be continued.*)

THE IRON AND STEEL INSTITUTE.

THE annual summer meeting of the Iron and Steel Institute was held in Birmingham last week, commencing Tuesday, the 20th inst., and extending over Friday, the 23rd inst. Sir David Dale, the President, took the chair at the sittings for the reading of papers, and it may be said here that the meeting was remarkably successful throughout, being one of the pleasantest and most instructive gatherings that has been held for a long time past; both Mr. Brough, the Secretary of the Institute, and the local committee are to be congratulated on the excellence of their arrangements.

There were twelve papers down for reading and discussion, of which the following is a list:—

"On the Direct Puddling of Iron," by E. Bonehill (Marchienne-au-Pont, Belgium).

"On the Production of Iron by a New Process," by R. A. Hadfield, member of Council (Sheffield).

"On the Thermo-Chemistry of the Bessemer Process," by Prof. W. N. Hartley, F.R.S. (Dublin).

"On the Hardening of Steel," by H. M. Howe (Boston, U.S.A.).

"On the Mineral Resources of South Staffordshire," by H. W. Hughes (Dudley).

"On the Iron Industry of South Staffordshire," by D. Jones, Secretary of the South Staffordshire Ironmasters' Association (Shifnal).

"On the Iron Industry of the South of Russia," by George Kamensky (St. Petersburg).

"On Cooling Curves and Tests of Cast Iron," by W. J. Keep (Detroit, U.S.A.).

"On the Analysis of Ferro-Chromium," by E. H. Saniter (Wigan).

"On Small Cast Ingots," by R. Smith-Casson (Birmingham).

"On Tests of Cast Iron," by T. D. West (Sharpsville, Pennsylvania).

"On Nickel Steel," by H. A. Wiggin (Birmingham).

The papers of Mr. West and Mr. Keep were taken as read, all the others being read and discussed.

On the members assembling on Tuesday morning, in the Council House of Birmingham Corporation, they were welcomed by the Mayor, and by the members of the local reception committee.

The first paper taken was that by Mr. D. Jones, on the iron industry of South Staffordshire. This was an interesting contribution, but mainly historical in its character. It dealt with the rise and progress of the iron industry of the district from its earliest days, and, in treating of more modern times, pointed out how the production of wrought-iron had decreased as steel had taken its place, although a good deal of puddled iron is still produced in the district. The paper of Mr. Hughes, on the mineral resources of South Staffordshire, was very much of the same character, and gave, in a convenient form, many facts relating to the subject.

M. Bonehill's paper on the direct puddling of iron was next read. This process appears to be a revival of, and doubtless an improvement on, a method of puddling which was proposed, and to a limited extent carried out, in the earlier years of the century, but which never obtained any great hold in the iron

industry. It consists, briefly, in running molten iron from the blast furnace into a reservoir, and from thence letting it flow into the puddling furnace, the latter being of larger description than is generally used. It is obvious that with this process, as compared to the ordinary method of feeding the puddling furnace with cold pig, there is a saving of fuel, inasmuch as the metal does not require melting: on the other hand, the difficulty of getting a uniform product, owing to the inability to mix various kinds of pig, has to be overcome. Apparently the author has been successful in the latter respect, although how he has accomplished his end was not stated in the paper; the tests given, however, indicate that a superior quality of iron is produced.

Mr. Kamensky's paper on the iron industries of South Russia was, like the two first contributions, of an historical nature. In this case, however, there was less of ancient history in the memoir, and necessarily so, as the production of iron in Russia, as an industry of importance, is of essentially modern growth. It is true that iron-making has been carried on in Russia for a long time past, but it is only within the last year or two that any great strides have been made. Now, however, there are several works in operation, and it appears likely that more will follow: so Russia may in her turn put in a claim for a share of the opening markets of the world. This is a fact that British steelmakers may perhaps look on not altogether with satisfaction; but it is inevitable. Only by increased exertion can British manufacturers maintain their position in the markets of the world; but there is one point, however, worthy of attention. If Russia is about to start many steel works, large quantities of plant and machinery will be required. It is proposed that the Institute shall next year hold its summer meeting in Russia. The suggestion is a bold one, but is worthy of consideration, for it is only by pushing abroad that steel makers can hope to keep abreast of the times. The days are past when the manufacture of iron and steel was almost entirely centred in England. Now there are works all over the world, under intelligent and scientific management. It is unreasonable to expect that we, in England, will continue to originate all new and valuable processes, and it is well, therefore, that English manufacturers should go abroad to reap the advantages of foreign research and practice; just as foreign manufacturers have in times past, and are still, reaping the advantage of English experience and study.

The reading and discussion of the above four papers constituted the business of the first sitting. The afternoon of that day—Tuesday, the 20th inst.—was devoted to visits to works. One party proceeded to the Staffordshire Steel and Ingot Iron Company's establishment at Bilston, where the operations of rolling sections and plates were witnessed. A large quantity of basic steel is produced at these works; and the method of dealing with the basic slag, which is largely used for agricultural purposes, was inspected with interest by the members. Another party visited the Electric Construction Company's works at Wolverhampton; whilst, again, others distributed themselves amongst various works in Birmingham.

On assembling again on Wednesday morning the first paper taken was that contributed by Prof. Hartley, on the thermochemistry of the Bessemer process. This was an exceedingly interesting paper, which those engaged in subjects of this nature would do well to read in full in the *Transactions* of the Institute. The author commenced by saying that the flame issuing from the mouth of a Bessemer converter was first investigated by Sir Henry Roscoe in 1863 (see Manchester Literary and Philosophical Society's *Proceedings*, vol. iii. p. 57, and *Philosophical Magazine*, vol. xxxiv. p. 437); by Leilegg (see *Sitzungsberichte Kaiserl. Akademie der Wissenschaften*, Wien, vol. lvi. part ii.); and by Marshall Watts in 1867 (see *Philosophical Magazine*, vol. xxxiv. p. 437); by Tunner (see Dingler's *Polytechnisches Journal*, vol. clxxviii. p. 465); by J. M. Silliman (see *Philosophical Magazine*, vol. xli. p. 1); by von Lichtenfels (see Dingler's *Polytechnisches Journal*, vol. cxci. p. 213); by Spear Parker (*Chemical News*, vol. xxii. p. 25); by Kupelwieser (*Oesterreichische Zeitschrift für Berg- und Hüttenwesen*, No. 8, p. 59, 1868); by Brunner and Wedding in 1868 (see *Zeitschrift für das Berg-Hütten-und Salinenwesen im preussischen Staate*, vol. xvii. p. 117, 1869); and also by A. Greiner in 1874 (see *Revue Universelle des Mines*, vol. xxxv. p. 625). Up to the present time the nature of the spectrum, the cause of its production, and sudden disappearance when decarburisation of the metal takes place, and the connection between the decarburisation of the metal and the extinction of the spectrum, have not been satisfactorily explained. According to Roscoe, Leilegg, Kupelwieser, and Spear Parker, the spectrum is characterised by bands of carbon or of carbon monoxide, which disappear when all the carbon is burnt out of the metal. On the other hand, Simmler, Brunner, von Lichtenfels, and Wedding hold that the spectrum is not due to carbon, or to carbon monoxide, but to manganese and other elements in pig iron. Dr. Marshall Watts had come to the conclusion that it was not the spectrum of carbon in any form, nor of manganese, but that of manganetic oxide. Leilegg proved that carbon monoxide yields a continuous spectrum, which causes the bright spectrum of the Bessemer flame; but he also attributed certain lines, or bands, to the high temperature of the carbon monoxide. Marshall Watts established the fact that six lines of the spectrum of iron were present in the Bessemer spectrum; Greiner observed in flame from highly manganiferous pig iron the spectrum of manganese. The author concluded this part of his paper by pointing out the fact that notwithstanding the great advance which has been made in spectroscopy during the last twenty years, our knowledge of flame spectra has remained almost stationary, although much attention has been directed to the spectra of the elements as we obtain them at higher temperatures by vapourising substances in the electric arc, and by the transmission of electric sparks.

Prof. Hartley next proceeded to describe a method of accurately investigating the Bessemer flame. He pointed out that the determination of wave-length of lines and bands by eye observation only, with instruments of the usual form, is laborious under the most advantageous conditions, but it is especially so when the spectra are constantly changing; and it becomes practically impossible when the lines and bands to be measured are in the ultra-violet. Spectra which are recorded by photography are capable of being more accurately measured at leisure by very simple means; moreover, they constitute a permanent record; and for accurate observations, determinations of wave-lengths are absolutely essential. The author next went on to describe a modification of the instrument he had originally designed for this purpose. This is described in the *Proceedings* of the Royal Dublin Society, and also in Thorpe's "Dictionary of Applied Chemistry," article "Spectroscope." This instrument was especially designed for use in steel work, particularly for studying the spectra of flames and heated gases of open-hearth furnaces. It was therefore desirable that it should give a fair amount of dispersion at the less refrangible end of the spectrum. A train of four quartz prisms was at first arranged, and a camera was fitted with a rack and pinion movement to the frame holding the dark slide, so that as many as thirty spectra could be photographed on one plate. The stand, however, was found to be too light. Instead of four quartz prisms, a single prism of calcite may be employed if the surfaces are well protected from dust; the prism table was fixed so that it could be placed in almost any required position. The camera was of metal with an eye-piece behind the frame for the dark slide, so as to make it available for visual observation. In a circular box at the end of the camera, which was reduced in size, the dark slide can be fixed at any angle, as it is rotated by means of a toothed wheel. The prisms move automatically with the camera, and in order to secure the minimum angle of deviation to the mean rays photographed there is a condensing lens of 3-inch focus. There is a slit plate, covered with thin quartz to exclude dust and dirt, and upon this the image of the flame was projected. A metal plate, with a V-shaped piece cut out at one end, slides over the slit plate, and serves to shorten or lengthen the slit and secure a greater or smaller number of spectra on one photographic plate. In some cases a photograph was taken every half-minute, from the commencement to the termination of the "blow." This could be accomplished only by the use of the arrangement described, as the plates were no more than 3 inches by $2\frac{1}{2}$ inches. The instrument was focussed by a photograph of sun spectra.

The author also described an ingenious arrangement consisting of yellow cloth, with armholes and sleeves fitted with elastic, by means of which he carried on development of the photographs without use of a dark room. By this apparatus it was shown that a large number of lines in the spectrum of the Bessemer flame were coincident with lines in the solar spectrum, and the position of the lines and edges of bands with respect to the sodium line was recorded, being measured with a micrometer screw and microscope. Enlargements were made in which the spectra were magnified ten diameters. Several interpolation curves were drawn by which linear measurements were

reduced to oscillation-frequencies, and by means of Barlow's mathematical tables these were reduced to wave-lengths which are the reciprocals of the oscillation-frequencies. The author then went on to describe some of the difficulties met with in obtaining measurements of bands, due to alterations in width, or to their becoming less distinct at the edges. The question is dealt with in "Flame Spectra of High Temperatures," *Philosophical Transactions*, 1894, part 1.

Prof. Hartley had carried out experiments at Crewe, and at Dowlais, in South Wales. Results obtained by photography of the spectrum of the Bessemer flame were given in the paper. For the details we must refer our readers to the original memoir. As the author pointed out, the Bessemer spectrum is a complex one, which exhibits differences in constitution during different periods of the blow, and even during different intervals of the same period. Watts had observed that the spectrum differs in different works, owing to variations of temperature and the composition of the metal blown. After discussing the various opinions held by previous investigators as to the utility of spectrum analysis in steel making—on which subject inquirers are by no means agreed the cause of the non-appearance of lines at the termination of the blow is discussed. Prof. Hartley then proceeded to what was perhaps the most interesting part of his paper, namely, the temperature of the Bessemer metal and of the flame, and the use of the spectrum as an index of temperature. Watts concluded that though the temperature of the flame was above the melting point of gold, it was below that of platinum. Le Chatelier (*Comptes rendus*, vol. cxiv. p. 670) was of opinion that the temperature of the Bessemer converter during the boil is 1330° C., at the finish 1580° C., while the steel in the ladle is at 1640° C. There is no measure of the temperature at the hottest period of the boil, and unless the metal in the converter is cooled during the last minute of the blow, which some of the author's photographs indicated, it was difficult to understand how its temperature could be raised by the addition of the cooler spiegeleisen and ferro-manganese. The rise of temperature at this period could be accounted for by the after-blow. Of course when the metal is charged with oxygen, the additional spiegeleisen, containing carbon and manganese, would cause the combustion of these elements. When the oxyhydrogen flame spectra of the manganese, magnetic oxide of iron, and ferric oxide are photographed, the number of lines and bands in the spectra are not more numerous than with a Bessemer flame spectrum of only half a minute's exposure, although the above spectra may have received any exposure from thirty to eighty minutes. When a substance emits a spectrum composed of bands and lines, it is evidence of the presence of the substance in the flame in a state of glowing vapour; when the same substance emits two spectra, one differing from the other by the largely increased number of bands or lines, it is evidence that either the substance is more copiously vapourised, or that the temperature of the vapour is higher. When a simple spectrum changes to one of a more complex character, the alteration is due to an increase in temperature, other things being equal. Similarly when a spectrum extends through the visible rays into the ultra-violet region, and an increase is observed in the number and intensity of the ultra-violet rays, nothing but an increase of temperature will serve to account for the change in the spectrum. No increase of material in the flame would increase the refrangibility of the rays emitted by its vapour; hence the study of the ultra-violet spectra of flames by the photographic method becomes an important line of investigation.

After pointing out the difficulty of ascertaining the maximum temperature of any flame (as such temperature may exist over but a very small area), and giving an instance, the author states that Le Chatelier's recent measurements of the temperature of furnaces have given numbers considerably lower than those usually accepted. Langley's estimate of the temperature of the Bessemer flame at 2000° C. because platinum appears to be rapidly melted in it—is not to be relied upon. Le Chatelier finds that the metal is not fused but dissolved in drops of molten steel. Marshall Watts observed that the sodium lines 5681 and 5687 may be employed as an index of temperature, since they are present in the spectrum of any flame containing sodium the temperature of which is hot enough to melt platinum, but they do not appear at lower temperatures. The Bessemer flame does not show this double line, but only the D lines, neither does it show lithium orange lines, which appear at a somewhat lower temperature. It may therefore be concluded that the flame is not hot enough to produce these lines. The proportion of sodium in the Bessemer flame is evidently very small from the narrowness and want of intensity of the D lines, and the fact that they are not seen reversed in any spectrum; hence, though the temperature may be high enough, the quantity of material present is not sufficiently large to yield the lines 5681 and 5687.

We have not space to follow the author in all the interesting details of his reasoning, but we have perhaps said enough to indicate his line of thought. He later points out that, judging by the number of lines and bands belonging to iron and manganese, which have been photographed in the spectrum of the Bessemer flame, the temperature must in any case nearly approach that of the oxyhydrogen flame, even if it does not very generally exceed it. The paper concluded with particulars of the heat of combustion of the oxidisable impurities in pig iron. He calculates, as far as data are available, the absolute heating effect of such oxidation. The temperature retained according to these calculations amounts to 1454° C. above that of molten cast iron. This, however, is a theoretical value, and allowance must be made for the specific heats of the gases, the metal, and the slag, which are greater at the elevated temperatures than at the temperatures at which the numbers representing specific heats were determined. The specific heat of the converter must be considerable, but it must be remembered that it is already heated to the temperature of the molten metal; but even if we allow that 50 per cent. of the heat is absorbed, or conveyed away, we should then have the temperature 727° C. above that of the molten pig iron; and thus, with grey iron, at 1220° C. the metal may have acquired a temperature of more than 1947° C., which is very considerably above the melting point of platinum.

The discussion which followed the reading of this paper was interesting, but no new points of importance were added. Mr. Bauerman considered that the author was right in laying stress on the temperature of the flame as well as on the materials in the converter. Mr. J. Stead pointed out that some of the calculations were made in cases where the composition of the metal was very different to that common in England. Mr. Tucker also pointed out the difficulty in arriving at any conclusion owing to the variation in metal used, and he referred to the effect of a temperature of dissociation which might be obtained if the metal were sufficiently rich in silicon. His own experiments supported those of Prof. Hartley, that the temperature was certainly at times considerably above the melting point of platinum, and he was inclined to think that the temperature of dissociation was often reached.

The next paper was also one of considerable scientific interest. It was Mr. Howe's contribution on the hardening of steel, and was read in abstract by Mr. Brough, the Secretary of the Institute. As the paper had been received so recently, copies of it had not been distributed, and it was manifestly impossible to discuss a memoir of this abstruse nature at first sight, especially as the paper was not read in full. It was therefore wisely determined to have the text corrected, after which the paper will be distributed, and its discussion taken at the next meeting in May. For the present, it will suffice to say that the author deals largely with the vexed problem of the allotropic state of iron. It would have been a pity to have discussed the paper on the spot, as neither Prof. Roberts-Austen nor Prof. Arnold were present; neither had M. Osmond been able to send his usual written contribution. In fact, the only person present whose name has become at all prominently identified with the states of iron treated was Mr. Hadfield, who spoke briefly, saying that he had not had time to master the paper. We will, therefore, defer our abstract of this memoir until the time comes to give an account of the next meeting.

Mr. R. A. Hadfield's paper on the production of iron by a new process was next read. The author's object has been to obtain a pure iron; for which purpose he had had recourse to aluminium as an agent. The first result was that he made an alloy of iron and aluminium very rich in the latter constituent, there being no less than 36 per cent. present. In spite of being a failure, so far as the object in view was concerned, a very interesting result was obtained; for although there was no more than a trace of carbon present, the alloy was hard enough to scratch glass. Proceeding on the same lines, however, and working with ferrous oxide and granulated aluminium, a sample of iron containing 99·75 per cent. of that metal was finally obtained at the very moderate cost of about eighteen pence per pound.

Mr. Saniter's paper, describing a new method for the analysis of chrome and ferro-chromium, was the last read at this sitting. This is a further extension of Mr. Stead's modification of Dr. Clarke's process, and has the great advantage of reducing the time occupied in the analysis.

On the afternoon of this day there were several excursions, the chief of which was to Worcester, where the works of the Royal Porcelain Company were inspected. Another party visited the Round Oak Iron and Steel Works, while others proceeded to the glass works, fireclay works, small arms factories, and to other works in and around Birmingham. In the evening there was a very successful reception and entertainment in the Edgbaston Botanical Gardens.

The final sitting of the meeting was on Thursday of last week, when a paper by Mr. Henry Wiggin, on nickel steel, was first taken. In this contribution the advantages of nickel steel as a constructive material were brought forward; its great tensile strength combined with excessive ductility being dwelt upon. Another advantage possessed is freedom from corrosion, as compared with ordinary steel. Instances were given of the nickel steel containing 3½ per cent. of nickel, which had a tensile strength fully 30 per cent. higher than ordinary steel, and an elastic limit at least 75 per cent. higher. The author does not give any details in regard to cost, which is naturally higher than that of ordinary steel; but speaking upon the subject generally, he was of opinion that the additional price that would have to be charged would generally be more than compensated for by increased efficiency. In the discussion, Mr. W. Beardmore, of Glasgow, said he had been making large quantities of nickel steel for the last two years. This was for armour-plates, but he was now preparing a series of tests to submit to Lloyd's with a view to introducing the material for marine purposes. Mr. Jeremiah Head, who had lately visited America, said that at the works of Mr. Carnegie he had seen large quantities of nickel steel produced at a cost, he was told, of about £7 a ton; but there natural gas of great richness was available. Mr. Thompson, of New York, who had been largely engaged in the manufacture of nickel steel, said that in America 50,000 to 75,000 tons of this material had been produced during the last three years. A German chemist had found that with an alloy of 15 per cent. of nickel almost a new metal was made having a tensile strength of 244,000 lbs. to the square inch, and an elastic limit as high in proportion. He estimated that to build a large battleship of nickel steel would add but 2 per cent. to her cost, whilst the efficiency would be doubled. Mr. Thomas Turner afterwards pointed out that nickel steel was supposed to have a wide range of extension and contraction with variations of temperature, so that if a ship went to the polar regions it might become even feet shorter in its length.

Mr. Smith-Casson's paper, on small cast ingots, was next read. The author claims to have got very good results by casting ingots together from the bottom. This was the last paper read at the meeting.

Thursday afternoon was devoted to an excursion to Stratford-on-Avon, whilst on the following day, Friday, an excursion was made to Kenilworth and to Warwick, where members and their friends were entertained at the Castle by Lord and Lady Warwick.

THE SPECTRUM OF HELIUM.[1]

IN the *Chemical News* for March 29 last (vol. lxxi. p. 151), I published the results of measurements of the wave-lengths of the more prominent lines seen in the spectrum of the gas from clèveite, now identified with helium. The gas had been given to me by the discoverer, Prof. Ramsay; and being from the first batch prepared, it contained other gases as impurities, such as nitrogen and aqueous vapour, both of which gave spectra interfering with the purity of the true helium spectrum. I have since, thanks to the kindness of Profs. Ramsay and J. Norman Lockyer, had an opportunity of examining samples of helium from different minerals and of considerable purity as far as known contamination is concerned. These samples of gas were sealed in tubes of various kinds and exhausted to the most luminous point for spectrum observations. In most cases no internal electrodes were used, but the rarefied gas was illuminated solely by induction, metallic terminals being attached to the outside of the tube.[1] For photographic purposes a quartz window was attached to the end of the tube, so that the spectrum of the gas could be taken "end on."

[1] From the *Chemical News*, August 2[illegible].

My examinations have chiefly been made on five samples of gas.

(1) A sample from Prof. Ramsay in March last. Prepared from clèveite.

(2) A sample from Prof. Ramsay in May last. Prepared from a specimen of uraninite sent to him by Prof. Hillebrand. Gas obtained by means of sulphuric acid; purified by sparking.

(3) A sample from Prof. Ramsay in June last. Prepared from bröggerite.

(4) A sample from Prof. Lockyer in July last. Prepared by a process of fractional distillation from a sample of bröggerite sent by Prof. Brögger.

(5) A sample of gas from Prof. Ramsay, "Helium Purissimum." This was obtained from mixed sources, and had been purified to the highest possible point.

In the following table the first four samples of gas will be called:—(1) "Clèveite, R."; (2) "Uraninite, R"; (3) "Bröggerite, R"; and (4) "Bröggerite, L." Only the strongest of the lines, and those about which I have no doubt, are given. The wave-lengths are on Rowland's scale.

The photographs were taken on plates bent to the proper curvature for bringing the whole spectrum in accurate focus at the same time. The spectrum given by a spark between an alloy of equal atoms of mercury, cadmium, zinc, and tin, was photographed at the same time on the plate, partially overlapping the helium spectrum; suitable lines of these metals were used as standards. The measurements were taken by means of a special micrometer reading approximately to the 1/100,000th inch, and with accuracy to the 1/10,000th of an inch. The calculations were performed according to Sir George Stokes's formula, supplemented by an additional formula kindly supplied by Sir George Stokes, giving a correction to be applied to the approximate wave-lengths given by the first formula, and greatly increasing the accuracy of the results.

Wave-length.	Intensity.	
7065·5	5	A red line, seen in all the samples of gas. Young gives a chromospheric line at 7065·5.
6678·1	8	A red line, seen in all the samples of gas. Thalén gives a line at 6677, and Lockyer at 6678. Young gives a chromospheric line at 6678·3.
5876·0	30	The characteristic yellow line of helium, seen in all the samples of gas. Thalén makes it 5875·9, and Rowland 5875·98. Young gives a chromospheric line at 5876.
5062·15	3	
5047·1	5	A yellow-green line, only seen in "Helium Puriss." and in "Bröggerite, R," and "L." Thalén gives the wave-length as 5048.
5015·9	7	A green line seen in all the samples of gas. Thalén gives the wave-length 5016. Young gives a chromospheric line at 5015·9.
4931·9	3	
4922·6	10	A green line, seen in all the samples of gas. Thalén gives the wave-length 4922. Young gives a chromospheric line at 4922·3.
4870·6	7	A green line, only seen in "Uraninite, R." Young gives a chromospheric line at 4870·4.
4847·3	7	A green line, only seen in "Uraninite, R." Young gives a chromospheric line at 4848·7.
4805·6	9	A green line, only seen in "Uraninite, R." Young gives a chromospheric line at 4805·25.
4764·4	2	There is a hydrogen line at 4764·0.
4735·1	10	A very strong greenish blue line, only seen in "Uraninite, R."
4713·4	9	A blue line, seen in all the samples of gas. Thalén's measurement is 4713·5. Young gives a chromospheric line at 4713·4.
4658·5	8	A blue line, only seen in "Uraninite, R."
4579·1	3	A faint blue line, seen in "Uraninite, R." Lockyer gives a line at 4580, from certain minerals. I can see no traces of it in the gas from Bröggerite. A hydrogen line occurs at 4580·1.

[1] *Journal of the Institution of Electrical Engineers*, part 91, vol. xx., Inaugural Address by the President, William Crookes, F.R.S., Jan. 15, 1891.

Wave-length.	Intensity.	
4559·4	2	Young gives a chromospheric line at 4558·9.
4544·1	5	
4520·9	3	A faint blue line, seen in "Uraninite, R." Lockyer gives a line at 4522, seen in the gas from some minerals. Young gives a chromospheric line at 4522·9. It is absent in the gas from Bröggerite.
4511·4	5	A blue line, seen in "Uraninite, R," but not in the others. It is coincident with the strong head of a carbon band in the CO_2 and Cy spectrum.
4497·8	2	There is a hydrogen line at 4498·75.
4471·5	10	A very strong blue line, having a fainter line on each side, forming a close triplet. It is a prominent line in all the samples of gas examined. Young gives the wave-length 4471·8 for a line in the chromosphere, and Lockyer gives 4471 for a line in gas from Bröggerite.
4435·7	9	Seen in "Helium Puriss."
4437·1	1	Young gives a chromospheric line at 4437·2.
4428·1	10	These two lines form a close pair. I can only see them in "Uraninite, R." No trace of them can be seen in the gases from other sources. Young gives chromospheric lines at 4426·6 and 4425·6.
4424·0	10	
4399·0	10	A strong line, only seen in "Uraninite, R." Absent in the gas from the other sources. Lockyer gives a line at 4398 in gas from certain minerals. Young gives a chromospheric line at 4398·9.
4386·3	6	Seen in all the samples of gas. Young gives a chromospheric line at 4385·4.
4378·8	8	These two lines form a pair seen in "Uraninite, R," but entirely absent in the others.
4371.0	8	
4348·4	10	Seen in "Uraninite, R." Lockyer finds a line at 4347 in the gas from certain minerals.
4333·9	10	Probably a very close double line. Seen in "Uraninite, R," and "Clèveite, R." Not seen in the other samples. Lockyer gives a line in the gas from certain minerals at 4338.
4298·7	6	Only seen in "Uraninite, R." Young gives a chromospheric line at 4298·5.
4281·3	5	Only seen in "Uraninite, R."
4271·0	5	Only seen in "Uraninite, R." The strong head of a nitrogen band occurs close to this line.
4258·8	7	Seen in all the samples of gas.
4227·1	5	Only seen in "Uraninite, R." Young gives a chromospheric line at 4226·89
4198·6	9	These three lines form a prominent group in "Uraninite, R," they are very faint in "Clèveite, R," and in Bröggerite, L," but are not seen in "Bröggerite, R."
4189·9	9	
4181·5	9	
4178·1	1	An extremely faint line. Lockyer gives a line at 4177, seen in the gas from certain minerals, and Young gives a chromospheric line at 4179·5.
4169·4	6	Seen in "Helium Puriss."
4157·6	8	A strong line in "Uraninite, R," very faint in "Bröggerite, R," and "L," not seen in "Clèveite, R."
4143·9	7	Strong in "Clèveite, R," in "Helium Puriss.," and in "Bröggerite, L." It is faint in "Uraninite, R," and not seen in "Bröggerite, R." Lockyer gives a line at 4145 in gas from certain minerals.
4121·3	7	Present in all the gases except "Clèveite, R."
4044·3	9	Present in "Uraninite, R," and "Clèveite, R." Absent in the others.
4026·1	10	These lines form a very close pair, seen in all the samples of gas, except "Bröggerite, R." Lockyer finds a line in Bröggerite gas at 4026·5.
4024·15	6	
4012·9	7	Seen in all the samples of gas.
4009·2	7	Seen in "Helium Puriss."
3964·8	10	The centre line of a dense triplet. Only seen in "Clèveite, R," in Helium Puriss.," and "Bröggerite, L." Hale gives a chromospheric line at 3964.
3962·3	4	Seen in all the samples of gas.
3948·2	10	Very strong in "Uraninite, R," very faint in "Clèveite, R," and not seen in the others. Lockyer finds a line in gas from Bröggerite at 3947. There is an eclipse line at the same wave-length.
3925·8	2	Seen in "Helium Puriss."
3917·0	2	Seen in "Helium Puriss."
3913·2	4	Only seen in "Uraninite, R," and "Helium Puriss." Hale gives a chromospheric line at 3913·5.
3890·5	9	A very strong triplet, seen in all the samples of gas. Lockyer finds a line having a wave-length 3889 in gas from Bröggerite. Hale gives a chromospheric line at 3888·73. There is a strong hydrogen line at 3889·15
3888·5	10	
3885·9	9	
3874·6	6	Only seen in "Uraninite, R."
3867·7	8	Seen in "Helium Puriss."
3819·4	10	Seen in all the samples of gas. Deslandres gives a chromospheric line at 3819·8.
3800·6	4	Seen in "Helium Puriss."
3732·5	5	Seen in "Helium Puriss." Hale gives a chromospheric line at 3733·3
3705·4	6	Seen in all the samples of gas. Deslandres gives a chromospheric line at 3705·9.
3642·0	8	Only seen in "Uraninite, R."
3633·3	8	Seen in "Helium Puriss."
3627·8	5	Only seen in "Uraninite, R."
3613·7	9	Seen in "Helium Puriss."
3587·0	5	Seen in "Helium Puriss."
3447·8	8	Seen in "Helium Puriss."
3353·8	5	Seen in "Helium Puriss."
3247·5	2	Seen in "Helium Puriss."
3187·3	10	The centre line of a close triplet. Very faint in "Clèveite, R," and "Uraninite, R." and strong in "Helium Puriss." and in "Bröggerite, L." It is not seen in "Bröggerite, R."
2944·9	8	A prominent line, only seen in "Helium Puriss." and in "Bröggerite, L."
2536·5	8	Seen in "Helium Puriss." A mercury line occurs at 2536·72.
2479·1	4	Seen in "Helium Puriss."
2446·4	2	Seen in "Helium Puriss."
2419·8	2	Seen in "Helium Puriss."

Some of the more refrangible lines may possibly be due to the presence of a carbon compound with the helium. To photograph them, a long exposure, extending over several hours, is necessary. The quartz window has to be cemented to the glass with an organic cement, and the long-continued action of the powerful induction current on the organic matter decomposes it, and fills the more refrangible end of the spectrum with lines and bands in which some of the flutings of hydrocarbon, cyanogen, and carbonic anhydride are to be distinguished.

There is a great difference in the relative intensities of the same lines in the gas from different minerals. Besides the case mentioned by Prof. Kayser of the yellow and green lines, 5876 and 5016, which vary in strength to such a degree as to render it highly probable that they represent two different elements, I have found many similar cases of lines which are relatively faint or absent in gas from one source and strong in that from another source.

Noticing only the strongest lines, which I have called "Intensity 10," "9," or "8," and taking no account of them when present in traces in other minerals, the following appear to be special to the gas from uraninite:—

4735·1
4658·5
4428·1
4424·0
4399·0
4378·8
4371·0
4348·4
4198·6
4189·9
4181·5
4157·6
3948·2
3642·0

The following strong lines are present in all the samples of gas:—

7065·5
6678·1
5876·0
5015·9
4922·6
4713·4
4471·5
4386·3
4258·8
4012·9
3962·3
3890·5
3888·5
3885·9
3819·4
3705·4

The distribution assigned to some of the lines in the above tables is subject to correction. The intensities are deduced from an examination of photographs, taken with very varied exposures; some having been exposed long enough to bring out the fainter lines, and some a short time to give details of structure in the stronger lines. Unless all the photographs have been exposed for the same time, there is a liability of the relative intensities of lines in one picture not being the same as those in another picture. Judgment is needed in deciding whether a line is to have an intensity of 7 or 8 assigned to it; and as in the tables I have not included lines below intensity 8, it might happen that another series of photographs with independent measurements of intensities would in some degree alter the above arrangement.

In the following table I have given a list of lines which are probably identical with lines observed in the chromosphere and prominences:—

Wave-lengths observed in [illegible].	Intensities.	Wave-lengths of chromospheric lines,[1] Rowland's scale.
7065·5	10	7065·5
6678·1	10	6678·3
5876·0	30	5876·0
5015·6	6	5015·9
4922·6	10	4922·3
4870·6	7	4870·4
4847·3	7	4848·7
4805·6	9	4805·25
4713·4	9	4713·4
4559·4	2	4558·9
4520·9	3	4522·9
4471·5	10	4471·8
4437·1	1	4437·2
4428·1	10	4426·6
4424·0	10	4425·6
4399·0	10	4398·9
4386·3	6	4385·4
4208·7	6	4208·5
4227·1	5	4226·80
4178·1	1	4179·5
3964·8	10	3964·0 H[2]
3948·2	10	3945·2 H
3913·2	4	3913·5 H
3888·5	10	3888·73 H
3819·4	10	3819·8 D
3733·5	5	3733·3
3705·4	6	3705·9 D

W. CROOKES.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

M[illegible] A[illegible] S[illegible], well known as the builder of the [illegible] tunnel on the Comstock Lode in Nevada, and now Mayor of San Francisco, has just offered the State University Regents thirteen acres of land within the city limits, on which to erect buildings for the affiliated colleges of the University. In addition to this, he will deed to the Trustee of the city thirteen acres adjoining, as a site for the Sutro library of over 200,000 volumes. The gift is valued at £300,000, and will be worth £400,000 when the contemplated improvements are made.

THE Clothworkers' Exhibition, awarded by the Oxford and Cambridge Schools Examination Board to the best candidate in physical science at the examination held for higher certificates, has been gained by T. W. Fagan, Denstone College, Staffordshire. The exhibition, which is of the value of £52 10s. a year, is tenable for three years by the holder as a non-collegiate student at either Oxford or Cambridge.

MR. W. M. GARDNER, Assistant Lecturer in Dyeing in the Yorkshire College, Leeds, has been appointed Head Master of the Chemistry and Dyeing Department of the Bradford Technical College.

THE Calendar of the Durham College of Science, Newcastle-upon-Tyne, for the session 1895–96 has just been published, and also separate prospectuses of the day and evening classes.

SIR A. ROLLIT asked the First Lord of the Treasury on Tuesday whether the Government intended, and when, to propose legislation in pursuance of the report of the Gresham Commission or the University of London. In reply, Mr. Balfour said that legislation will be impossible on the subject in the course of the present Session, and he was unable to say what action will be taken by the Government.

THE operations of the City and Guilds of London Institute are divided broadly into two branches, viz. the educational work of three London Colleges, and of the Technological Examinations. The new edition of the programme of the latter, including regulations for the registration and inspection of classes in technology and manual training, has come to hand. It is more bulky than any of the previous programmes of the examinations, which fact may be taken as an indication that the Institute is growing with the extension of technical education. The technical subjects in which examinations are held now number sixty-three. A practical examination for "electric wiremen" has been added, and a corresponding addition has been made to the syllabus for the preliminary examination in electric lighting. The syllabuses of several other subjects have been modified, and that in wood-work has been rewritten.

THE forty-second Report of the Department of Science and Art has just been received. A noteworthy point shown by the statistics contained in it is the diminution in the number of science schools, classes, and students under instruction, brought about by the abolition of grants for second class passes in the Elementary Stages of Science subjects. As compared with the previous year, the number of schools in 1894 had decreased by 152—from 2754 to 2602; the number of pupils had decreased by about 10,000—from 193,431 to 183,120; and the number of classes in different branches of science had decreased by 908—from 10,341 to 9433. This diminution is attributed to the changes in payments on results, "and also probably to the opening of numerous technical classes by the local authorities in different parts of the country, which have drawn away the students from the classes in pure science. The decrease in the number of schools and classes is owing partly to the same cause, and partly to the amalgamation of smaller schools, or to their absorption in the more prosperous and better supported schools in their neighbourhood, many classes in which instruction of a very elementary nature only had been given being at the same time closed." A determination has been made of the average ages at which students in the Department's science classes obtained successes in the Elementary and Advanced Stages. It was found that the average age in Day Schools for a student to obtain a first class elementary success was about 14, and for a second class Advanced about 15½, while in the evening classes the ages were respectively about 18 and 21. In addition to statistics, and information as to science instruction and technical education, the Report contains the reports of the work of the Geological Survey of the United Kingdom, and of the Committee on Solar Physics.

1 "A [illegible] Astronomy of [illegible]," by Dr. J. Scheiner, [illegible] 1894.

2 [illegible] D [illegible] H [illegible] are wave-[illegible] of the chromo-[illegible] (D) [illegible] Ha (H). [illegible] extend [illegible] of Lockyer [illegible] M[illegible] [illegible] between the wave-[illegible] in the chromo-[illegible] the [illegible].

SCIENTIFIC SERIALS.

American Journal of Science, August.—The earth a magnetic shell, by Frank H. Bigelow. This paper gives the vectors of the polar magnetic field at the earth due to the sun, together with certain deductions from their intensity and distribution. Unless the magnetic permeability of the interior of the earth is less than 1, which is highly improbable, the polar vectors obtained must be interpreted as stream lines flowing round an obstacle in the interior of the earth. In other words, the outer stratum of the earth is permeable to the external magnetic forces, while the nucleus is not; that is to say, the earth is a magnetic shell. The diameter of this impermeable nucleus is calculated at 6340 miles, and the thickness of the shell at 790 miles. The external polar field is concentrated in two belts, one of which is the auroral zone round the poles, and the tropical belt at the two tropics. It is a pity that most magnetic observatories are placed on the mid-latitude depression. Since both the magnetic and the electromagnetic vectors represent cosmical forces of the same type as gravitation, connecting the sun with the planets, they should be taken into account in general theoretical astronomy, or the celestial mechanics of the solar system. It is possible that certain irregular motions as yet unexplained may be accounted for on the basis of these additional forces.—On the velocity of electric waves, by John Trowbridge and William Duane. The apparatus used for photographing successive sparks whose images were thrown on the plate by a revolving mirror, was substantially the same as previously described; but the dielectric used was plate glass, and the terminals were made of cadmium. The average value for the velocity of electric waves travelling along two parallel wires was $3{\cdot}0024 \times 10^{10}$ cm. per second, a value which differs from the velocity of light by less than 2 per cent. of its value, and from the ratio between the two systems of electromagnetic units by even less.—On the distribution and the secular variation of terrestrial magnetism, by L. A. Bauer. The distribution and the secular variation appear to be closely related, they obey similar laws, and seem to be connected in some way with the rotation of the earth. The following are some of the laws traced by the author: The mean declination along a parallel of latitude is always westerly, the minimum occurring near the equator. The mean inclination along a parallel of latitude follows quite closely the law: $\tan I = 2 \tan \phi$ where I is the inclination and ϕ the geographical latitude. The minimum range in declination, and the minimum average secular change from 1780 to 1885 along a parallel of latitude occurred near the equator, the values generally increasing upon leaving the equator. The corresponding values in the case of inclination were maxima, and decrease upon leaving the equator.—Complementary rocks and radial dykes, by L. V. Pirsson. "Complementary rocks" are such that if the basic types are combined with the accompanying acid types, they give the composition of the main type of magma with which they are associated.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 1.—Dr. E. Lindon Mellus gave the results of experimental lesions of the cortex cerebri in the Bonnet Monkey. The experiments were confined to the left hemisphere, and consisted in the removal of minute portions of the cortex (generally about 16 sq. mm.) representing the centres for movements of the hallux and thumb, as well as several centres within the facial area. The animals recovered from the operation without any sign of sepsis, and were killed from ten to thirty-five days after the operation, the brains and cords hardened in Müller's fluid, and stained by the Marchi method. Numerous association fibres, both coarse and fine, connecting the lesion with the surrounding cortex, were found degenerated. These were always most numerous in the immediate neighbourhood of the lesion, and mostly distributed to the two central convolutions.

From lesions in the hallux centre degenerated association fibres were distributed to both central convolutions to the level of the inferior genu of the fissure of Rolando, to the parietal lobule, to the posterior portion of the superior frontal convolution, to the lobulus paracentralis, precuneus, and the gyrus fornicatus. Degenerate fibres crossed in the middle third of the corpus callosum and were distributed to corresponding portions of the right cortex, the degeneration on the right side being considerably less than on the left. In the lower levels of the left internal capsule the degeneration was scattered over the area of the middle third of the posterior limb, being somewhat anterior to its position in higher levels. From the posterior limb of the internal capsule most of the fine degeneration passed into the optic thalamus, while the coarse passed on into the crus, where it was found in the middle third. Many coarse degenerate fibres passed from the crus into the substantia nigra. At the decussation of the pyramids the tract divides, the larger portion crossing to the opposite lateral column, while the smaller goes to that of the same side. The amount of degeneration passing to the lateral column of the same (left) side varies from a third of all the degeneration in one case to about a twentieth in the others. In each case a few degenerate fibres remain in the left anterior column after the completion of the decussation. The amount varies in different cases, and is not apparently dependent on the proportion of degenerate fibres passing to the lateral column of the same side. The relations and extent of the degenerated areas remain unchanged throughout the cervical and dorsal cord. The degeneration in the crossed tract of each side is evenly scattered over its entire area, the two sides only differing in the density of the degeneration. In the lumbar region the degeneration in each crossed tract and in the left anterior column begins to go out, and, in the only case examined at that level, the degeneration had not all disappeared at the level of the third sacral root.

In lesions of the thumb centre (ascending parietal convolution just above the inferior genu of the fissure of Rolando) degenerated association fibres were distributed to the central convolutions from the border of the longitudinal fissure nearly to the fissure of Sylvius. To a less degree, but in varying amounts, degenerate fibres were traced to the posterior portions of the middle and inferior frontal convolutions, to the supra marginal and angular gyri, the upper or posterior portion of the superior temporal convolution, the precuneus and lobus quadratus and paracentralis and the gyrus fornicatus. Degenerate fibres crossing in the middle third of the corpus callosum were distributed to the corresponding convolutions of the right side, though less in amount and area of distribution. There was a remarkable variation in the size of the fibres distributed to the central convolutions of both hemispheres, being coarse above the level of the lesion and fine below, thus corresponding with the measurements made by Bevan Lewis of the corpuscles of the fourth layer of the cortex in this region. The arrangement and distribution of the degeneration in the posterior limb of the left internal capsule was the same as in lesions of the hallux centre, and there was the same passage of fine degeneration from the capsule to the thalamus. The amount of coarse degeneration passing from the crus to the substantia nigra was much greater than in lesions of the hallux centre, varying from a half to nearly the whole of the degeneration reaching the crus. In one case only was there a division of the degenerated tract at the decussation of the pyramids such as was observed in lesions of the hallux centre, and the amount of degeneration passing to the left lateral column was less than in either of the hallux cases. This was also the only case in which a few degenerate fibres remained in the left anterior column after the completion of the decussation. In two cases some degeneration was found in the right capsule and crus occupying the same position and following the same course as the degenerate fibres in the left capsule and crus, but its direct connection with the lesion could not be demonstrated. From the level of the seventh cervical root downward the degenerate fibres steadily and gradually disappeared, and at the level of the third dorsal root there were none left, thus confirming the results obtained by excitation of the nerve roots.

The lesions within the facial area were, with one exception, along the upper border of the fissure of Sylvius. The single exception was in the ascending frontal convolution near the inferior genu of the fissure of Rolando. In all these experiments the degenerate association fibres were mostly distributed to the central convolutions, but in some instances to the posterior portions of the middle and inferior frontal, the superior and inferior temporal convolutions, and the supra marginal gyrus. The degeneration in the corpus callosum was mostly in the anterior half of the middle third, and the distribution of degenerate fibres to the convolutions of the right hemisphere more nearly corresponded to that of the left than in lesions of the hallux or thumb centre. In all the lesions of the facial area the degenerations in the uppermost levels of the capsule were in the anterior portion, gradually moving backward in the lower levels until they were

found in the same position (the middle third of the posterior limb) as the degenerations resulting from lesions of the hallux and thumb centres. In this backward movement of the facial fibres in the capsule there is necessarily a level in which they envelope the genu, which would account for the fact that they are generally described as occupying that position. As in the other lesions, most of the fine degeneration passed from the internal capsule to the thalamus. In the crus the degeneration was scattered pretty evenly over the area of the middle third, exactly corresponding to the situation of the pyramidal fibres in the other experiments, and not occupying the position usually assigned to them, mesial to the pyramidal fibres. No degeneration was found in the accessory bundle to the fillet. As in the other experiments, degenerate fibres were found passing from the crus to the substantia nigra. The remaining degenerate fibres began to leave the left pyramidal tract at the junction of the pons and medulla, passing as single degenerate fibres to the facial nucleus of one or the other side. Below the level of the facial nuclei these fibres passed to the motor nuclei of the glossopharyngeus and vagus on both sides, the majority crossing the raphe to reach the nuclei on the opposite side. Occasional fibres were observed which apparently passed to some termination dorsal to these nuclei. This movement of degenerate fibres continued as far as the sensory decussation. A few degenerate fibres (probably thumb or finger fibres) remained in the pyramid and crossed in the decussation to the right lateral column, and disappeared in the lower cervical or upper dorsal region. In some of the facial lesions there were appearances of degeneration in the right internal capsule, but its connection with the lesion could not be demonstrated.

PARIS.

Academy of Sciences, August 19.—On matches tipped with explosive mixtures, by M. Th. Schlœsing. The author has experimented with a number of mixtures of substances with the view of finding a paste endowed with the properties of that mixture containing white phosphorus, and not having its poisonous character. The results show that it is necessary to use potassium chlorate, red phosphorus, ground glass, and glue or its equivalent, and that it is by no means a simple matter to find a perfect substitute for the paste used in tipping common matches.—On the storms and earthquakes in Austria during June, by M. Ch. V. Zenger. It is shown that during this period: (1) Solar activity has been very great. (2) Magnetic perturbations have been very ample and frequent. (3) Earthquakes and cyclonic storms of extraordinary violence have coincided with the appearance of numerous and brilliant meteorites, and with the passage of numerous shooting stars.—On equilateral hyperbolæ of any order, by M. Paul Serret.—On permanent deformations and the rupture of solid bodies, by M. Faurie.—On the conducting power of mixtures of metal filings and dielectrics, by M. G. T. Lhuillier.—Researches on the combinations of mercury cyanide with chlorides, by M. Raoul Varet. A thermochemical study on the combinations of mercury cyanide with the chlorides of sodium, ammonium, barium, strontium, calcium, magnesium, zinc, and cadmium. The solutions of these double salts do not give the isopurpurate reaction with a picrate at 30°, and hence the cyanogen remains wholly in combination with the mercury at this temperature. On boiling, however, there is evidence of interchange of a small proportion of cyanogen for chlorine.—Thermal researches on cyanuric acid, by M. Paul Lemoult. As in the case of phosphoric acid, the addition of each of three equivalents of alkali is marked by a different evolution of heat; the acid is a tribasic *mixed* acid.—Heat of combustion of some β-ketonic ethereal salts, by M. J. Guinchant.—Determination of heat disengaged in alcoholic fermentation, by M. A. Bouffard.—On the gum of wines, by MM. G. Nivière and A. Hubert.—On the migration of phosphate of lime in plants, by M. L. Vauhin.—Origin and rôle of the nucleus in the formation of spores and in the act of fecundation, among the Uredineæ, by M. Sappin-Trouffy.

BERLIN.

Physiological Society, July 5.—Prof. Munk, President, in the chair.—Prof. H. Munk spoke on contractures he had observed in monkeys after removal of the motor areas of the cerebral cortex.—Prof. Gad reported some experiments of Prof. Nicoladoni (of Athens), which had demonstrated the presence of fat granules in the pyloric gastric glands and in Brunner's glands.

July 10.—Prof. du Bois Reymond, President, in the chair. Dr. Schultz demonstrated micro- and macro-scopically the contraction of the unstriated muscle fibres of the stomach of Salamander. It was seen that the excised strips only contract when they are cut out in the direction of the long axis of the fibres, not when the fibres are cut through at right angles to their axis.—Dr. Rawitz had stained the lymphatic glands in the mesentery of *Macacus cynomolgus* by his "additive" method. He found the nuclei of the cells were generally placed excentrically, and contained a minute round chromatin patch. The linin network was marked by minute nuclei at the points of intersection and attachment. The structure of the plasma was quite indeterminate, but it appeared to contain a small round body, 2 to 3 μ in diameter, which stained somewhat deeply, and which he regarded as van Beneden's "attraction sphere."—Dr. Schultz had examined the optical properties of unstriated muscle-fibres of vertebrates in polarised light. It was found that although single fibres were not doubly refracting, a thicker layer of them was so quite distinctly. From this he concluded that the *single* fibres are in reality doubly refracting, but too feebly so to be perceptible. The double refraction became less during contraction, from which he concluded that, in accordance with von Ebner's theory, the anisotropic property of the fibres is due to differences in their internal tensions, the latter being greater in a transverse than in a longitudinal direction.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Erdmagnetische Messungen in Österreich: J. Liznar (Wien).—Durham College of Science, Calendar (Reid).—Die Schöpfung des Menschen und Seiner Ideale: Dr. W. Haacke (Williams and Norgate).—Elements of Coordinate Geometry: Prof. S. Loney (Macmillan).—A Laboratory Manual of Organic Chemistry: Prof. Lassar-Cohn, translated by Prof. A. Smith (Macmillan).—Astronomische Mittheilungen von der Königlichen Sternwarte zu Gottingen: Prof. W. Schur, Vierter Theil (Gottingen, Kaestner).—Symons's British Rainfall, 1894 (Stanford).—Forty-second Report of the Department of Science and Art (Eyre and Spottiswoode).

PAMPHLETS.—Geological Survey of Alabama; Report upon the Coosa Coal Field: A. M. Gibson (Montgomery).—Plants and Gardens of the Canary Islands: Dr. D. Morris (Spottiswoode).

SERIALS.—Botanische Jahrbucher fur Systematik, Pflanzengeschichte und Pflanzengeographie, Einundzwanzigster Band, 3 Heft (Williams and Norgate).—Journal of the Institute of Jamaica, April (Kingston).—L'Anthropologie, Tome 6, No. 4 (Paris).—Quarterly Journal of Microscopical Science, August (Churchill).—Journal of the Royal Horticultural Society, August (117 Victoria Street).—Longman's Magazine, August (Longmans).

CONTENTS.

THURSDAY, SEPTEMBER 5, 1895.

THE PENDULUM AND GEOLOGY.

Results of a Transcontinental Series of Gravity Measurements. By George Rockwell Putnam. *Notes on the Gravity Determinations Reported by Mr. G. R. Putnam.* By Grove Karl Gilbert. (Washington, U.S.A.: *Philosophical Society's Bulletin*, vol. xiii. pp. 31–76.)

SINCE the number of swings, which a pendulum of given length makes in a certain number of hours, depends upon the attraction of the earth at the place where it is swinging, it follows that, if an observer carries the same pendulum to different places, and notes the number of swings at each place he visits, he can by that means compare the force of gravity at the several places. If the earth were a smooth spheroid consisting of concentric shells, each of uniform density throughout, then gravity would have the same value at all stations situated on the same parallel of latitude. But if, as is the case in nature, there are mountains and elevated plateaus along the course followed by the observer, gravity ought to vary from its normal value, and in fact it is found to do so. Theoretically it is possible to calculate what variation of gravity at a given station ought to be caused by the altitude of the station, and the attraction of the neighbouring visible masses—*i.e.* of the mountain or plateau where the pendulum is swung, and of the rock masses round about, and when these disturbing causes are allowed for, and the corresponding corrections made, the value of gravity as deduced from the rate of the pendulum might be expected to tally with what it would be at the base level, supposing the mountains and all the surrounding masses carted clean away, and the smooth surface of the globe laid bare. This correction is termed reducing to the sea level, or to the mean level if the reference is made, not to the sea, but to some inland station. The question then to be answered for each station is, whether when this correction has been made, or, in technical language, when gravity has been reduced to the sea, or mean, level, does the reduction give the value which might be expected for the latitude? If it does not, this points to some deviation from regularity in the density of the earth's crust below the station, the nature of which may be inferred from the character and amount of residual discrepancy, when the reduction has been made. In this way it is that the pendulum becomes a kind of geological stethoscope.

In investigations of this kind, the elevated ground which forms the station is usually very much wider than it is high, so that, bearing in mind the law of the inverse square, it may be regarded as an extensive plain. If from local peculiarities it cannot be so regarded, compensatory allowances are made to bring it under that category. The effects of the station being situated on an elevated plateau are of three kinds, two of which cause gravity to appear smaller than it would appear at the sea level beneath the station, and one which causes it to appear greater. Of the two which make it appear smaller, the more important is, that the increased distance from the earth's centre causes the attraction of the earth as a whole to be diminished; the other, which is insignificant, and usually neglected, is that the increased distance from the axis of rotation increases the centrifugal force, which is opposed to gravity. The third effect, which causes gravity to appear greater than at the sea level, arises from the attraction of the matter of which the elevated plain, or mountain, is composed, for that may be regarded as an adventitious mass of rock, in excess of the sphere, placed beneath the pendulum. The reduction of the gravity observed at the station consists, therefore, in adding a correction equivalent to the diminution due to the elevation of the station, and subtracting a correction equivalent to the attraction of the mass of the elevated plain. If the reduction so made does not bring the observed value to agree with the value at the sea level, appropriate to the latitude of the station, there must be some geological cause present to account for the discrepancy.

It came to light in 1847, in consequence of the great trigonometrical survey of India, that, on approaching the range of the Himalayas within about sixty miles, the plumb-line, or vertical, was slightly deflected towards the mountains, so that it did not remain exactly perpendicular to the earth's surface. This was what might have been expected, because the great rocky mass would naturally draw the plumb-line towards it. But when the attraction of the mountains came to be calculated, it was discovered that, although their action was great enough to have caused a source of perplexity to the surveyors, it was nevertheless not so great as might have been expected. Clearly, then, some geological cause was latent, which required to be explained.

After some not very successful attempts at explanation by others, Airy, then Astronomer Royal, proposed in 1855 a solution of the difficulty which met the case. He assumed, as in those days was usually done, that the crust of the earth was comparatively thin, and rested upon a more or less liquid substratum, which in his paper in the *Philosophical Transactions* he called "lava." Then he showed that a great mountain mass would break the crust through unless it was supported by a protuberance beneath it, projecting downwards into a layer denser than itself. In short, it needed to be held up in hydrostatic equilibrium, much as an iceberg is supported in the ocean; and he explained how, under these circumstances, the observed deficiency of attraction of the plumb-line towards the mountains would be accounted for.

Although this observation upon the plumb-line was not a direct investigation of the force of gravity, it was nevertheless conducive to it, for the unexpected abnormality in the horizontal effect of mountain attraction rendered it probable that the same cause, whatever it might be, would produce some corresponding effect upon vertical attraction, *i.e.* upon gravity. It has been explained how the pendulum is the suitable apparatus for measuring gravity, and accordingly the pendulum was called into requisition to make more direct observations. At certain stations of the Indian Survey, of which the height and position had been already determined, the mean number of swings, called the "vibration number," was observed, which were made by the pendulum in twenty-four hours; and the force of gravity at the different stations was thus compared. The local attraction of the elevated mass on

which the pendulum stood, and the effect of elevation above the sea, were then allowed for, and the vibration number, when so corrected, was regarded as the vibration number for that station when reduced to the sea level. The pendulum used would have made 86,000 vibrations in twenty-four hours at the equator. It must therefore have been slightly longer than a seconds pendulum, which would make 86,400 in the same interval. The observations showed that there was a more or less marked deficiency of gravity over the whole continent of India, and that the deficiency was greatest at the most lofty stations. At Moré, 15,408 feet above the sea, the deficiency was enough to make the vibrations in twenty four hours twenty-four fewer than they ought to have been if the attraction of the mountain had produced its full effect. It was obvious, therefore, that some hidden cause existed which counteracted the attraction of the mountain, and this could have been no other than a deficiency of density in the matter beneath it. The conclusion is identical with that reached by Airy in connection with the deflection of the plumb-line, namely, that the Himalayan range is supported by a downward protuberance, projecting into a more dense substratum.

This mode of support, as already remarked, is similar to what is termed hydrostatic equilibrium. As applied to the support of the earth's crust American geologists have given to it the name "isostacy," which well describes the phenomenon.

During the past year an extensive series of gravity measurements has been carried out by the Coast and Geodetic Survey of the United States, by the use of the half-second's pendulum, a much smaller and more portable instrument for the determination of gravity than any hitherto employed. Observations were made at twenty-six stations, eighteen of which follow nearly along the 39th parallel of latitude; and these are particularly well adapted to throw light on important questions regarding the condition of the earth's crust.

> "This line of stations, commencing at the Atlantic coast, ascends to near the Appalachians, traverses the great central plain, gradually increasing in altitude from 495 to 6041 feet, then rises to the high elevation of the main chain of the Rocky Mountains, reaching an altitude of 14,085 feet at Pike's Peak, descends into the eroded valleys of the Grand and Green Rivers, crosses the summit of the Wasatch ridge, and finally descends to the great western plateau of the continent."

This series of gravity determinations affords an exceptionally favourable opportunity of helping to determine whether the support of the elevated regions traversed appears to be best accounted for by rigidity in the foundations on which they rest, so that, in spite of their weight and the largeness of the area occupied by them, they are prevented from sinking down into the material beneath; or, on the other hand, whether they are supported, as we have said that Airy suggested, namely by floating in a denser substratum, or, as the Americans say, by "isostacy," which is the same thing as hydrostatic equilibrium.

The general principle of the method pursued in reducing gravity to the sea level has been already explained. It consists in adding a correction equivalent to the diminution of gravity due to the elevation of the station, and subtracting a correction equivalent to the attraction of the mass of the elevated plain upon which the station may be considered to be situated. When these two corrections have been made, gravity so corrected would be the same as that appropriate to the latitude, or, as it may be termed, to the "computed value," unless there is some deviation from regularity in the density of the matter below sea level. The result proved that this was the case. For gravity so reduced turned out to be invariably less than that appropriate to the latitude. It was clear, therefore, that at these stations in America there was a deficiency in density beneath the elevated districts, just as had already been found to be the case in India. There could be no doubt that isostacy had a share in contributing to their support. The inquiry was now carried a step further. Did each mountain individually owe its support to a separate protuberance of its own beneath it, or was the mountainous region as a whole supported in that manner, each separate mountain owing its support to the strength of the crust on which it was a mere excrescence? The case might be illustrated by conceiving a number of logs of wood of different sizes. If these float side by side in water, the larger logs will stand the higher above the surface of the water; but each log will have a part immersed which will be its individual support, and this will be deeper for the logs which stand the higher. But if these logs are placed upon a raft, the support will be general, and derived from the support of the part immersed of the entire raft, and its depth will depend upon the aggregate weight of the logs. Nevertheless it need not dip deepest beneath the logs which stand the highest above the water, or above the floor of the raft.

The presumption was against each elevation being separately isostatically supported, because the deficiency in gravity, and therefore in density, was not found to be greatest precisely beneath the highest stations. To carry out the inquiry more fully, it was considered that, by omitting the part of the reduction to the sea level which takes account of the attraction of the mass of the plain (which would mean omitting to subtract the attraction produced by it), we should, as it were, transfer its mass to the subjacent parts, and so make up for the lack of density, and obtain the condition of uniform density below the sea level. There would then remain only the correction for elevation necessary. If this proceeding gave the value appropriate to the latitude under each station, it would show that the individual stations were seriatim in isostatic equilibrium. But the attempt failed. It was found that the attraction of the matter of the more elevated stations was not separately compensated by defect of density immediately below. The analogy of the detached floating logs did not hold good. It remained to inquire whether the series of stations was in isostatic equilibrium when considered as a whole—the case more nearly analogous to the raft. If this were so, gravity, when reduced to the sea level, would be uniform for the whole tract.

For this purpose a mode of reduction devised by M. Faye was adopted. The altitude of the country surrounding the station within a radius of 100 miles was reduced to a mean altitude, and the attraction of a plate of rock of thickness equal to the difference of altitude between this mean

plain and the station was allowed for, and it was found that this correction brought the gravity at each station much nearer to the computed value for the latitude than either of the previous methods. The conclusion was that, when large areas were considered, they were approximately in isostatic equilibrium. "The result of this series [of observations] would therefore seem to lead to the conclusion, that general continental elevations are compensated by a deficiency of density in the matter below sea level, but that local topographical irregularities, whether elevations or depressions, are not compensated for, but are maintained [supported] by the partial rigidity of the earth's crust." (Putnam.) "The measurements of gravity appear far more harmonious when the method of reduction postulates isostacy, than when it postulates high rigidity. Nearly all the local peculiarities of gravity admit of simple and rational explanation on the theory that the continent as a whole is approximately isostatic, and that the interior plain is almost perfectly isostatic." (Gilbert.)

It appears therefore that the crust of the earth is sufficiently thick and strong to carry such unequal loads as considerable mountains upon its surface without necessarily breaking through; but, when a large area is involved, it bends downwards into a denser material beneath, so that the crust and the load it carries are conjointly in approximate hydrostatic equilibrium.

O. FISHER.

SOME RECENT BOOKS ON MYCOLOGY.

British Fungus-Flora. A Classified Text-book of Mycology. By George Massee. Vol. iv. 8vo, pp. viii. 522. (London and New York: George Bell and Sons, 1895.)

Systematic Arrangement of Australian Fungi, together with Host-Index and List of Works on the Subject. By Dr. McAlpine, Government Vegetable Pathologist. 4to, pp. vi. 236. (Melbourne: Robt. S. Brain, Government Printer, 1895.)

Guides to Growers. No. 18. *Onion Disease.* By D. McAlpine. (Victoria: issued by the Department of Agriculture, 1895.)

MR. MASSEE is to be congratulated on the completion of another volume of his "British Fungus-Flora." There has been no complete work of the kind issued since the publication of M. C. Cooke's "Handbook of British Fungi" in 1871, and the knowledge of these obscure plants has advanced enormously since then. In the first three volumes the author treated the *Basidiomycetes* and the *Hyphomycetes*; the present volume takes up the large natural order of the *Ascomycetes*, and deals in turn with three families—the *Gymnoascaceæ*, the *Hysteriaceæ*, and the *Discomycetes*. The *Hysteriaceæ* form such a natural transition between the *Discomycetes* and the *Pyrenomycetes*, that it seems a pity Mr. Massee has not so arranged the families as to make them follow each other in the text-book; he has, however, very carefully pointed out the affinities of the different groups.

A general account of the *Ascomycetes*, their life-history, habitat, &c., is given in the introduction. The author agrees with Brefeld that sexual reproduction is unknown in this family. There is also some useful information about the best methods of collecting and preserving specimens, and of examining them. New descriptions have been written out for many of the plants, based in nearly every case on the author's own observations. Wherever it has been possible, he has examined the type specimens, or those specimens accepted as authentic in well-known *exsiccati*. It is impossible to over-estimate the value of such work. The descriptions are full and complete, and great care has been taken to give careful measurements.

The *Hysteriaceæ* have not before been worked up for Britain. Mr. Massee has not included *Acrospermum* in this family, nor in this volume. We await the next instalment of his work, to see where he will place it.

"British Discomycetes," by Mr. W. Phillips, has been for some time the standard work for that family. It was published in 1887, and there has been no reason for any material change in the way of treating the subject. The genera *Xylographa*, *Biatorella*, and *Abrothallus*, previously included among lichens, have been proved to be fungi, and are recorded, *Xylographa* in the family of the *Phacideæ*, *Biatorella* and *Abrothallus* in the *Patellariæ*.

The classification of the fungi is pretty well fixed as regards the natural orders, but no two systematists are agreed on the arrangement of genera and species. What characters are important enough to constitute a genus, is a question that each one answers in his own way. Phillips gave great importance to microscopic characters, but he was also largely guided by features visible to the naked eye or on slight magnification. He has comparatively few well-marked groups, and somewhat large genera with sub-genera. Saccardo laid much more stress on the differences between the species, and created new genera to represent deviations from the types, or revived old genera that had been sunk by systematists like Phillips. Mr. Massee goes even further; he retains nearly all the genera that had been kept up by Saccardo, and he has added in the *Discomycetes* eight genera revived from older authors, and five new genera, none of these being founded on new plants. Mr. Massee may be right in his views of classification, but the multiplication of genera and species, where that can be avoided, is much to be regretted. The matter has been admirably stated by Mr. Spruce in his "Hepaticæ of the Andes and Amazon," p. 73. "For a local flora," he writes, "or a limited area, too many genera will tend to produce confusion rather than precision, especially where several of the genera are monotypic; so that, on the whole, it seems desirable to make our genera as comprehensive as possible." There are several monotypic genera included in this volume, as for instance *Cubonia*, to which genus *Ascophanus Boudieri* has been transferred on account of its globose spores, those of *Ascophanus* being elliptical.

The task of classifying the *Pezizæ* is no light one; they are here divided into three large groups—*Glabratæ*, *Vestitæ*, and *Carnosæ*, under which the genera and species are arranged in a way that differs, in many instances, from that of every previous writer. The two first groups are familiar to us as the *Nudæ* and *Vestitæ* of Phillips. In the latter group the genus *Lachnella* has been dropped, and the species are dispersed and reclassified under *Erinella*, *Echinella*, *Diplocarpa*, *Dasyscypha*, &c. *Lachnella Cupressi* has been placed by itself in the genus *Pitya*, because the margin is free from the external hairs that are so marked a feature of this group, and because it

grows on conifers! In this group we also find *Geopyxis* Persoon emended Myc. Eur. i. p. 224 (not p. 42, according to both Saccardo and Mr. Massee): Persoon did not make *Geopyxis* a genus, although Saccardo also credits him with having done so; he published it as a division of *Peziza*, and Saccardo is the first who made it a genus, and therefore it ought to be *Geopyxis* Sacc. One of the species is the beautiful *Peziza coccinea* of old authors, transferred by Phillips to *Lachnea*, by Saccardo to *Sarcoscypha*, and now by Mr. Massee to *Geopyxis*. The division of the *Carnosæ* includes the genera *Peziza*, *Otidea*, *Humaria*, and others. A new genus, *Curreyella*, has been made to include *Peziza radula* and *P. trachycarpa*. Are we to assume that the Cuban species *Massea quisquiliarum* grows also in Britain?

In the family of the *Helvelleæ* there is much less alteration and rearrangement; but even there, two genera have been retained that were considered unnecessary by Phillips and Saccardo: *Cudonia* Fr., to contain *Leotia circinans*, which differs from others of the genus in the possession of filiform spores, and *Mitrophora* Lév., in which are placed two species of *Morchella*, *M. gigas* and *M. semilibera*. In these the lower half of the pileus is free from the stalk.

The numerous changes, however much we regret them, testify to the care with which Mr. Massee has treated the subject. He has omitted to mention one point of considerable morphological interest: that the abnormal many-spored condition of the ascus in *Tympanis* is due to budding of the original eight spores in the ascus.

The classified list of fungi, issued by Dr. McAlpine, has been compiled to assist vegetable pathologists in determining the diseases of plants due to these organisms. The knowledge of Australian fungi is as yet very incomplete, and we may expect the list to be largely augmented. M. C. Cooke's "Handbook of Australian Fungi" has served as a basis for the present work, and to it have been added the genera and species recorded by the more recent collectors and workers in this branch of botany. Australia possesses such a unique flora of the phanerogams, that we should have liked some indication of the fungi that belong exclusively to that country. The author has mainly followed the method of classification which has been adopted by Saccardo in his "Sylloge Fungorum." Dr. McAlpine retains the *Hyphomycetes* as a class by themselves, but describes them as imperfect *Ascomycetes*; this is hardly correct, for though many of them have been proved to be form-genera, others are unrelated so far as is yet known.

Besides giving us a list of fungi, Dr. McAlpine has drawn up some very instructive tables. The number of fungi recorded varies very much from colony to colony. Victoria heads the list with 1070 species, though we suspect the position of pre-eminence is due to the presence of Baron von Mueller, rather than to the abundance of fungi. Queensland records 1060 species, a large percentage of the whole due to the labours of an indefatigable worker, Mr. F. M. Bailey. Brisbane has 739 species, and New South Wales lags far behind with 406. There is much work evidently to be done before the localities are all worked out. The total number for Australia and Tasmania is 2294, as compared with 5040 recorded for Britain. The total number of species known to science is somewhere about 40,000. Dr. McAlpine has also prepared a host-index, which presents many points of interest. On *Casuarina*, that peculiar Australian tree, we find *Fomes igniarius*, a cosmopolitan species. *Eucalyptus* seems specially afflicted—leaves, bark, branches and trunk have all their separate fungal parasites. The *Compositæ* are hosts to but two, an *Æcidium* and a *Synchytrium*, evidently an incomplete account.

The *Agaricineæ* and *Polyporeæ* have received a much larger share of attention than the more minute forms of the *Discomycetes* and the *Pyrenomycetes*; Australian collectors give an account of but five *Nectrias* and two *Valsas*, but these forms are very easily overlooked. The *Phycomycetes* are also very sparingly represented; there are two *Peronosporas*, one on tobacco-leaves, the other on the onion. There is no record of potato disease, nor of salmon disease; we can only congratulate the colony on its immunity.

In addition to the authority and date for each fungus, Dr. McAlpine gives the locality in Australia, the habitat and a description in English of the species, but in no case does he indicate the characters of the genus; the list thus strikes the reader as being very imperfect, and the absence of all information as to the size of the particular plants renders it less useful than it might otherwise have been. We think he has vainly spent his strength in his attempt to provide an English equivalent for the scientific name of each fungus. Popular specific names have not been given even to flowering plants, such as the different kinds of *Myosotis* or *Crepis*, and such names are equally valueless in the case of fungi.

Dr. McAlpine has recently published, in "Guides to Growers," a most useful and practical account of the disease of onions caused by eelworms, with the best methods of cure. The worms live in the soil, and various dressings are recommended, suitable rotation of crops, or burning the surface of the land. This particular eelworm attacks the stems of plants, and in the case of the onion destroys the bulbs, leaving the roots unharmed.

A. L. S.

OUR BOOK SHELF.

The Climates of the Geological Past, and their Relation to the Evolution of the Sun. By Eug. Dubois. (London: Swan Sonnenschein and Co., 1895.)

THE first part of this essay consists of a brief and judicious summary of the geological evidence as to great changes of climate in past ages, while the second part is an attempt to explain the causes of such variations. Various well-known theories have been advanced to account for the phenomena, but none have met with general acceptance; a few years ago Dr. Neumayr wrote: "Most plausible and simple would it certainly be were the sun a variable star that at different periods emits different quantities of heat; but for this no proof is forthcoming." (NATURE, vol. xlii. p. 180.) The author of the present work seems to have adopted Dr. Neumayr's suggestion, but goes further and attempts to show that the postulated changes of solar radiation have actually taken place. In a general way, the fact that the sun must once have been hotter, has been frequently stated as a possible cause of the higher temperatures during early geological times, but a gradual cooling of the sun is insufficient to explain all the vicissitudes of geological climates. Basing his estimate on the relative proportions of stars of different spectroscopic types, the author considers that the sun has

passed about three-fifths of its star life, and that we cannot be far wrong in assuming for the past a maximum duration of about ten million years, and a radiation in the white-star stage twice as intense as at present. As a step towards the reconciliation of the life assigned to the sun by physicists and that demanded by geologists, it is suggested that in consequence of the higher temperature when the sun was a white star, denudation was carried on more vigorously, and animal and vegetable life developed more rapidly than has been supposed.

Notwithstanding that the author has approached the subject with an enlightened mind, he does not appear to have greatly advanced the explanation. For the production of changes other than those due to the progressive cooling of the sun, it is necessary to suppose that the sun is subject to periodical changes, and the chief argument brought forward in favour of this supposition is that the acknowledged eleven-yearly period of the sun renders it probable that there may also be periods of longer duration.

It is clear that such long-period changes are quite outside our range of observation, and the indirect evidence brought forward is unconvincing. We do know, however, that the variation which has been observed in stars resembling the sun is very rare and always slight.

Methodisches Lehrbuch der Elementar-Mathematik. Von Dr. Gustav Holzmüller. (Leipzig: Teubner, 1894–5.)

THIS is a text-book of elementary mathematics, showing the extent of knowledge required of the German schoolboy; and apart from the interesting presentation of the subjects in a manner far superior to anything we can show, the book is well worthy of translation as illustrating the difference in the standards of requirement of German and English schools; the knowledge exacted of the German schoolboy being about the equivalent of our B.A. requirements.

But then the German schoolmaster, although working to a much higher standard, can take his responsibilities lightly; he has merely to point out to his pupils that it depends entirely upon themselves whether they are to spend three years or only one under the civilising influence of the drill-sergeant.

The harder his pupils work, to escape with one year of military service, the higher the standard which the government inspector can exact for exemption; thus the paradoxical result is attained that the system of conscription tends ultimately to elevate the intellectual standard of school knowledge; but, on the other hand, the physical development of youth runs great risk of being stunted. Obviously there is no place in a German school, or French school either now, for the cricket, rowing, and football, which we now consider of equal importance with abstract studies. All Europe is now an armed camp, this country excepted; and the observant philosopher is doubtless beginning to draw inferences as to the comparative effect of the systems on the development of the human race.

Dr. Holzmuller's "Einführung in die Theorie der isozonalen Verwandschaften und der Conformen Abbildungen," 1882, is a well-known standard work, profusely illustrated with carefully-drawn diagrams, which emphasise many delicate points in the Theory of Functions in a manner much more convincing than arguments depending on a procession of analytical formulas; so also in this "Methodisches Lehrbuch," a plentiful supply of figures serves as a substitute for long algebraical calculations.

The author has made these elementary parts of mathematics more interesting and pleasant reading by historical notes and simple applications; and altogether the work is a great contrast to the dry bones we are accustomed to here; it would be well for our writers of school books to study the sentiment expressed in Dr. Holzmüller's preface: "Uns von der allzustarren Gebundenheit der Lehrpläne zu befreien." G.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Heights of August Meteors.

IN addition to the four or five meteors recorded last week in NATURE (vol. lii. p. 395–6), by Mr. Denning, as having been simultaneously observed at more than one station during this year's August-meteor period, particulars which have just now reached me of some observations of the Perseids made at Tring, in Herts, on the night of the 11th inst., show that two of the meteors seen and mapped here between 9.45 and 12 P.M. on that night, also had their apparent paths mapped simultaneously at a place at Tring, about nineteen miles due north from this point of observation. The base-line between the two stations is a rather short one for such comparative determinations, but as the recorded flights passed nearly overhead, and nearly from east to west across the line between the stations, the conditions for accuracy were very favourable in both the meteors' cases, and the apparent paths seem also, by the comparison, to have been mapped with much precision. They require, however, as usual, corrections of a few degrees at the beginning and end points to make them quite geometrically compatible.

Tring.—August 11, 9.53 P.M.; 1st magn.; left a long streak along a long path; 33°, from $332\frac{1}{2} + 39$ to $287\frac{1}{2} + 42\frac{1}{2}$; (corrected path, 35°, from 328 + 40 to 280 + 41). Duration, 2 or 3 seconds.

Slough.—August 11, 9.53 P.M.; 1st magn., white; 37° in 1·2 second, from 331 + 53 to 268 + 51; (corrected path, 35°, from $336 + 50\frac{1}{2}$ to 277 + 54). Left a bright white streak on its whole course for 3 seconds.

The observed tracks are 15° to 13° apart, nearly parallel, but slightly converging; and if made parallel, about 14° apart throughout, they indicate a radiant-point at the east-horizon (11° N. from E.), at 21 + 7, near μ and o Piscium, from very near which radiant-point the meteor was, no doubt, directed, as its long streak-leaving flight plainly enough denoted a very nearly horizontal motion. The resulting real path is from 77 miles over a point 4 miles north of Farringdon, in Oxfordshire, to 77 miles over a spot 3 miles E.N.E. from Uxbridge, in Middlesex. This course of 50 miles, with a duration of 1·2 seconds, gives the speed of flight $41\frac{2}{3}$ miles per second, the speed for meteors with parabolic motion from the same radiant-point (omitting a small addition for the earth's attraction) being $40\frac{3}{4}$ miles per second. In Mr. Greg's "General Comparative Table of Radiant Positions," as No. 106 of the list ("British Association Report," 1874, p. 333), a place at 22 + 5 is given as the average radiant-centre of a group of several meteor showers observed by Schmidt, in Athens (p. 321–2 of the same "Report"), in July, August, and September. It was thus from a very central direction of a rather notable autumnal group of meteor-showers in the neighbourhood of α Piscium, that this bright streak-leaving meteor seems to have proceeded. The corrections above applied to the recorded paths, although apparently considerable, are really only slight shiftings of the flights lengthwise; their original lines of direction, and hence their resulting radiant-point being left, as nearly equally as possible in both the paths, almost unaltered.

Tring.—11.3 P.M.; shot 12°, leaving a streak, from 345 + 58 to 325 + 53 (corrected path, 13°, from 343 + 58 to 322 + 52).

Slough.—11.4 P.M.; 3rd magn.; shot 10°, without streak, from 350 + 72 to 312 + 70 (corrected path, 10°, from 352 + 72 to $317 + 70\frac{1}{2}$).

The path corrections here are only small shortenings or extensions of the apparent tracks to bring their lengths into agreement (at distances apart then of 13° to 18°, in the right directions) without disturbing the path-directions, which diverge from 45 + 53, a point nearly coinciding with the usual radiant-point of the Perseids on August 10–11, at 44 + 56.

The concluded real path is from 67 miles over a point 5 miles west of Leighton, in Bedfordshire, to 53 miles over a point 3 miles west by north from Tring. The length and downward slope of the real path was $19\frac{1}{2}$ miles, from 45° altitude, 34° north from east. The time of flight of this small Perseid was not noted at either place of observation, but as it probably agreed with that of several similar short Perseids noted nearly overhead on the

same night, which varied with the lengths of path from about 0·5 to 0·7 sec., the velocity deduced would be between 39 and 27 miles per second, fairly agreeing with the real meteor-speed of the Perseids, which is 38 miles per second.

It would be interesting to learn if any observations were made elsewhere of a meteor seen at Tring at 9.32 P.M. on the 19th inst., descending from near the zenith at 317½ + 37, southwards to 320 + 15, which exceeded the fixed stars in brightness, and which was brilliant enough to attract the attentions of ordinary wayfarers there; so that with this observation of its path at Tring, its real course, and the position of its radiant-point in the northern sky might be determined. A. S. HERSCHEL.

Observatory House, Slough, Bucks, August 29.

Do the Components of the Compound Colours in Nature follow a Law of Multiple Proportion?

ON examining the data contained in Mr. Pillsbury's interesting and valuable *résumé* on colour measurements in the United States, by means of ordinates and abscissæ for the various colours on squared paper, it became at once evident from the parallelism of the diagonals which could be drawn that, although previously hidden, there was a numerical relation underlying them, and that probably the measurements would furnish an affirmative answer to the question printed at the head of this letter. Can it possibly be that those compound colours which occur with such profusion in nature are the result of simple colours being combined in definite multiple proportions? Can there be a law of multiple proportions here, similar to that which holds good in the domain of chemistry?

Let us see how far the data which Mr. Pillsbury gives support such a conclusion: they cannot from their paucity prove it. If we take all the foliage greens given, raise the percentage of black to 100 in each case, and proportionately increase or decrease the yellows and greens, then the amount of yellow in each case divided by the amount of green in each case will give a ratio which, the black being equal, may be said to represent in figures the colour of the particular foliage. Now what do we find on examining the resulting ratios? They are all divisible into groups of multiples of 2, which may be represented as in the last column of the table by 1-Y, 2-Y, 3-Y. It will be noticed that while the figure in the second decimal place is not exactly a multiply of 2, yet it tends very much in that direction.

	Black.	Yellow.	Green.	Yellow : Green.	Yellow.
Hemlock-Spruce	100	2·25	10·1	·22	1-Y
White Pine ..	,,	2·9	12·8	·22	
Apple ...	,,	6·25	3·75	·48	
Hornbeam	,,	6·8	15·3	·45	2-Y
Hickory	,,	5·3	11·1	·47	
White Birch	,,	6·8	14·1	·48	
White Oak	,,	9·3	14·3	·65	3-Y

Of course the conclusion reached cannot by any means be considered proved, as we do not know if the foliage greens were selected by Mr. Pillsbury purposely, or were merely the result of pure chance; but it would seem amply to repay further investigation, and I should be pleased to hear that Mr. Pillsbury could undertake it, or, if he feels unable, but would furnish me with the necessary material, I would try and undertake it myself.

As much stress is laid upon the commercial utility of this system of colour measurement, might I suggest that in all cases the simple colour of which there was the largest quantity should be taken as measuring 100? By this means there would always be one less number to recollect, write, or telegraph, than there are simple colours in the compound—no small factor when dealing with large quantities. F. HOWARD COLLINS.

[illegible], Edgbaston.

Transformation of Moulds into Yeasts.

Experiments carried out in Dr. Jorgensen's laboratory in Copenhagen, on the morphological relations of moulds and yeasts, are of great interest, and have an important bearing upon the study of the Japanese method of saké brewing, an investigation of which was made by the writer whilst in Japan, and the results of which were published by the National University of Japan in 1881.

In this process a mould is allowed to grow over the surface of steamed rice until the grains are firmly matted together by the fibres of the mycelium, and this product (*koji*), mixed with fresh steamed-rice and water, is placed in mash-tuns and slightly warmed. After a short time active fermentation sets in, resulting in the preparation of a liquid (*saké*) containing as much as 15 per cent. of alcohol. The question as to the origin of the ferment-cells was discussed in the memoir above referred to, and the conclusion was arrived at that there was no evidence to show that the mycelium underwent any change, but that the ferment-cells were derived either from the air, or from the vats, or from spores which had attached themselves to the surface of the mycelium. Microscopic drawings were given illustrating the appearance of the mash at various periods during the fermentation, and in these the mycelium was seen to preserve its original form to the end of the process. The feature upon which most stress was laid by those who asserted that the mould was converted into the ferment, was the suddenness with which the fermentation showed itself, of which fact there could be no doubt; but it seemed to the writer that there was a very simple explanation of this. The fermentation appears immediately after the warming of the mash, which has already been exposed to the air in shallow vessels at a low temperature for several days before being collected into a single vat. It is also allowed to stand in this vat for several hours before heating, during which time probably all the dissolved oxygen has been used up by the ferment. By heating, the temperature is then raised to about 25 C., a condition very favourable to the growth of yeast. Knowing how rapidly the yeast plant buds under such conditions, it does not seem necessary to invoke the transformation of the mycelium into ferment-cells to account for the sudden appearance of the fermentation.

The note (NATURE, August 22, p. 397) further says that Juhler found in his flasks in which the Japanese mould, *Aspergillus oryzæ* (called *Eurotium oryzæ* in the writer's memoir), was cultivated a growth of typical alcohol-producing saccharomyces cells. If there were spores attached to the surface of the mycelium, it seems necessary to know in what manner they were destroyed before the introduction of the mould into the culture flasks. It would also be interesting to have more details of the size of these cells, to ascertain if they correspond exactly with those found in the native Japanese fermenting vats. The size of the full-grown cells measured by the writer were on the average 0·0082 m.m. in their longest diameter—that is, between the dimensions of ordinary beer-yeast and wine-yeast.

Cardiff, August 24. R. W. ATKINSON.

IN reply to Mr. Atkinson's inquiries, we would refer him to Juhler's original communication on his experiments with *Aspergillus oryzæ*, to be found in part ii. of the *Centralblatt für Bakteriologie*, Nos. 1 and 9, 1895.

August 29. THE WRITER OF THE NOTE.

Mr. Seebohm on Middendorff's Credibility.

MR. SEEBOHM writes (*antea* p. 385): "There is no reason to believe that Middendorff ever found the eggs of the little stint. The eggs which he records as being those of *Tringa minuta* were probably those of *Tringa ruficollis* and possibly those of *Tringa subminuta*." To me these statements seem made in oblivion of the facts, and as some years ago I exhibited in London (*Proc. Zool. Soc.*, 1861, p. 398) one of the specimens on which doubt is thus thrown, I beg leave to show that there is no reason for believing that distinguished explorer to have been mistaken. The only eggs he records (*Sib. Reise*, II. ii. p. 221) are four, the parent of which he caught under his game bag. No other nest is mentioned, and this one was found on July 1, 1843, in the Taimyr peninsula, which is admittedly as well within the range of *T. minuta*, as it is outside of that recorded for *T. ruficollis* (*cf.* Palmén, *Vega-Exped. Vetenskapl. Jakttagelser*, v. tab. 4). Though not recognising these two birds as good species, Von Middendorff had carefully noticed (*tom. cit.* p. 222) the difference between examples obtained in the far East (Ochotsk) and in the high North (Taimyr), expressly stating that the latter agreed with Naumann's figure which undoubtedly represents *T. minuta* in summer plumage. As to *T. subminuta*, I am not aware of any evidence of its occurrence in the Taimyr, and by conjecture only can it be ascribed to that district; but the supposition that a single nest can have belonged to both *T. ruficollis* "and possibly" *T. subminuta*, is a masterpiece of conjecture wholly above my power of comprehension.

ALFRED NEWTON.

Magdalene College, Cambridge, August 23.

ON PHOTOGRAPHS OF THE MOON TAKEN AT THE PARIS OBSERVATORY.

QUITE recently some negatives of photographs of the Moon, taken at the Paris Observatory by MM. Lœwy and P. Puiseux, were exhibited at the Academy of Sciences.

The negatives have been carefully studied, enlargements made, and specimens sent to all the principal scientific societies interested in them. These enlarged copies are of great help in the study of the Moon, and have been the means of making clearer many uncertain points, for they allow every detail to be seen without difficulty. Their chief advantage, however, lies in the great expanse of surface which they embrace; many facts, hard to discover on the smaller negatives, have now been ascertained.

In their communication made to the Academy, MM. Lœwy and Puiseux gave an account of the results they have obtained in studying these photographs. Some of them are of great interest.

Considering, first, the Moon's surface, they note that its markings are of a less varied type than those of the earth, and its prominences are chiefly of a circular shape. By the way in which the Moon reflects, it is thought that its crust is of solid matter, similar to volcanic rocks. This agrees perfectly with Laplace's hypothesis, in which he states that the Moon was thrown off from the earth when the latter was in a nebulous state. The Moon's mean density scarcely surpasses that of the crust of the earth; its materials, judging exclusively from the exterior crust, are of a more uniform chemical composition. But although we might trace its history from the time in which it was thrown off from the earth, it is clear that all the facts rest on a very uncertain basis; it is scarcely probable that the Moon had the same appearance then it has now; it is only when the masses had become to a certain extent solid, that the surface-markings could have been formed which are now to be seen. A very long period must have elapsed between the nebulous state of the Moon and its present fixed condition, the process commencing, no doubt, by the union of the particles of scoriæ. Owing, however, to currents arising from various sources, ruptures must often have taken place, causing lines to be left on the parts which were not quite solid.

The various lines, which can be followed on the photographs, may be quite easily described. They are valleys between huge mountains. One of the largest is the valley of the Alps, to the west of Plato; another one between Herschel and Hipparchus, between Bode and Ukert; and one to the south-west of Rheita. It would be absurd to imagine them anything like the terrestrial valleys; they are almost perfectly straight, do not branch off at all, and keep the same width almost the whole length. There is no sign of what has become of the materials out of them, and when minutely examined, they appear to have flat bottoms; this fact seems to prove that they were once filled with some liquid which has dried up. As before stated, their origin is most probably due to currents, which must necessarily have developed in the mass of the moon when still fluid. These valleys are grouped about in various parts, and run parallel chiefly, especially near the equator, but they also go in other directions. There is nothing to show that the direction has remained the same.

So long as the revolution and rotation of the Moon were not performed in the same time, the tides must have produced very considerable change of level, which would hinder the crust from becoming solid. The scoria, therefore, would gradually form itself into larger and larger islands, which, however, might often have got broken up owing to constant collisions. Still gradually gaining in thickness, they eventually constituted the oldest part of the Moon, and at their expense the circular formations were formed which we now see. After a time banks of scoriæ of great length covered the Moon, leaving only narrow passages for circulation. Continual collisions destroyed the projecting parts, which facilitated the ultimate joining of the islands.

The fluid masses of a body like the Moon take part in the general circulation, but naturally have their tides under the influence of gravity. The combination of these two movements produces irregular rates in the floating masses, which more or less always impede their displacement. This irregular rate causes renewed collisions and rectilinear formations differing in direction from the first. After such various forces had been brought into play, it is not astonishing that the marks left are not absolutely regular and symmetrical. The parallel lines indicate the existence of similarly directed currents at the time the superficial solidification was going on. The lines running in different directions, indicate changes in the direction of those currents.

Let us now consider the result of a huge boulder of crust getting detached and falling. If falling on a slope, it would naturally slip down, and in the matter, not yet solid, form, as it were, a path; thus ultimately a valley would be made. This explanation applies itself more especially to the valley of the Alps, because of its very precise shape. If, therefore, these valleys are imperfect joinings of ancient ruptures, they must form, on the hard crust, lines of less resistance. The lines of craters are now easily explained, also the various holes in the furrows, which may be looked upon as explosion outlets.

If, on the other hand, they date from superficial solidification, their presence must have influenced the subsequent formations. Admitting that, under a part of the crust already thick, a diminution of pressure is produced, capable of producing a cavity, these changes might be brought about by the gradual cooling of the Moon, or by the movements of the interior tides. The cavities might take almost a circular form if the crust were homogeneous, having for the centre the point where the pressure was at a minimum. But if there are other ruptures and lines, they would probably form the boundary to the cavity. We notice that the polygon form is most frequent after the circular; in many cases, also, the furrows form tangents to the circles.

MM. Lœwy and Puiseux remark, finally, that it is not for them to say which of the hypotheses is correct; they merely wish to call attention to the immense help the enlarged negatives may prove themselves to be. Eventually, no doubt, they will be the means of making a map, which may show us that the surface structure of the Moon is very similar to our own.

We imagine that not every one will agree with all the opinions above expressed by MM. Lœwy and Puiseux, but it is clear that several important questions have been raised by the magnificent photographs we owe to their skill and industry.

UNSCIENTIFIC EXCAVATIONS IN EGYPT.

PROF. DR. G. SCHWEINFURTH has recently written a most interesting letter to the editor of *Die Zeitschrift für Aegyptische Sprache*. According to him, the time has arrived when a limit ought to be put to the energy of Egyptian excavators. Within the last few years there has been such a tremendous collecting of antiquities, that it has seemed to be the desire to leave nothing whatever for the next generation to discover. Our near descendants will, in all probability, not thank us for our want of patience; it may have totally different methods of research, and may bring opinions forward we have not dreamt of. If this be the case we shall, most probably, be blamed for having dis-

turbed so much, and shall be accused of "vandalism" under the mask of advancing scientific research.

There is no doubt that the excavations have been carried on too fast. The great museum in Egypt has no proper catalogue, and is arranged and filled up with things in a most unsatisfactory way; many objects have not even got the date when found. In this way, what would be treasures have become absolutely valueless on account of the carelessness of former officials, who constantly depended on each other, and, in many cases, on their memory, for the facts connected with objects found. This will always be the state of things unless the excavations are supervised by museums; for the haste with which they are carried on, does not allow time to work out the history properly. The things are merely brought under cover; they accumulate, and only short notices are written about them. It is for this reason that many noted things found have not been heard of till years afterwards; likewise, before the old treasures were properly examined, others have been dug out, particular attention being given to pretty things with which to ornament museums. Consequently, while search was being made for inscriptions, smaller objects were neglected, and many details overlooked.

Whereas formerly complaints were ever being made about the difficulty of obtaining permission to excavate, now the state of things is just the opposite. There is too much liberality; men are allowed to excavate, who have no knowledge whatever as to how to set about it, and have no serious object in view. Valuable things have been removed from the Fayûm, Heliopolis, and other places by quite uneducated people, and sold as market goods in Cairo. All this sort of thing makes the advancement of science a farce.

A natural consequence of this hasty digging, but a state of things greatly to be lamented, is the destruction of the ancient topography. The confusion caused is beyond description. It is very desirable that there should be an international inspection committee, which would insist on things being cleared up, and not allow the graves and tombs to remain open, with bones and limbs of the dead in them, which is so often the case.

Another deplorable fact is the absolute ignoring of objects connected with natural history. These objects require special care when being dug out, and also are more difficult to find. Their destruction greatly endangers the science of antiquity, and many an object, the value of which is now unknown, may in some future period be the key to some great problem. Likewise the bones of domestic animals are overlooked, although the many pictures of these animals help to make a study of them very interesting, and the remains of plants and flowers are similarly neglected, though these objects are the stepping-stones to the restoration of the ancient history.

JOSEPH THOMSON.

BY the death of Mr. Joseph Thomson, we have to mourn one of the foremost of contemporary African explorers. His loss is all the more sad, as it comes in what ought to have been the prime of his manhood. When we remember what Thomson has done, what a part he played in the exploration of British East Africa and in securing for England her supremacy on the Niger, it is difficult to realise that he has done it all before the age of thirty-eight. Joseph Thomson was born in Dumfriesshire, on February 14, 1858, and was the son of a quarry-owner. He was educated at Edinburgh, and early took a keen interest in African exploration, in which he first personally joined as assistant in Keith Johnston's expedition to the African Lakes. He left Dar-es-Salaam early in 1879, but before it reached its first objective point—Nyasa—its leader died. Thomson was then little over twenty-one years of age; but he rose to the occasion, took command, and single-handed carried the expedition to a triumphant conclusion. He explored the plateau between Nyasa and Tanganyika, and the western shore of the latter from its southern end to the Lukuga; there he added another to the pile of contradictory statements as to the relations of this river and the lake. He tried to work westward to the Upper Congo, but, owing to the hostility of the natives, he was compelled to return to Ujiji and back to the coast. This was Thomson's first expedition, and in some ways it was his best, for his scientific observations were then made with greater care and detail than in any of his later journeys. The following year he returned to East Africa to search for the coal reported on the Rovuma. Next year he was sent to Africa on the famous expedition, the story of which he so brilliantly told in "Through Masai-land." He left Mombasa in 1882 with a powerful caravan, fitted out by the Geographical Society, in order to determine whether there be a practical route across the Masai country to the Nyanza, to explore Mount Kenya, and to study the meteorology, ethnology, and natural history of the region traversed. After great difficulties with his men, he marched inland to Taveta, at the foot of Kilima Njaro. There he joined a powerful caravan under the famous slave-trader, Jumbo Kinameta, and together they traversed Masai-land to Lake Naivasha, going first along the route of Last, and then along that of Fischer. Thomson then turned to the east, and was the first European to set foot on the plateau of Laikipia and to see Kenya from the west. But the Masai were present in force, and Thomson had either to fight or retreat. He chose the latter alternative, and, contenting himself with a distant view of Kenya, under cover of night fled northward to Baringo. He explored this district, which he was the first European to reach, and then went on to the Nyanza, and back to the coast. His next expedition was up the Niger. His tact and patience in dealing with natives, here stood him in good stead, and rendered this expedition his most successful, for he returned with the treaties which gained for England practical supremacy in the Niger Basin. In 1888, with Mr. Crichton-Browne, he undertook a journey to south-western Morocco, materially correcting some previous descriptions of the structure of that country. He took a series of altitudes, and with characteristic acumen discovered for himself the divergences between the results given by aneroids and boiling-point thermometers; but it was equally characteristic that he did not follow up the subject, and contented himself with attributing it to the imperfection of his instruments. In 1891 he was sent by the British South Africa Company to annex the metalliferous region of Katanga. He was greatly hindered by the Portuguese, who fired upon his flotilla, and when he reached the frontier of Katanga he found that Captain Stairs's expedition had arrived before him, and secured the country for the Belgians. Thomson returned to England with his health ruined by his six African expeditions. Residence at Kimberley saved him for a while, but phthisis had taken too firm a hold to be dislodged, and after a lingering illness he passed away on August 2.

It is too early to attempt to estimate fairly Thomson's work as an explorer; but no one could follow in his footsteps without recognising how singularly keen was his topographic insight, how rapid his powers of observation, and how marvellously true were his instincts. His powers, in fact, amounted almost to genius. In his quickness of perception and his literary skill he reminds us of Burton, though without Burton's scholarship and colossal capacity for steady work. But Thomson's brilliant gifts had their dangers, and it is impossible to compare his work with that of some of his contemporaries, or even of some of his predecessors, without recognising that he was

sometimes as careless as he was capable, and that he rarely used his great abilities to the full. He belonged to the school of explorers who prefer rapid traverses and pioneer work, to scientific investigations and detailed surveys. He reminds us by his geographical work of Livingstone, and by his love of sport of Selous, rather than of men like Fischer, Schweinfurth and Junker. He was fonder of the field than of the library, and often did not, apparently, know which of his results were new, and which were known before. The thing of which he was proudest was that he had never taken the life of a native, for he had avoided hostilities owing to his tact and infinite patience, which was especially creditable to a man of such an impulsive temperament. His love of peace, however, was not due to any fear of war, for he was brave to recklessness. That he felt warmly, and could speak impatiently, was shown by his criticisms upon the management of the Emin Relief Expedition. In his most famous expedition it was unfortunate that he followed such a trained naturalist and learned ethnographer as Fischer, and was himself followed by such a laborious and skilled cartographer as von Höhnel. On the other hand, this journey was the one which showed Thomson's powers at their best; for he then had the fullest scope for the exercise of his tact as a leader of men, his dash and daring as an explorer, his enthusiasm as a sportsman, and the consummate skill with which he gained the affections of his men and the confidence of suspicious natives. The same qualities won him respect at home. He is described, by those who knew him, as singularly modest and unassuming. His frank sincerity and genial humour endeared him to a wide circle of friends, who devotedly cared for him in his long illness, and now mourn his early death.

J. W. GREGORY.

WILLIAM CRAWFORD WILLIAMSON.

WHEN the author of this article began the work for his "Einleitung in die Palæophytologie," he soon realised that it was quite impossible to produce such a book without an accurate knowledge of Williamson's collection of sections. He therefore wrote to Manchester and requested permission to make use of the collection. An invitation to Williamson's hospitable house was the immediate result. He there spent eight delightful and busy days, during which the host was never weary of demonstrating his specimens to his guest, who was astonished at their abundance, or of imparting to him the fullest information from his store of knowledge. The guest departed with feelings of the warmest respect and gratitude. In the course of the following years, however, he has often again had the privilege of returning to Manchester and London, and of knitting closer the bonds of reverence and friendship with him who is gone. The last occasion was in the spring of the current year, when the writer left with the conviction that it had been their last meeting. Williamson's death actually took place at Clapham Common, on June 23, when in his seventy-ninth year.

William Crawford Williamson was born at Scarborough, on November 24, 1816. His father, John Williamson, a gardener by profession, but by the bent of his mind a naturalist, and especially a geologist, was a zealous colleague of William Smith, who was attached to him both by friendship and by their common pursuits, and who spent two whole years, 1826–1828, under his roof.

Young Williamson's father encouraged his scientific tastes, even from his earliest days; his observational faculties were strengthened by frequent excursions; the association with Smith, and with the circle of active geologists of that fruitful period, influenced his boyhood, and left behind an effect which lasted his whole life. He has often told the writer about his geological and botanical rambles with his father and friends along the beautiful cliffs of the Scarborough and Whitby coasts. He had an extraordinary love for his more immediate home, and was proud to call himself a Yorkshireman.

Williamson's first publications, "On a Rare Species of *Mytilus*," and "On the Distribution of Organic Remains in the Lias Series of Yorkshire," appeared when he was only in his eighteenth year. About the same time he also contributed a considerable number of drawings to Lindley and Hutton's "Fossil Flora of Great Britain," a work which was completed in 1837, when he was twenty-one. In his later years he did not continue to work much at remains preserved as impressions, for his whole interest had become diverted to anatomical studies. One or two papers on *Zamia gigas* (now called *Williamsonia*), however, owe their origin to the material accumulated in those youthful days. The last and most important of these papers appeared in 1870, in the *Transactions* of the Linnean Society, vol. xxvi., under the title "Contributions towards the History of *Zamia gigas*."

Williamson's family was not much blest with this world's goods. He was therefore obliged to adopt some practical calling, and naturally chose the medical profession, for which he prepared, first at Manchester, while at the same time acting as Curator of the Natural History Museum there, and subsequently in London. In 1840 he became member and licentiate of the Royal College of Surgeons. Soon afterwards he settled in Manchester as a medical man, and remained there over fifty years, carrying on for a long time an extensive practice. In addition to this the professorship of Geology and Natural History at the Owens College was conferred on him in 1851, an office which he administered, in its full extent, for many years. In 1872, however, he handed over the geology to Boyd Dawkins, and from 1880 onwards gave up the zoology, and confined himself to botany. This he continued to teach down to 1892, when his decreasing bodily strength compelled him to retire altogether. He then removed to London, in order that with the aid of the greater facilities there offered he might the better advance the scientific work, which he was still zealously pursuing. Here, after three more years, he too soon ended a life of which one may certainly say, with the Psalmist, that its strength was labour and toil.

For medical practice and professorial duties, though strenuously and most conscientiously carried on, did not satisfy Williamson's mighty power of work. Concurrently with these occupations, a constant flow of scientific production went on, the many-sidedness of which is scarcely conceivable to the present generation. Not only did he write articles in medical journals, which lie beyond the scope of the present notice, he also continued to work with the greatest zeal at zoology, botany, and, above all, geology and palæontology, as is testified by his numerous publications large and small.

From his youth upwards, Williamson had been much occupied with the investigation of fossil fishes, and in the latter half of the thirties, and beginning of the forties, he wrote various memoirs on this subject. His studies of lower organisms gave rise to the works on *Campylodiscus*,[1] on *Volvox Globator*,[2] and on *Foraminifera*, the last and most important of which, embracing the whole of his researches on the subject, was published by the Ray Society in 1858, under the title of "The British Foraminifera." These writings have received due acknowledgment in the works of Carpenter and Bütschli.

In 1833 the remarkable work by Witham, of Lartington, appeared, in which the study of the internal structure of carboniferous fossil plants was entered upon for the first time, with the help of the thin ground sections

[1] "Annals of Nat. Hist." vol. i., 1848.
[2] *Memoirs* of the Manchester Lit. and Phil. Soc., vol ix. 1851, and *Transactions* of the Microscopical Soc., vol. i., 1853.

invented by Nicol. This work laid the foundation of our knowledge of the structure of the *Lepidodendra* and *Stigmariæ*, and Brongniart then applied the new method, with the most brilliant success, to the investigation of *Sigillaria*. Williamson also soon attained brilliant results by its aid, studying the shells of Foraminifera, and the scales and teeth of fishes. Two papers, published in the *Philosophical Transactions* (1840 and 1851), and considered excellent by competent judges, were the result.

Naturally, the study of fossil plants, which had been so successfully begun, was not neglected, whenever such material could be obtained in the proper state of preservation, which at that time was not easy. Williamson's first attempt of the kind, the precursor of the whole palæobotanical literature which he created, was the paper "On the Structure and Affinities of the Plants hitherto known as *Sternbergiæ*," in which the *Sternbergiæ* were identified as medullary casts, which had been surrounded by an Araucarian wood. As already mentioned, however, the material for an extended use of the method was at that time still wanting. Then, just at the right moment, came the discovery of the calcareous nodules, enclosing vegetable remains, in the Ganister beds of the coal-fields of Lancashire and Yorkshire. The investigation of the treasures thus revealed was first taken in hand by their discoverer, Binney himself, and subsequently by Carruthers and Williamson. The latter first began with works on the Calamarieæ, three of which appeared in rapid succession from 1869 to 1871. They are: "On the structure of an undescribed type of *Calamodendron* from the upper coal measures of Lancashire"; "On a new form of Calamitean strobilus"; and "On the organisation of an undescribed verticillate strobilus from the lower coal-measures of Lancashire."

As was necessarily the case, material now began to accumulate in Williamson's hands, and he enjoyed the active co-operation of various zealous collectors. Then, in his fifty-fifth year, he began the great series of memoirs which mark the culminating point of his scientific activity, and which will assure to him, for all time, in conjunction with Brongniart, the honourable title of a founder of modern Palæobotany.

In the course of the following twenty years, nineteen memoirs of this series appeared in the *Philosophical Transactions*, under the general title "On the Organisation of the Fossil Plants of the Coal-measures." They all contain exclusively his own observations, made entirely on material from the British coal fields. It is a gigantic work, which by itself alone would form the abundant fruit of a man's whole life. It was supplemented, however, by various other contributions to the same subject, published in the *Memoirs* of the Literary and Philosophical Society of Manchester, the *Annales des Sciences naturelles*, and the *Annals of Botany*. During the same period, in 1887, also appeared Williamson's exhaustive "Monograph on the Morphology and Histology of *Stigmaria ficoides*," which will long form the basis of our knowledge of these fossils.

The recognition by palæontologists and botanists of the full importance of these works of Williamson's, has been of course a slow and gradual process. This was really due to external circumstances. In the first place, Williamson found it necessary, as the material in his collection, and his own experience increased, to return repeatedly in his later memoirs to plant remains which had been dealt with in the earlier parts. Consequently, if one wish to obtain an idea of any group, it is always necessary to study several of these treatises simultaneously. This, however, presents great difficulties, except to those who possess separate copies. For the reader stands helpless before a pile of sixteen volumes of the *Philosophical Transactions*!

On the other hand, there is another point which must be taken into account. Williamson's method of anatomical description, clear as it is, bears the stamp of the scholastic ideas of a past time. For this reason it is only understood with difficulty by the botanists of the present day, and must often first be translated into the form now customary. This is laborious, and has stood greatly in the way of the rapid diffusion of his results.

Williamson himself was fully conscious of these drawbacks, and finally, in order to remedy them, he began a new series of memoirs, in conjunction with Dr. Scott, the object of which was to present a connected and systematically-ordered account of the results obtained, clothed in the language of modern anatomy. The first memoir of this series appeared in 1895, in the *Philosophical Transactions*, and treats of the Calamarieæ and Sphenophylleæ. Two further papers are already completed, but he was not spared to see them published.

The basis of all Williamson's labours in fossil botany is, of course, the collection of slides which he left, containing some thousands of preparations. It is unique of its kind in the world, and of the greatest importance, for it contains the evidence for all the innumerable special observations recorded in his works. Like Willdenow's herbarium or Lindley's collection of orchids, it will always remain an invaluable source of information, to which palæontologists from all sides must resort. Its owner was aware of this, and so also is the author of this notice, who may boast that he knows the collection as scarcely any one else does. It was through him that Williamson decided to prepare and distribute, in a printed form, a detailed index, giving exact references to the individual preparations, and the places where they are cited in the memoirs. This was necessary, for the multitude of preparations often made it very laborious, even for the owner, to look out a particular section to demonstrate some special fact. This work was taken in hand about 1890, and has considerably increased the usefulness and value of the collection to posterity. Three instalments, and those the most important, have already appeared under the title, "General Morphological and Histological Index to the Author's Collective Memoirs on the Fossil Plants of the Coal-measures." Only the Cordaiteæ, the Gymnospermous seeds, and a number of fossils of doubtful affinity, are still wanting. We may, no doubt, ultimately look for a synopsis of these from the hand of a friend, so as to complete the entire work.

If we now consider the contents of the palæobotanical literature created by Williamson during the last twenty-five years of his life, we find that it consists, first of all, of the most minute description and reconstruction of all those types of plants which took part in the formation of the coal-beds of Great Britain. He abstained on principle from concerning himself with non-British material. We have acquired from him the most exact knowledge of the structure of the Calamarieæ, the Lepidodendreæ, the Sphenophylleæ, the Ferns, and Lyginodendreæ. As regards several of these groups, it is true, he had before him fairly detailed investigations by previous observers, but in other families, especially the Calamarieæ and Lepidodendreæ, he himself laid almost the whole foundation of our knowledge. He showed that both groups are, as regards their fructifications, indubitable Archegoniatæ, but that they possessed, like our recent Gymnosperms, a secondary formation of wood from a cambium; he taught us to recognise, in the Stigmariæ, the subterranean organs of the Lepidodendreæ and Sigillariæ; he reconstructed in the genera *Lyginodendron* and *Heterangium*, described by him, a type of plant which, by its characters, occupies an intermediate position between Filicineæ and Gymnosperms, especially Cycadeæ. It thus can find no place in the system of recent plants, but represents a direct derivative of the unknown ancestral stock from which the two groups still living have also sprung. In connection with this type, Renault's Poroxyleæ have since turned out to be their

later Permian relations, while the Protopityæ of the Culm are more ancient allies, with similar characteristics. We thus learn how far back we must go, in the series of geological formations, in order to meet with the last traces of the common ancestors of those classes in the vegetable kingdom which are now living.

By his discovery of Archegoniate plants with secondary growth, Williamson however came into collision with the doctrines of Adolphe Brongniart, otherwise so highly reverenced by him, who held this character to be an absolute criterion of the Phanerogams, and denied the possibility of its occurrence in other classes of the vegetable kingdom. Hence a literary feud arose between Williamson and B. Renault, Brongniart's distinguished pupil. The latter endeavoured to prove that Williamson was in error in the identification of his *Lepidodendra*, that they were really *Sigillariæ*, and together with the latter belonged to the Gymnosperms, while the truly Archegoniate *Lepidodendra* were destitute of any secondary growth. The answer was not long in coming; proof was heaped on proof, until ultimately the real state of the case was made completely clear. In all essential points victory was on the side of our author. Other subsidiary differences respecting *Stigmaria*, the Calamarieæ, &c., require no more than a mention here.

It was thus made evident by Williamson that cambial growth in thickness is a character which has appeared repeatedly in the most various families of the vegetable kingdom, and was by no means acquired for the first time by the Phanerogamic stock. This is a general botanical result of the greatest importance and the widest bearing. In this conclusion Palæontology has, for the first time, spoken the decisive word in a purely botanical question. The result has proved well worth the great trouble and labour which had to be gone through in order to attain it.

It would be difficult to conceive a more magnificent monument to Williamson than one which he himself set up at Manchester, in one of the halls of the Owens College Museum.

In the year 1887 there was discovered in a quarry near Bradford, a gigantic petrified tree-stump, which, when carefully exposed, was found to run out at the base into a widely-spreading system of ramifications of a Stigmarian character. In the quarry this precious relic, like many others before it, would in a very short time have fallen a victim to destruction by weather and the hand of man. Williamson, however, acquired it by purchase, had it carefully subdivided into numerous pieces, and brought it home safe and complete to Manchester. This was not accomplished without the greatest personal exertions and a considerable expenditure of money (to which several friends contributed), for there were whole waggon-loads of material to be removed. Then the first thing which had to be done was to secure from the University authorities the necessary space for erecting the fossil. This was not an easy matter, and great opposition had to be overcome, as we can easily understand on looking at the specimen, which measures over 29 feet in diameter.

Finally it was fitted together, piece by piece, and fixed in its natural position, resting on a massive pedestal of brickwork. The fiery youthful zeal of a man already over seventy, overcame all the difficulties that arose. People were astonished at the unusual development of energy which this *Stigmaria* had caused, and gave it, in good-humoured jest, the name of "Williamson's Folly." "Williamson's Folly" may now be reckoned among the sights of England, and Manchester may be proud of possessing it, for it represents a last gift, worthy of all honour, from the deceased, to the place which for so many years was his home and the scene of his activity.

The author of this notice, who only knew Williamson during the last years of his life, must not attempt to picture to those who lived with him his kindly and benevolent nature, which always retained the freshness of youth, or his simple character. That would be a work of supererogation, for the whole of scientific England knew and respected him, and wherever he went he was a welcome and honoured guest. The writer can only report, in all brevity, on the work of Williamson's life, and when asked to undertake this, it was with pleasure that he took up his pen for that purpose. Solms-Laubach.

NOTES.

The resignation of Dr. Albert Günther, F.R.S., of the post of Keeper of Zoology at the Natural History Museum, South Kensington, is announced. Dr. Gunther has occupied for over thirty years the position he now vacates.

The "Swiney" Lecturer this year is Dr. J. G. Garson, who will take as the subject of the twelve lectures he purposes giving, "The Geological History of Man." The lectures, admission to which will be free, are to be delivered in the lecture theatre of the South Kensington Museum on Mondays, Wednesdays and Fridays, at five P.M., beginning on Friday, October 4.

We have to record the death of two prominent members of the medical profession abroad, viz. Dr. Pasquale Landi, Professor of Clinical Surgery successively in the Universities of Siena, Bologna, and Pisa, and Dr. Texier, Professor of Internal Pathology in the Medical School of Algiers.

Mr. Charles Mitchell, whose death, at the age of seventy-five, occurred on August 22, was a well-known engineer and shipbuilder. He founded the Walker shipbuilding yard on the Tyne, a yard which under his guidance developed into one of the largest in the country. In 1882 it was merged into the Elswick Company of the present Lord Armstrong, and up to the time of his death Mr. Mitchell practically superintended the whole of the shipbuilding work of the Company.

The *Athenæum* says that during the autumn of this year a monument is to be unveiled at Osteel, in East Friesland, in memory of the discoverers of the sun's spots, David and Johann Fabricius. The site chosen is the place in the cemetery where the grave of the elder Fabricius was discovered about nine years ago.

We are informed by Prof. John Milne, that communications respecting the *Transactions* of the Seismological Society, and the *Seismological Journal*, may be addressed to him at Shide Hill House, Shide, Newport, Isle of Wight, at which place a small station has been established to record earthquakes having their origin in distant localities, and other unfelt movements of the earth's surface.

The annual general meeting of the Federated Institution of Mining Engineers will be held in North Staffordshire, at Shelton, Stoke-upon-Trent, on September 18 and 19, when papers on "The Depth to Productive Coal-measures between the Warwickshire and Lancashire Coal fields," "Gold-mining in Nova Scotia," "The Use of Steel Girders in Mines," "Economic Minerals of the Province of Ontario, Canada," and "The Blasting Efficiency of Explosives" are expected to be read, and a discussion of various papers which have already appeared in the *Transactions* of the Institution may take place. A number of excursions are also arranged.

The fifth quadrennial meeting of the International Congress of Otology will take place at Florence, under the presidency of Dr. V. Grazzi, from September 23 to 26. Various discussions will be opened by Dr. Barr of Glasgow, Dr. Gellé of Paris, Prof. Gradenigo of Turin, Prof. A. Politzer of Vienna, and Dr. Secchi of Bologna; and there are in the complete programme,

which [illegible] just been issued, the titles of no fewer than fifty-nine original communications to be brought before the meeting. It is hoped that British otology will be well represented, as it is intended to invite the next congress to meet in London, either in 1898 or 1899. Full particulars as to terms of membership, routes, hotels, &c., may be obtained from Dr. St. Clair Thomson, 28 Queen Anne-street, W.

AN International Congress of Technical, Commercial, and Industrial Education is being organised by the Société Philomathique of Bordeaux, and is to be held at Bordeaux from September 10 to 21. The programme is, we understand, a full one, and contains many items of interest and importance.

A FEATURE of the annual meeting of the Yorkshire Naturalists' Union, which is to take place at York on October 30, will be an exhibition of specimens, photographs, &c., showing work done during the past year in all departments of the Union. It is requested that all members who intend to exhibit will communicate direct with the Local Secretary, at the Museum, York, on or before October 21.

THE various medical schools will be reopened at the beginning of October, and at most of them introductory addresses will be delivered to the students. On October 1, at St. George's Hospital, the speaker will be Mr. George D. Pollock; at the Middlesex Hospital Dr. W. Julius Mickle, and at the Westminster Hospital Dr. Monckton Copeman. At the latter institution Viscount Peel will distribute the prizes. The introductory address at University College will be delivered by Prof. J. Rose Bradford, and the annual dinner of old and present students will take place at the Hôtel Métropole on October 1, under the chairmanship of Sir Richard Quain, Bart. Mr. A. P. Laurie will give the address at St. Mary's, and the annual dinner will be held the same evening at the Holborn Restaurant, Mr. Malcolm Morris occupying the chair. At St. Thomas's Hospital the prizes will be distributed, on October 2, by Sir Edwin Arnold, K.C.I.E. At Guy's there will be no formal introductory address, but on the evening of October 1 Mr. J. De'Ath will read a paper at the opening meeting of the Physical Society, on "Our Profession, our Patients, our Public and our Press." The annual dinner will take place in the Club Dining Hall, Dr. Pye-Smith in the chair. At the Yorkshire College, Leeds, Prof. D. J. Leech will, on October 1, distribute the prizes and deliver an address. Prof. Victor Horsley is announced to speak at the Sheffield School of Medicine, Mr. Jonathan Hutchinson at University College, Liverpool, and Prof. I. H. Napier at St. Mungo's College. At Mason College, Birmingham, Prof. Percy Frankland will deliver the address, taking as his subject "Pasteur and his Work."

THE Council of the Institution of Civil Engineers has issued a list of suggested subjects for papers during the session 1895–96, for which the undermentioned prizes may be awarded: (1) The Telford Fund, left "in trust, the interest to be expended in annual premiums, under the direction of the Council." The bequest (with accumulations of dividends) produces a gross amount of £235 annually. (2) The Manby Donation, of the value of about £10 a year, given "to form a fund for an annual premium or premiums for papers read at the meetings." (3) The Miller Fund, which, with accumulations of dividends, realises nearly £140 per annum. Out of this the Council has established a scholarship, called "The Miller Scholarship," and is prepared to award one such, not exceeding £40 in value, each year, and tenable for three years. Competitors for this scholarship must be under the age of twenty-five years. (4) The Crampton Bequest of £500, the annual income of which amounts now to £13 14s., [illegible] foundation of "The Crampton Prize," for "presentation to the author of the best paper on the Construction, Ventilation, and Working of Tunnels of Considerable Length, or failing that, then of any other subject that may be selected." (5) The balance of the Trevithick Memorial Fund of £100 0s. 9d., the interest of which is £2 15s. a year. The list of suggested papers, although not exhaustive, is far too long for us to print, but may be had, with further information, upon application to the Secretary of the Institution.

THE Royal Academy of Medical, Physical, and Natural Sciences of Havannah, at a meeting held on April 28, decided to offer amongst other prizes, mostly for medical essays, one—the Cañongo Prize, value 250 dollars in gold—for the best essay on "The Pharmacological Study of the Fluid Extracts." The competition, which is open to any person whether belonging to the medical profession or not, will be closed on March 19, 1896, by which date all papers must be sent in, written in French or Spanish, and sealed, with a motto on the internal envelope, and in another envelope bearing the same motto the author's name and address. The adjudication will take place on May 19, 1896, when the prizes will be distributed to the successful competitors. Further particulars may be obtained by writing to the Secretary, Dr. Vicente de la Guardia, Havannah.

UNDER the active presidency of the Earl of Derby, a vigorous effort is being made by the British Dairy Farmers' Association to give a helping hand to one of the most important branches of agriculture, dairy farming, and its allied industry of poultry raising. At the twentieth annual London Dairy Show, to be held at the Royal Agricultural Hall in October next, prizes to the value of £2515, in addition to 142 gold, silver, and bronze medals, are offered for competition in 451 different classes, in many of which a keen contest is already assured.

AN interesting memoir has been recently published by Dr. Max Müller, on the effect of fever temperature upon the growth and virulence of the typhoid bacillus. In view of the conflicting opinions which have from time to time prevailed on the manner in which a high temperature affects the agent of infection in cases of typhoid fever, these results are of some considerable practical interest. Thus in 1882 we find Jorgensen ventilating the idea that the development of the morbific material in the system in cases of typhoid fever might be retarded by greatly reducing the temperature of the body, whilst other authorities have as confidently stated that the feverish rise in temperature was capable of destroying the typhoid organism, or, at any rate, hindering its development. Both of these opinions are based on very slender experimental evidence. Dr. Max Muller has carried out a series of researches in which he has carefully recorded the growth of the typhoid bacillus at various temperatures, and he states that when preserved at about 40° C. this microbe takes five minutes longer to proliferate, or produce a new generation, than when it is kept at a temperature of from 37·5° to 38·0° C. respectively; that is to say, in the absence of all adverse circumstances, under the most favourable conditions, as many as forty-five generations of typhoid bacilli may proceed in one day from a single parent bacillus at the *normal* temperature of the body, whilst at about 40° C. thirty-nine such generations may be elaborated. In considering these appalling figures it must, however, be remembered that such an uninterrupted multiplication of the typhoid bacillus does not necessarily take place in the human system; the conditions which surround it in the latter case are of a far more complicated and subtle character than those which obtained in Dr. Müller's laboratory culture-tube! But these results show that a fever temperature of about 40° C. is not able to destroy the typhoid bacillus, or to affect its growth to any considerable extent; even higher temperatures of 41·5° to 42·0 C. were also incapable of annihilating this microbe, and typhoid bacilli kept for sixty-two days at 42·0° C. showed subsequently no abatement of their vitality. As regards

the effect of such temperatures on the virulence of the typhoid bacillus, Dr. Müller states, but only as the result of very limited experiments, that he could detect no difference in the behaviour in this respect of those kept at 37° and 40° C. respectively.

A MODIFIED centesimal system of subdividing time and angular measures is advocated by M. H. de Sarranton, in the *Revue Scientifique*. He proposes to retain the hour as a fundamental unit of time, on account of its universal acceptance, its convenience, and the hopelessness of the task of altering it. But the hour should be divided into 100 minutes, and the minute into 100 seconds. Thus each new minute would be three-fifths of an old minute, or thirty-six seconds, while the new second would be a little over a third of the present second. Two of the new seconds would cover the time of a brisk step, like the accelerated pace used in the French army. The new second is the time taken by one semi-vibration of a simple pendulum 12·9 cm. long. Time could then be consistently expressed in hours and decimals. Thus 8·3348 h. might be read 8 hours 33 (new) minutes 48 (new) seconds, and calculations involving time would be much simplified. Clock and watch dials would be subdivided into hours, as usual, but the smaller divisions for the minute and seconds hands would be hundredths of the circle instead of sixtieths, and every tenth division would have to be slightly marked. For angular measurement, M. de Sarranton proposes 240°, subdivided into 100 minutes of 100 seconds each, so that they could be converted into hours by shifting the decimal point one place to the left.

A FEW particulars of the new mouth of the Vistula are given in the *Globus*. It was made by regulating the old branch going into the Baltic, which was straightened and shortened from ten miles to four and a half, while the channel was broadened by shifting the dyke on the left bank six miles to the west. At the same time, the Danzig branch was cut off by a lock. This useful piece of work will not only make the Vistula more accessible, but will prevent the disastrous floods which caused far-reaching destruction in winter and spring, near the mouth of the river. The work cost a million pounds, half of which was borne by the districts concerned, and half by the German Treasury.

THE current number of the *British Medical Journal* has a note on the vision of School Board children, based upon a report of Dr. James Kerr, medical superintendent of the Bradford School Board. The tests employed were designed to detect every child who had not good distant vision with one eye at least, the list of children thus obtained including those with defect of distant sight from all causes, remediable or otherwise. Such a list having been made, it was an easy matter to more fully examine all the children thus tabulated, and to classify and deal with them as might be necessary. In the report, tables are given showing the number of children examined, and the percentage of defective eyesight in the different standards from one to seven. A perusal of Dr. Kerr's report will, in the opinion of our contemporary, well repay those who have to conduct similar examinations of large numbers of school children.

WE have received from the Deutsche Seewarte (Hamburg) the report of its labours during the year 1894. The duties of this institution differ materially from those of the German Meteorological Institute (Berlin), whose report we lately noticed, inasmuch as the former deals specially with weather prediction and marine meteorology. In both of these branches great activity is shown, and we have frequently referred to the useful work carried on. The detailed discussion of the meteorology of the various oceans, for the benefit of seamen, the preparation of synoptic weather charts of the North Atlantic Ocean, for the advancement of practical meteorology, and the publication of observations taken in remote parts of the world, are noteworthy instances of the industry of the institution. For the purpose of obtaining information relating to maritime meteorology, it has not only established many agencies in German ports, but the Consuls in several foreign ports, including English, also take part in enlisting observers and supplying the necessary registers; the result being that about 450 voluntary observers were co-operating at the end of the year in the mercantile marine alone.

THE annual report of the Department of Mines and Agriculture, New South Wales, for the year 1894, has come to hand. In it reference is made to the resignation of the position of palæontologist of Mr. Robert Etheridge, occasioned by his accepting the curatorship of the Australian Museum. Mr. Etheridge will, however, we are pleased to notice, still retain connection with the department, having, the report says, volunteered to act as honorary consulting palæontologist.

WE have received from the Keeper of the Manchester Museum, Owens College, a new handy guide to the museum, which has been compiled for the purpose of indicating cursorily the principal objects in the building and its general arrangement, for the benefit of visitors whose time is limited. To those who can afford time to pay several visits, the illustrated guide is recommended as being more complete and useful.

THE new part of the *Asclepiad*, Sir B. Ward Richardson's quarterly, contains articles on "Cycling and Heart Disease," "The late prevailing Epidemic," and, with portrait, "John Abernethy, F.R.S."

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*, ♂ ♀) from India, presented by Mr. Hugh H. Collis; a White-tailed Sea-Eagle (*Haliætus albicilla*) from Northern Russia, presented by Mr. Robert Ashton; two Red-backed Shrikes (*Lanius collurio*), British, presented by Mr. C. Ingram; a Natterjack Toad (*Bufo calamita*) from Surrey, presented by Mr. Hanley Flower; a Melodious Jay Thrush (*Leucodioptron canorum*), deposited, a —— Capuchin (*Cebus ?* ♂), a Porto Rico Pigeon (*Columba corensis*), a Vinaceous Pigeon (*Columba vinacea*), a Barn Owl (*Strix flammea*), seven Adorned Ceratophrys (*Ceratophrys ornata*) from Brazil, purchased; a Great Kangaroo (*Macropus gigantus*, ♂), a Rufous Rat Kangaroo (*Hypsiprymnus rufescens*, ♂), a short-headed Phalanger (*Belideus breviceps*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE FORMS OF JUPITER'S SATELLITES.—A paper, by Mr. S. I. Bailey, on the forms of the discs of Jupiter's satellites, is communicated by Prof. E. C. Pickering to the current *Astrophysical Journal*. A number of observations of the satellites has been made with the thirteen-inch refractor at the Arequipa Observatory; and the results were: "Under the best conditions, that is, with the instrument in perfect adjustment and good seeing, satellites II., III. and IV. were always seen round. Satellite I. was twice seen having an apparent elongation in the same direction as Jupiter. In both cases the satellite was near the planet. On the second occasion, I., when off the disc, but near Jupiter, appeared elongated, but an hour later, plainly seen on the disc of Jupiter, it appeared perfectly round. On the other hand, the shadows of I. and III. on other nights were seen elongated. Several occultations and transits were observed, but the limb of Jupiter was not seen when, to me, it gave any indication of transparency. . . . During the hours given, we failed to detect any systematic change of form in any of the satellites. These observations, scattered through the cloudy season, may not be the best possible, for the same observers and instrument in Arequipa, nevertheless it does not seem probable to me that any frequent periodic recurrence of an ellipticity, approximating in amount that of Jupiter itself, would have escaped detection."

EPHEMERIS OF SWIFT'S COMET.—The following elements and ephemeris of Swift's comet, the reappearance of which was announced last week, have been computed by Dr. Berberich, and are published in *Edinburgh Circular*, No. 45. The elements are deduced from the observations: Mount Hamilton, August 21; Nice (M. Javelle), August 24; Hamburg, August 25.

Another observation of August 23, made by Mr. J. Witt at the Urania Observatory, Berlin, is closely represented by the ephemeris. Dr. Berberich thinks the comet will possibly belong to the group of periodic comets with short revolution.

Elements.

T = 1895, Sept. 3·3030, M.T. Berlin.

ω = 170 37·04 }
[illegible] = 172 50·05 } Mean Equinox 1895·0.
i = 4 38·55 }
log [illegible] = 0·10537.

Ephemeris for Berlin Midnight.

1895.	R.A. h. m. s.	Decl.	log. Δ.	log. r.	Brightness.
Sept. 4	1 0 7	+6 17·3			
6	1 4 3	6 18·0	9·7100	0·1056	1·17
8	1 7 50	6 17·5			
10	1 11 28	6 15·9	9·7071	0·1604	1·18
12	1 14 57	6 13·2			
14	1 18 16	6 9·6	9·7062	0·1679	1·18
16	1 21 25	6 5·1			
18	1 24 23	5 59·7	9·7073	0·1700	1·16
20	1 27 11	5 53·6			
22	1 29 49	5 46·9	9·7107	0·1727	1·16

The brightness at August 21·5 has been taken as unity.

COMETS AND THE SUN-SPOT PERIOD.—Since the discovery of the periodicity of the sun-spots, investigations have shown that many terrestrial phenomena are, and others may be, closely allied to it. These are generally looked upon as results due to the variation in the sun. If it were found that comets had an eleven yearly period, we should have the question before us as to whether this period be the result of this period, or whether the period depended to a certain extent on this periodical cometary influx. If the sun, as has been supposed, were fed, so to speak, with cometary matter, then the spot period would naturally be dependent on some external source of supply such as this. But since the solar atmosphere has a circulation which seems now to have been fairly shown to be the cause of this periodicity, such an outward supply of energy is not thought now to be of such importance as would have been the case some years ago. This does not take away the interest, however, from Herr J. Unterweger's investigation concerning the connection of spots and appearances of comets, but would rather instigate it. The author has, by a strict examination of the elements of the larger periodical comets, obtained a function which can be represented mathematically by a formula, and from which an eleven-yearly period since 1740 can be recognised. From the year 1833, also, the maxima and minima points fall together, without exception, with those of the sun-spot curves. In determining the length of the period, the amplitude of the period was set for each series as a function of the length of the period, and thus each value for the duration of the period ascertained. The calculation was so arranged that two neighbouring values, which made the amplitude a minimum, were also determined. The values for the function came out as 8·682, 11·220, 13·365 years, those for the series showing the relative number of sun-spots being 8·721, 11·254, 13·424 years.

To determine also whether the points of maxima and minima of the function were coincident with those for the series showing sun-spot numbers, the curves drawn from the values derived [illegible] to such an extent that a secondary maximum could be [illegible] of them in similar positions.

Other [illegible] which the investigator indicates as having been [illegible] identification of the thirty five yearly sun-spot period, the function giving larger values in 1778, 1816, 1848 and 1882, and smaller minima in 1764, 1806, 1834 and 1867; the time between two successive maxima being in the mean 34·8 years.

The [illegible] period 1764–1806, with maximum at 1777–80, [illegible] number of sun spots and a large [illegible] which began in 1768 and ended about 1785. The 1806–1834 interval, with a maximum at 1816, corresponds to a maximum of sun-spot and to an intense *glets herverstors* from 1814 to 1824. The third and fourth periods are also likewise explained.

Cases are also made out for the secular variations in the climate, and a suggestion is thrown out that if we may look upon "Kometen als stark elektrische Massen," then at the times of their maximum number and least distances from the earth, small induced currents may be set up, which will be recorded by the magnetic needle; this latter question has not, however, been investigated.

THE SUN'S PLACE IN NATURE.[1]

XI.

The Clock Rate.

THE proper regulation of this clock error and consequent "trail" of the spectrum across the plate parallel to itself are essential to the success of photographs taken by the objective prisms. The spectrum of a bright star must obviously be made to trail more quickly than that of a fainter one, and a shorter exposure is sufficient. Since for the same clock error, and in the same time, a star near the pole will give a shorter trail than one nearer the equator, declination must also be taken into account. Keeping a constant clock error, equal widths of spectrum for stars of different declinations may be obtained by lengthening the time of exposure for stars away from the equator, but in that case, the stars near the pole would be over-exposed in relation to those nearer the equator.

The exposure given to stars of equal magnitudes should evidently be the same, no matter in what part of the sky they may be situated, and the clock error should, therefore, be increased in proportion to the secant of the angle of declination.

The light-ratio of stars being $2·512^n$, where n expresses the difference in magnitude, the time of exposure must vary in the same proportion, and the clock error in inverse proportion. Thus, where 5 minutes' exposure is sufficient for a first-magnitude star, 31 minutes is required to obtain a fully-exposed spectrum of a star of the third magnitude. This law, however, only applies to photographic magnitudes, and must be modified according to the type of spectrum or the colour of the star.

The red stars, being much weaker in blue and violet rays than the yellow or white stars, require much longer exposures than white stars of equal magnitude. To obtain a spectrum of β Pegasi extending to the K line, for example, at least three times the exposure required by a white star of similar magnitude must be given.

For conveniently adjusting the exposures, tables have been constructed which show at a glance the position of the regulator for a star of given magnitude and declination.

It is obvious that with an instrument of high dispersion, the number of stars it is possible to photograph is very limited, as the long exposures required for the fainter stars are impracticable, and, even if possible, the definition of the lines would be destroyed by atmospheric tremors.

Hence, it is at present only possible to photograph the spectra of the faint stars on a very small scale. With an objective of 8 inches aperture and 44 inches focal length, and a prism of 13° refracting angle, Prof. Pickering has photographed the spectra of stars down to the eighth magnitude. These spectra are about 1 centimetre long, and a millimetre broad, and though they do not show a very great amount of detail, they are sufficient to reveal the type of spectrum.

With an instrument capable of photographing faint stars, a large number of spectra may be taken at one exposure; but, with the instruments of larger dispersion, this is not generally the case, as there are few bright stars of nearly equal magnitude sufficiently close together.

The Electrical Control.

In consequence of the great accuracy required in the driving of the telescope when long exposures are necessary, the 10-inch equatorial has been fitted with a simple and inexpensive form of electrical control. This is a modification of that designed by Mr. Russell, of the Sydney Observatory.[2] The existing driving

1 Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 4[illegible].)

2 *Monthly Notices* [illegible]

gear has been altered so that the driving rod performs its revolution in a second, and the motion is then communicated to the driving screw through a small worm wheel. The driving rod is vertical and in two parts, the lower portion ending in a faced ratchet wheel, 3 inches in diameter, and with 200 teeth. The upper part of the rod ends in an arm at right angles to itself, and this arm carries a ratchet of suitable shape held down by an adjustable spring. An electro-magnet connected with the controlling pendulum, is arranged so as to only permit the ratchet to pass it once a second (see Fig. 42). If the clock be driving too quickly, the ratchet is held until the stop is raised by the pendulum. When held in this way the ratchet is lifted out of the teeth, and the driving clock itself is not affected.

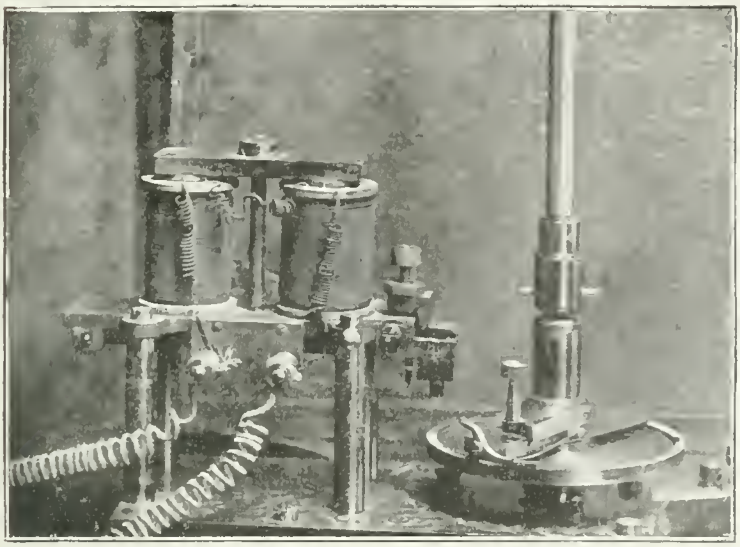


FIG. 42. Electrical control for 10-inch equatorial.

In order that this form of control may be effective, it is essential that the clock should be going too quickly, as it is only capable of retarding the driving-rod.

The controlling pendulum is, of course, regulated to the rate required for the particular star which is being photographed.

In Mr. Russell's form of control the two parts of the driving rod are connected by friction plates. It was found, however, on testing this arrangement, that when the upper portion was held by the electro-magnet the rate of the governors was seriously retarded; hence I introduced a ratchet wheel, and its working leaves nothing to be desired.

Enlargement of the Negatives.

Many of the negatives taken have been enlarged about nine times on glass, and further copies have been taken on bromide paper, bringing the enlargement up to about twenty-five times the size of the original.

Owing to various causes the photographic spectra obtained by the method of trails show irregularities resembling the lines along the spectrum observed when the slit of a spectroscope is partly clogged with dust. It has been noticed that the period of the irregularities is equal to the time of revolution of the main driving screw of the telescope, and hence they may be accounted for by supposing the driving gear to be mechanically imperfect. In that case some of the parallel lines which, by their juxtaposition form the broadened spectrum, are superposed, while others are drawn apart, thus giving rise to dark and bright lines parallel to the length of the spectrum. These lines are more apparent in the case of bright stars than fainter ones. If the telescope were driven with perfect regularity and the atmosphere were quite steady, we should obtain a spectrum of uniform intensity along its width. This condition has very nearly been obtained in some cases.

The irregularities above described are eliminated in the enlarged negatives by giving them a very slight up-and-down motion during exposure in a direction parallel to the lines of the spectrum. This was originally done by hand, but a negative holder has been constructed in which the necessary motion is given to the negative by a small driving clock.

A diagram of the arrangement is given below. The only drawback to this method is that defects of the film are apt to produce, by a succession of their images on the enlarging plate, lines (generally very faint) which have a semblance of the true spectrum lines.

To distinguish the real lines from the artificial ones, a direct enlargement of the spectrum is made on the same plate alongside the other, the to-and-fro motion being dispensed with. By a comparison of the two enlarged strips, one can see at a glance which are the true lines of the spectrum, and which are those produced by small irregularities on the film. It may be stated that Dr. Scheiner has also used a somewhat similar method to the one described, the only difference being that he caused the plate on which the enlargement was to be taken to have the oscillating motion, instead of the original negative. The method employed by me, though no account of it had been published, had been in use for some time before Dr. Scheiner's method was announced.[1]

My object was not so much to obtain photographs of the spectra of a large number of stars, as to study in detail the spectra of comparatively few; hence many of the stars have been photographed several times with special exposures and foci for different regions of the spectrum.

As in the case of stellar spectra observed by eye, the photographic spectra vary very considerably in passing from star to star.

In the classification of stars adopted from a consideration of the visual observations, only the broader differences in the spectra have been taken into account. Prof. Pickering, however, has suggested a provisional classification in connection with the Henry Draper Memorial photographs of stellar spectra, but this chiefly relates to photographs taken with small dispersion. Now that it has become possible to obtain large dispersion photographs of the spectra, much more detail is revealed, and hence I determined to deal with the presence, or absence, or changes of intensity, of individual lines to a greater extent than Prof. Pickering has done in his observations so far published.

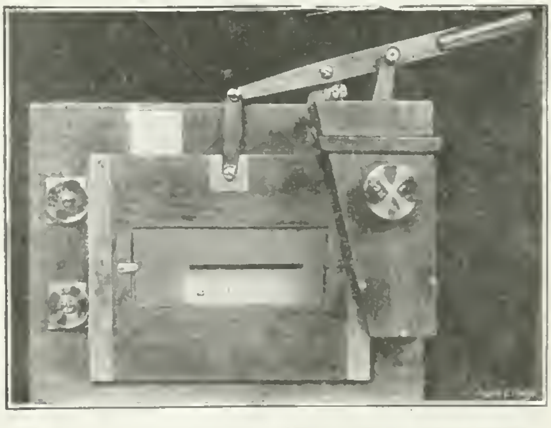


Fig. 43. Negative holder used in enlarging

In the first instance, I arranged the various stars of which the spectra have been photographed in tables, without reference to any of the existing classifications, and not taking into account the finer details.

The basis upon which this first grouping was founded is the extent of the continuous absorption at the blue end of the spectrum. Such a distinction was not possible in the case of

[1] NATURE, vol. xlii. p. [illegible]

eye observations, and it is only by photographs that a classification from this point of view can be made.

Some spectra show a remarkable continuous absorption either in the ultra-violet or violet, in others this absorption extends to about K, whilst in a third class it reaches as far as G.

These considerations gave four marked groups. Each of these main groups are next sub-divided into sub-groups by the most marked differences in the spectral lines. I do not propose to give the detailed inquiry in this place.

The important fact which stood out when the photographic attack had got so far was that, whether we take the varying thicknesses of the hydrogen lines or of the lines of other substances as the basis for the arrangement of the spectra, it was not possible to place all the stars in one line of temperature, but it was necessary to arrange the stars in two series.

When this sorting was completed, I was in a position to consider the various divisions of the photographic spectra thus arrived at, in relation to the groups which were previously suggested from a discussion of eye observations. It is clear that if I got the same results the first conclusions would be strengthened.

We have, therefore, to inquire how far this condition is satisfied by the mass of new facts at our disposal. This involves the consideration of some points in connection with the meteoritic hypothesis, and it must specially be borne in mind that the fundamental difference between mine and other classifications is that it demands the existence of bodies of increasing as well as bodies of decreasing temperatures.

Since in my classification the connection between nebulæ and stars is insisted on, it was necessary to obtain a spectrum of one of the brightest of the nebulæ as a term of comparison. The nebula of Orion was selected, and a photograph taken with a 30-inch silver on glass reflector in February 1890. This photograph contained 54 lines, which were carefully tabulated for the purposes of the comparison to which reference has been made.

The Complex Origin of the Spectra of Nebulæ.

On the hypothesis, the bright lines seen in the nebulæ should have three origins:

(1) The lines of those substances which occupy the greatest volume (or largest area in a section); in other words, the lines of those substances which are driven furthest out from the meteorites and occupy the interspaces, when possibly they may be rendered luminous by electricity. Chief among these, from laboratory experiments, we should expect hydrogen, and next, from the same experiments, we should expect gaseous compounds of carbon.

(2) We are justified in assuming that the most numerous collisions will be partial ones—grazes—sufficient only to produce comparatively slight rises in temperature. The nebula spectrum, so far as it is produced by this cause, will therefore depend upon the phenomena produced in greatest number, and we may hence expect to find the low temperature lines of various metallic substances.

(3) In addition to the large number of partial collisions there will be a relatively small number of end-on collisions, producing very high temperature,[1] and, so far as this cause is concerned, there will be some lines produced which are associated with very high temperatures.

Combining these conclusions, in the spectra of nebulæ we should expect to find evidence of

Hydrogen and compounds of carbon.
Low temperature metallic lines and flutings.
Lines which are only produced at very high temperatures.

The Passage to Bright-line Stars.

On the hypothesis, the lines seen in the spectra of bright-line stars should, in the main, resemble those which appear in nebulæ. They will differ, however, for two reasons:—

(1) Owing to partial condensation of the swarm the hydrogen area will be restricted, and the bright lines of hydrogen will lose their prominence; the volume occupied by the carbon compounds will be relatively increased, and the brightness of the carbon bands will be enhanced.

(2) On account of the increased number of collisions, more meteorites will be rendered incandescent, and the continuous spectrum will be brighter than in nebulæ.

[1] [illegible] *Proc.*, vol. liii, p. [illegible]

Stars of Increasing Temperature.

Initially, each pair of meteorites in collision may be regarded as a condensation.

Ultimately, when all the meteorites are volatilised, there will only be one condensation, in the shape of a spherical mass of vapour. Between these points there must be other conditions.

(Stage 1.) At the stage of condensation immediately following that of the bright-line stars, the bright lines from the interspaces will be masked by corresponding dark ones produced by the absorption of the same vapours surrounding the incandescent meteorites. One part of the swarm will give bright lines, another dark lines at the same wave-lengths, and these lines will therefore vanish from the spectrum. The interspaces will be restricted so that absorption phenomena will be in excess, and the first absorption will be that due to low-temperature vapours, that is, fluting absorptions of various metals. The radiation spectrum of the interspace will now be chiefly that of the compounds of carbon. Under these conditions we know from laboratory experiments[1] that the amount of continuous absorption at the blue end will be at a maximum.

(Stage 2.) With further condensation the radiation spectrum of the interspaces will gradually disappear, and the fluting absorptions will be replaced by dark lines, for the reason that the incandescent meteorites will be surrounded by vapours produced at a higher temperature, the number of violent collisions per unit time and volume being now greatly increased. This dark line spectrum need not necessarily resemble that of the Sun.

(Stage 3.) The line absorption and the continuous absorption at the blue end of the spectrum will diminish as the condensations are reduced in number, for the reason that only those vapours high up in the atmospheres surrounding the condensations will be competent to show absorption phenomena, in consequence of the bright continuous spectrum of the still disturbed lower levels of those atmospheres.

Among the more important lines which will disappear at this stage will be those of iron, for the reason that there will be bright lines from the interspaces occupying the same positions as the dark lines produced by the absorption of the vapour surrounding the stones.

The number of violent collisions per unit time and volume being further increased, we should expect the absorption of very high temperature vapours.

The Hottest Stars.

Ultimately, then, we should expect that the order of the absorbing layers will follow the original order of the extension of the vapours round the meteorites in the first condition of the swarm, and the lines seen bright in nebulæ, whatever their origins may be, should therefore appear almost alone as dark lines in the hotter stars, and the hydrogen especially should have its lines broadened with each increase of depth in the atmosphere. The continuous absorption at the violet end of the spectrum will be at a minimum. If, when the hydrogen lines are thick the swarm is not yet completely condensed, that is, if there be nebulous matter surrounding the central mass of vapour, a fine bright line will be seen down the centre of each dark one.

Stars of Decreasing Temperature.

When we consider the cooling condition, that is, what happens when the temperature of the mass of vapour is no longer increased by the fall towards the centre of meteorites composing the initial swarm, we should expect to find the phenomena indicated below.

(Stage 1.) The hydrogen lines will begin to thin out, on account of the diminishing depth of the absorbing atmosphere, and new lines will appear.

The new lines will not necessarily be the same as those observed in connection with the stars of increasing temperature.[2] In the latter there will be the perpetual explosions of the meteorites affecting the atmosphere, whereas in a cooling mass of vapour we have to deal with the absorption of the highest layers of vapours. Those lines which will first make their appearance, however, will be the longest low temperature lines of the various chemical elements.

[1] Lockyer and Roberts-Austen, *Roy. Soc. Proc.*, 1875, p. [illegible]
[2] *Roy. Soc. Proc.*, vol. xlv, p. [illegible]

(Stage 2.) The hydrogen lines will continue to thin out, and when the absorption of the hotter lower layers makes itself felt the spectra will show the high temperature spectra of the various chemical elements, showing many more lines. The difference between these and the lines seen in stars of increasing temperature should be one due to the different percentage composition of the absorbing layers, so far as the known lines are concerned.

With this increasing line absorption there will be a recurrence of the continuous absorption in the ultra-violet.

(Stage 3.) With the further thinning of the hydrogen lines and reduction of temperature of the atmosphere, the absorption flutings of the compounds of carbon should come in.

So much, then, for what we should expect, assuming the hypothesis to be true.

I now proceed to show how far these requirements are satisfied by the mass of new facts now at our disposal.

THE ACTUAL PHENOMENA RECORDED ON THE PHOTOGRAPHS.

Nebulæ.

The photographs of the spectrum of the Orion Nebula show lines at wave-lengths which approximate very closely to the lines of hydrogen, to flutings which appear in the spectra of compounds of carbon, to a fluting of magnesium at 5006, and to the longest flame lines of iron, calcium, and magnesium.

The chromospheric line designated D_3 has been recorded in the visual spectrum of the Orion Nebula by Dr. Copeland,[1] and the observation has since been confirmed by Mr. Taylor.[2]

The line which is always associated with D_3 in the spectrum of the chromosphere, viz. that at λ 4471 (Lorenzoni's *f*), is also shown in the photograph of the spectrum of the Orion Nebula.

The requirements of the hypothesis with regard to nebulæ are therefore met in every point so far considered by the new facts.

Dividing up the lines into the three groups of origins suggested, we have in the case of the Orion Nebula :—

(*a*) Spectrum of large interspace (= that of non-condensable gases driven out of the meteorites) = lines of hydrogen ; flutings of carbon.

(*b*) Spectrum of vapours produced by the large number of partial collisions = fluting of magnesium at λ 5006 ; low temperature lines of iron, calcium, and magnesium.[3]

(*c*) Spectrum of the vapours produced at a very high temperature by the relatively small number of end-on collisions. The solar chromosphere may be taken as indicating the spectrum associated with this very high temperature = chromospheric lines, $D_3 + \lambda\,4471$.[4]

Bright-Line Stars.

Prof. Pickering has shown that the Draper Memorial Photographs (copies of which he has very kindly forwarded me) prove that bright-line stars are intimately connected with the planetary nebulæ, the lines in the spectra being almost identical.

The main point of difference is that the chief nebular line near λ 5006 is not seen in the spectrum of bright-line stars, and this no doubt is due to the relative absence of feeble collisions as condensation goes on. The brightening of this line in the spectra of Nova Cygni and Nova Aurigæ, as the stars faded away, is sufficient evidence that it is associated with low temperature, and hence it is not surprising to find that it is absent from the spectra of the bright-line stars, which on this hypothesis are hotter than the nebulæ, since they are more condensed.

I have stated that we should expect the hydrogen lines to be fainter, and the carbon flutings, and the continuous spectrum to be brighter than in nebulæ.

(*a*) The hydrogen lines are decidedly less prominent. Indeed they were not recorded at all in the eye observations of γ Argûs (Arg.-Oeltz., 17681), of Wolf and Rayet's second and third stars in Cygnus,[1] but they are shown in Prof. Pickering's photographs.

(*b*) In my previous discussion of these bodies[2] I showed that there was evidence of a very considerable amount of carbon radiation in the visible region of the spectrum. Subsequent work and an examination of Prof. Pickering's photographs have strengthened this view.

(*c*) There can be no question as to the continuous spectrum being brighter in bright-line stars than in nebulæ.

Stars of Increasing Temperature.

(Stage 1.) We should expect the spectra to show—

(*a*) Absence of bright lines.

(*b*) The presence of dark metallic flutings.

(*c*) The presence of bright carbon flutings.

(*d*) Continuous absorption in the violet.

Many of the stellar photographs answer these requirements.

(*a*) They show no bright lines under normal conditions, but if the stars are variable, the disturbances which bring about the change of luminosity at maximum, produce bright lines in the spectrum as in the case of the spectrum of Mira Ceti photographed by Prof. Pickering.

(*b*) Dark flutings have been photographed in several spectra.

(*c*) The photographs appear to show the actual presence of carbon radiation ; further photographs are being obtained to carry on the inquiry.[3]

The stars of this class which have already been photographed at Kensington are well advanced in condensation, as indicated by the numerous dark lines, and all the flutings, both bright and dark, are confined to the region less refrangible than G. We should therefore not expect to get the more refrangible carbon flutings. It is among the least condensed stars that we should expect the bright carbon to be more manifest, and, indeed, in the spectrum of Mira Ceti photographed by Prof. Pickering, there is strong evidence of the presence of one of the more refrangible carbon bands commencing at λ 4215.

(*d*) The photographs fully demonstrate that there is a very considerable amount of continuous absorption in the ultra-violet or violet.

It must be added that the sequence of the spectra photographed resembles that deduced from eye observations, and the wonderful thing is that the observations of Dunér will bear the severe test which has thus been applied to them.

(Stage 2.) At this stage we should expect—

(*a*) Diminution in the amount of continuous absorption.

(*b*) Spectrum consisting of dark metallic lines, but possibly differing from the solar spectrum.

These conditions are fulfilled by the stars of which α Tauri and γ Cygni may be taken as types. The continuous absorption is least in the latter. These spectra show numerous metallic lines, but they do not exactly resemble the solar spectrum. The hydrogen lines are comparatively thin, while other lines have very different intensities as compared with lines in the solar spectrum.

In these stars we have to deal with the varying volatilities of the meteoretic constituents of the swarm, while in the case of stars which are cooling we have to deal with successive combinations rendered possible by the fall of temperature in a gaseous mass. Hence differences in the spectra are to be expected.

(Stage 3.) The phenomena which would be expected on the hypothesis, at this stage, are fully satisfied by such stars as α Cygni, β Orionis, ζ Orionis, ϵ Persei. In these stars there is

[1] *Monthly Notices*, vol. xlviii., p. 360.

[2] *Ibid.*, vol. xlix. p. 124.

[3] I have previously given evidence deduced from eye observations, indicating the presence of other low temperature flutings of manganese and magnesium.

[4] Since the lectures were delivered (and in this I summarised a paper I had previously sent in to the Royal Society), this part of the hypothesis has been enormously strengthened by the discovery of a new series of gases which the spectrum indicates are associated with the one giving the line D_3 which I discovered in 1868 and named helium. These new gases contain many lines in addition to D_3 and 4471, which appear both in the solar chromosphere and nebula of Orion and stars of increasing temperature.

[1] *Roy. Soc. Proc.* vol. xliv., pp. 33-43.

[2] *Ibid.*

[3] Subsequent eye observations by myself and Mr. Fowler seemed to leave no doubt as to the presence of these bright carbon flutings (*Roy. Soc. Proc.*, vol. xlvii. p. 40). Dr. Copeland had previously made important observations of "Nova" Orionis with reference to this point (*Monthly Notices*, vol. xlvi. p. 112), and he identified one of the bright bands as "the great hydrocarbon band seen in the spectrum of every comet that has been examined under favourable circumstances." Referring to his observations of α Orionis, Mr. Maunder ("Greenwich Spect. Observations," 1889, p. 22) states that "the carbon band at 5164 was coincident (within the limits of observation with this dispersion) with the bright space towards the blue of Dunér's band 7."

[illegible] the violet or ultra-violet, and the [illegible] is [illegible] absorption, the iron lines quite [illegible] after such a star as α Cygni is passed. The new lines which now make their appearance include the chromospheric line at λ 4471, and possibly a few others. It is important to note that the photographic region of the spectrum of the chromosphere has not yet been fully investigated, and hence a fair comparison with the spectra of these stars in the region F to K is not yet possible. M. Deslandres and Prof. Hale have photographed the chromospheric spectrum in the region more refrangible than H, but have not as yet published any account of the spectrum in the region now under discussion.

The Hottest Stars.

The conditions required by the hypothesis with regard to the stars at this stage are satisfied by such stars as ζ Cassiopeiæ and α Andromedæ.

In these stars we have—

(a) Broad lines of hydrogen, and

(b) Other absorption lines, chiefly of unknown origins, agreeing in position with some of the bright lines which appear in nebulæ.

It will be seen, then, that these considerations of the conditions of increasing temperature demanded by the hypothesis, have enabled us to determine that a long series of stellar spectra is in all probability a series in *ascending* order of temperature. All the phenomena we should expect, on the hypothesis, are met with among the photographs.

We have next to consider the phenomena connected with stars of decreasing temperature.

Stars of Decreasing Temperature.

(Stage 1.) With the failure of the supply of meteorites falling on the now condensed mass, cooling will commence, and the longest lines in the spectra of the various chemical elements should make their appearance. This condition is met with and is well evidenced by the iron lines in the spectrum of Sirius.

(Stage 2.) The conditions at this stage of cooling are satisfied by δ Cassiopeiæ, β Cassiopeiæ, α Canis Minoris. In these stars we get, in addition to fairly broad lines of hydrogen, nearly all the lines which appear in the solar spectrum, and these, it is well known, agree in the main with the arc spectra of the various chemical elements.

(Stage 3.) Such stars as Capella and Arcturus represent the conditions which are required by the hypothesis at this stage of cooling. The metallic line absorption is again at a maximum, and we find the lines of the various chemical elements similar to those seen in Stage 2 of the ascending series, but with different intensities and with different amounts of continuous absorption at the violet end of the spectrum. This difference, so far as the known lines are concerned, will be due to a different percentage composition of the absorbing mass of vapour.

Continuous absorption in the violet commences at this stage. There is [illegible] evidence of carbon in the solar spectrum, and in the spectrum of Arcturus—the only star which has yet been investigated with special reference to this point.

Hence, it seems probable that "the indications of carbon will go on increasing in intensity slowly, until a stage is reached, when, owing to the reduction of temperature of the most effective absorbing layer, the chief absorption will be that of carbon."

It is evident that all such stars will be dim, and hence their spectra have not been dealt with in this preliminary survey of the photographic spectra of the brighter stars.

General Results of the Discussion.

The general result of the above discussion so far as it goes is as follows:—Among the 171 stars already considered there are mainly two series of spectra, one representing the [illegible] of increasing temperature, while the other represents the series of decreasing temperature. The fundamental requirement of the meteoritic hypothesis is, therefore, fully justified by the discussion of the photographs.

A very interesting point in connection with the two series [illegible] the one spectrum, such as that of α Andromedæ [illegible] characteristics common to both, and we may [illegible] connect the two series together by this spectrum. In this case we should find, if we commence with the first spectrum in Series 1, say that of α Herculis, that the continuous absorption diminishes and that the breadth of the hydrogen lines regularly increases, until such a spectrum as that of α Andromedæ is reached. Then the condition would be reversed, the breadth of the hydrogen lines diminishing and the continuous absorption in the ultra-violet increasing in extent until such a star as Arcturus is reached.

It may be stated finally that the sequence now determined from the photographs follows exactly the same order as the groups originally suggested by the hypothesis, from a discussion of the eye observations. That is, it is not necessary to interchange any of the groups in order to obtain agreement with the photographic results.

J. NORMAN LOCKYER.

SCIENCE IN THE MAGAZINES.

PROFS. WEISMANN, Haeckel, and Karl Pearson will probably have something to say in reply to a paper which Dr. St. George Mivart contributes to the *Fortnightly*. The paper deals with what is described as "Denominational Science," in which dogma takes the place of facts, and persuasions are given out as if they were demonstrated truths. Dr. Weismann comes under Dr. St. George Mivart's displeasure in this regard; and a noteworthy characteristic of his is said to be "the confidence with which he propounds hypotheses which are either purely imaginary, or are only supported by an infinitesimal basis of fact, and the readiness with which he comes forward with a fresh gratuitous hypothesis, to replace others which have been refuted by newly-discovered truths." Prof. Haeckel is taken to task for the opinions expressed in his book on "Monism," lately translated into English. The bearing of Dr. St. George Mivart towards the book is indicated by the remark which opens the attack upon some of the points in it. We read: "It is difficult to say whether this small volume is more remarkable for the self-conceit and empty dogmatism, or for the ignorance it displays—ignorance concerning the most fundamental questions of which its treats." To assess these remarks at their proper value, it is necessary to read the article containing them, and the work to which they refer. Prof. Karl Pearson completes the trio upon whose views Dr. St. George Mivart outpours the vials of his wrath. His "Grammar of Science," and his remarks, in the *Fortnightly*, on Lord Salisbury's Oxford address, are given as evidence that "we have in England a denominational writer only second in self-confident dogmatism to Haeckel." All the members of the trio are held up as awful examples of "an unconscious slavery of the intellect to the mere faculty of the imagination, and the consequent presentation of shallow and illogical imaginary phantasms as deep and far-reaching intellectual truths in the form of baseless dogmas of denominational science." Huxley and Karl Vogt are compared by Prof. Haeckel in the *Fortnightly*, the former being given a higher place than the latter, both as regards his philosophical reasonings, and because he showed a much deeper insight into the essence and import of scientific things. Two pages of the six, which form Prof. Haeckel's notice, are taken up with a denunciation of Prof. Virchow's antagonism to Darwinism, and the theory of descent, especially with reference to the most important deduction from the theory—the descent of man from the ape. Virchow's dissent in this matter is used as one of the sticks with which Mr. F. H. Hill belabours agnosticism, and Huxley's support of it, in the *National*, under the title, "Gaps in Agnostic Evolution."

Mr. Herbert Spencer continues his analysis of "Professional Institutions," in the *Contemporary*, the evolution of the biographer, historian, and man of letters being traced this month. "The primitive orator, poet, and musician," says Mr. Spencer, "was at the same time the primitive biographer, historian, and man of letters. The hero's deeds constituted the common subject matter; and taking this or that form, the celebration of them became, now the oration, now the song, now the recited poem, now that personal history which constitutes a biography, now that larger history which associates the doings of one with the doings of many, and now that variously-developed comment on men's doings, and the course of things which constitute literature." Thus arose the rudiments of biography, history, and literature; and many facts illustrative of this early development are cited. Fiction developed out of biography and history, and gradually a class of story-tellers became differentiated. Indeed, for a time after fiction comes into existence, it is still classed and believed as biography. In our own times, we find

writers of history and biography and literature dividing into various classes, and finally there is the tendency of men of letters to unite into corporate bodies an integration which has only become possible in recent years. In the same magazine, under the title "Heredity Once More," Dr. Weismann replies at length to an article contributed by Mr. Spencer to the magazine last October.

Mrs. Percy Frankland writes popularly on "Sunshine and Life," in *Longman's Magazine*, which also contains an account, by Mrs. A. Lang, of the Rev. John Mulso and his unpublished letters to Gilbert White, of Selborne, whose *alter ego* he was. Miss A. Lorrain Smith describes "Ants as Mushroom Growers" in an illustrated article in *Good Words*: her paper deals with the leaf-cutting and fungus-growing ants of Nicaragua. The *Sunday Magazine* has a second paper by "Eha," on Indian jungle life. *Knowledge* contains an account of Prof. Petrie's conclusions with regard to a "Newly-found Race in Egypt"; and papers on "Wind-Fertilised Flowers," by the Rev. A. S. Wilson; "Satellite Evolution," by Miss A. M. Clerke; "Photographs of Elliptical and Spiral Nebulæ" (with a plate), by Dr. J. Roberts; and "Blind Cave-Animals," by Mr. R. Lydekker. *Chambers's Journal* contains short popular articles on "Cordite and its Manufacture," and "The Prospects of our Descendants in regard to Stature," and a gossip on the Great Auk.

We have received, in addition to the periodicals named in the foregoing, *Scribner's Magazine*, and the *Humanitarian*.

ON THE ELECTROLYSIS OF GASES.[1]

IN the experiments described in this paper I have used the spectroscope to detect the decomposition of gases by the electric discharge and the movement of the ions in opposite directions along the discharge-tube.

The method consists in sending the electric discharge through a tube so arranged that the spectra close to the positive and negative electrodes can easily be compared, the presence or absence of certain ions at these electrodes can thus be ascertained. This method is capable of much wider application than the one I previously used in my experiments on the "Electrolysis of Steam" (*Proc. Roy. Soc.*, vol. lii. p. 90), the use of which is attended with very great difficulty for any substance other than steam. The earlier method has, however, the advantage of being a quantitative method—the present one is only qualitative.

In my former experiment with steam, when I worked at atmospheric pressure and varied the length of the spark, I found that when the spark-length exceeded a certain length, d_1, there was an excess of hydrogen at the negative electrode and of oxygen at the positive, equal in amount to the quantities of hydrogen and oxygen liberated from a water voltameter placed in series with the steam-tube. When the sparks were shorter than a certain length, d_2, the hydrogen appeared at the *positive*, the oxygen at the *negative* electrode, but the quantity of these gases was again equal to the quantities liberated in a water voltameter placed in series with the steam-tube.

When the spark-length was between d_1 and d_2 the effects were irregular, and there seemed to be no connection between the amounts of gases liberated in the steam-tubes and those liberated in the voltameter.

In the following experiments in which the sparks were of constant length and the pressure was altered, corresponding effects were observed. Within certain limits of pressure definite and perfectly regular evidence of the separation of the ions of the gas sparked through was obtained; and the electrode at which a given ion appeared could be reversed by altering the pressure; there was, however, a range of pressures in which the separation of the ions was either not well marked or was irregular in character.

I shall begin by describing a very simple method of showing the separation of the ions produced by the discharge of electricity through a compound gas such as hydrochloric acid gas, which is applicable when the discharges through the constituent gases of the compound are of distinct and different colours; this is eminently the case with the hydrochloric acid gas, as the discharge through hydrogen in a capillary tube is red, through chlorine green.

Take a capillary tube of very fine bore, the finer the better (the tube I used was thermometer tubing of the finest bore I could procure), and insert platinum wires for electrodes in two small bulbs blown on the ends of the tube; then fill the tube with HCl gas, allowing it to run through the tube for a considerable time so as to get rid of any extraneous gas, and exhaust the tube so that the gas in it is at a very low pressure. Then when the discharge from a large induction coil passes through the tube, the following phenomena are observed. When first the discharge passes through the tube the colour is uniform throughout and of a greenish-grey; after the discharge has been passing for a little time the end of the tube next the cathode gets distinctly red, whilst that next the anode gets green; this difference in the colour at the ends of the tube goes on increasing until the tube presents a most striking appearance, the part near the cathode being bright red, while that near the anode is a bright green. The difference in colour attains a maximum value, and if the discharge is allowed to run for several hours the contrast between the two ends disappears to a very great extent; the discharge throughout the whole of the tube being pinkish and apparently passing mainly through hydrogen. This is doubtless due to the diffusion through the tube of the hydrogen which in the earlier stages of the discharge had accumulated about the cathode; one advantage of using very narrow tubes is that with them this diffusion is slow. When the tube is in this condition the colour of the discharge sometimes changes suddenly, and for a second or two is green instead of pink, showing that though in the main the discharge passes through hydrogen, it occasionally leaves the hydrogen and passes through the chlorine. This transference of the discharge from one constituent to another of a mixture of gases is not infrequently observed when the gases are mixed in certain proportions.

Some of these capillary tubes showed after the discharge had been passing through them for some time a peculiar patchy appearance, some portions of the tube being a much brighter red than the others, while other portions were green. In some tubes this occurred to such an extent that the discharge showed an irregularly striated appearance. This effect is due, I believe, to gases or moisture condensed on the walls of the capillary tube, and in some cases to irregularities in the chemical composition of the glass. I found that it did not occur if the tube before being used was heated for some time along its whole length to as high a temperature as it would stand without collapsing; this heating would tend to cleanse the walls of the tube. That differences in the quality of the gas also conspire to produce these patches is shown, I think, by the following phenomenon. A capillary tube of fine bore containing mercury vapour and a little water vapour developed a well-marked red patch; the tube was then heated for some inches in the neighbourhood of the patch. In general heating the tube makes the discharge yellow from the sodium vapour given off from the glass; in this case, however, the whole of the heated portion, with the exception of the patch, turned yellow; the patch itself withstood the heating and continued to show the bright colour characteristic of hydrogen.

Electrolytic Transport of one Gas through another. A tube of the shape shown in Fig. 1 was made of the finest bore thermometer-tubing; the extremities, C and D, of the tube in which

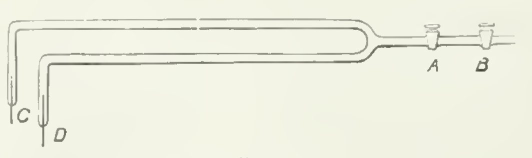


FIG. 1.

the electrodes were fused were bent down so as to be parallel to each other, and so near together that a slight motion of the tube suffices to bring either of the extremities in front of the slit of the spectroscope. The tube was mounted on a board moved by a lever; by moving this the observer at the spectroscope could readily bring the spectrum of either the positive or negative electrode into the field of view. A side tube, A B, was fused to the middle of the main tube and was provided with two taps; in the space between these taps a small quantity of any gas which it was desired to introduce into the main tube could be imprisoned, and could, by opening the tap A, be introduced into the discharge tube. The experiment consists in filling the main tube with a gas at a low pressure, observing the spectra at the

[1] Paper read at the Royal Society, by J. J. Thomson, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge.

two electrodes, then introducing by the side tube a very small quantity of gas into the main tube, and again observing the spectra at the two electrodes.

A tube was filled with hydrogen and showed no trace of the chlorine spectra; a very small quantity of chlorine was then let in through the side tube (in performing this experiment it is necessary to be careful that only a very small quantity of chlorine is introduced). After the discharge had been running through the tube for a short time, the chlorine spectrum was found to be bright at the positive electrode, though no trace of it could be detected at the negative. When the discharge was kept on for some time, the chlorine spectrum, though still visible at the positive electrode, got fainter; it did not appear at all at the negative. If a considerable quantity of chlorine was introduced through the side tube, the chlorine spectrum was visible at both electrodes, though it was brighter at the positive than at the negative.

When the induction coil was reversed, so that what was before the positive electrode became the negative, the first effect observed was that the chlorine spectrum flashed out with great brilliancy at the old positive electrode, and was much brighter than at any previous period. This, however, only lasted for a second or two; the chlorine spectrum rapidly faded away and for a time was not visible at either electrode. Soon, however, the chlorine spectrum appeared at the new positive electrode, having thus been transferred from one end of the tube to the other.

On again reversing the coil the same phenomenon was repeated. There is apparently no limit to the number of times this effect may be obtained; at any rate, I have driven the chlorine from one end of a tube to the other 14 times in succession by reversing the coil. The chlorine is always driven to the positive electrode, showing that the chlorine ion carries a charge of negative electricity. The same effect was obtained when a little vapour of bromine was introduced into the tube instead of chlorine. When, however, the capillary tube was filled with chlorine instead of hydrogen, and a little vapour of bromine let into the tube, the bromine went to the *negative electrode* instead of to the positive, as it did when introduced into the hydrogen tube. These experiments suggest that the two gases in the tube combine, and that the compound gas so formed is split up into ions which travel along the tube; that bromine when in combination with hydrogen is the negative ion, and therefore travels to the positive electrode; when, however, it is in combination with chlorine the bromine is the positive ion and travels to the negative electrode.

Another experiment tried was to let a little vapour of sodium into the middle of a capillary tube filled with air at a low pressure. To prevent the sodium vapour condensing on the walls of the tube, the whole tube was placed on a sand bath and the temperature raised so high that no condensation took place. After the discharge had run through the tube for about two hours the sand was removed from the tube, and the movement of the sodium vapour to the *negative* electrode was very apparent even without using a spectroscope, as there was a great patch of yellow light near the negative electrode and none in any other part of the tube.

Another experiment was to introduce a small quantity of hydrogen into a tube filled with air at a low pressure: the hydrogen made its way to the *negative* electrode. This experiment is a somewhat troublesome one, as it is exceedingly difficult to get these very fine capillary tubes so dry that the spectrum of the discharge does not show the hydrogen lines even before the hydrogen is introduced into the middle of the tube; indeed, I never succeeded in getting rid of the hydrogen lines at the very lowest pressures. By heating the tube and allowing dry air to pass through it for a long time, however, I got the tube so dry that it did not show the hydrogen lines at a pressure quite low enough to allow the discharge to pass freely through it. When the tube was in this state and hydrogen was let into the middle of the tube, the hydrogen spectrum appeared at the negative electrode, but not at the positive.

The appearance of hydrogen at the negative electrode when mixtures of [illegible] with other gases has been described by Mr. Baly in a very interesting paper in the *Philosophical Magazine*, [illegible] p. 200.

The preceding experiments suggest, I think, that this separation of two gases, A and B, by the discharge is due to the decomposition by the discharge of a chemical compound formed of A and B, in which the A atoms have a charge of electricity of one sign, the B atoms a charge of electricity of the opposite sign; these charged atoms under the influence of the electromotive force in the tube travel in opposite directions. Further, it follows from the experiment with the bromine vapour in an atmosphere of chlorine that the sign of the electrical charge on an atom of the same substance is not invariable, but depends on the substance with which this atom is in combination. We shall find numerous other instances of this change in the sign of the charge on an atom in experiments described in a later part of this paper.

Polarisation of the Electrodes.—This in the electrolysis of liquids is due to the accumulation at the electrodes of ions which have ceased to act as carriers of electricity. We have, I think, distinct evidence of a similar accumulation in the electrolysis of gases. For, as has been already described, after the discharge has been running for some time in one direction, giving the spectrum of some gas at one of the terminals, the spectrum of the gas at that terminal is momentarily brightened to a very great extent by suddenly reversing the direction of the discharge. After the current has been flowing for some time in one direction through, say, Cl in an atmosphere of H, the spectrum of the chlorine, though still visible at the positive electrode, gets faint, the chlorine apparently to a great extent ceasing to carry the discharge; when, however, the current is reversed, the atoms of chlorine can move freely, as they are not obstructed by the electrode, so that immediately after the reversal of the current there is probably more of the discharge carried by the chlorine than at any other time, and the chlorine spectrum is consequently brightest.

Discharge through a Compound Gas.—The separation of the ions by the discharge can be readily observed in a tube of the kind shown in Fig. 2.

It differs from an ordinary discharge tube merely in having a flat metal plate, A B, fastened across the tube. When the discharge passes through the tube, one side of the plate acts as a

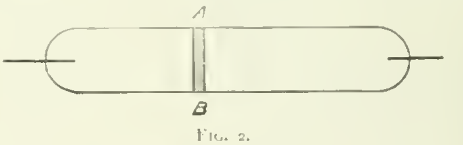


FIG. 2.

positive, the other as a negative, electrode. The tube is mounted on a stand, which the observer at the spectroscope can move by means of a lever so as to bring one side or other of the plate opposite the slit of the spectroscope; a very slight movement of the lever is sufficient to do this, so that the spectra at the two sides of the plate can readily be compared. I found that the results were more satisfactory when the current was kept flowing through the tube in one direction and the tube moved so as to bring the spectra at the two electrodes into the field of view than when the tube was kept fixed in one position and the current reversed. The latter method, however, suffices to show the separation of the ions in many cases, and it has the advantage of not requiring a plate across the tube; all that is necessary is to use for one of the terminals a disc whose plane is parallel to the slit of the spectroscope.

If the plate A B is thin, it is necessary to fuse it into the glass tube all the way round; otherwise, when the pressure is low, the discharge, instead of crossing the plate, goes through any little crevices there may be between the plate and the tube. The easiest way of making the tube is to use a plate about 0·5 cm. thick, cut from an aluminium cylinder which tightly fits the tube; with a plate of this thickness the narrow spaces between the tube and the plate are so long that the discharge goes through the plate rather than through the crevices.

The tube was filled with the gas to be observed and the spectra at the two sides of the plate compared. These spectra were in many cases found to differ in a very remarkable way; it was, however, only in exceptional cases that a line which was bright at one side of the plate was absolutely invisible on the other. The method used was to take two sets of lines, say A and B, as close together in the spectrum as possible, and compare the brightness of these sets of lines on the two sides of the plate; if it was found that the A lines were brighter on the positive side of the plate than on the negative, while on the other hand the B lines were brighter on the negative side of the plate than on the positive, then it was inferred that electrolytic

separation had occurred, and that the substance giving the A lines was in excess on the positive side of the plate, that giving the B lines on the negative. It is not safe to draw any conclusions from the variations in intensity of one line or one group of lines on the two sides of the plate, as the total quantity of light coming from the neighbourhood of the cathode often differs considerably from that coming from the anode. When, however, we get an increase in the brilliancy of one set of lines accompanied by a diminution in the brightness of another set, when we move across the plate we eliminate this source of error. The differences in the spectra at the two sides of the plate are most easily observed at pressures where there is not any very great difference between the luminosity of the cathode and the anode. As was mentioned at the beginning of the paper, there is a range of pressure within which the effects are irregular, and no decided differences are observed between the spectra at the two sides of the plate. It is desirable in these experiments to keep the tube on to the pipe as long as the experiment lasts, for the discharge always decomposes the compound gas, and unless the products of decomposition are continually pumped off and replaced by fresh supplies of the compound gas, the spectra of the discharge keep changing. With organic compounds this is especially necessary, as the character of the spectrum often changes entirely very shortly after the commencement of the discharge unless fresh gas is continually introduced.

In the following experiments the current was produced by a large induction coil with a mercury slow break.

When the tube was filled with hydrochloric acid gas at a low pressure, the separation of the hydrogen and chlorine was seen very distinctly, the hydrogen line being much brighter on the side of the plate which acted as the cathode (which we shall call the negative side of the plate) than on the positive side, while the chlorine, on the other hand, was brighter on the positive than on the negative side of the plate.

When the tube was filled with ammonia gas, the hydrogen lines were bright on the negative side of the plate, but were absent from the positive side, while on the positive side of the plate there was the positive pole spectrum of nitrogen, and on the negative side of the plate the negative pole spectrum of nitrogen and the hydrogen spectrum.

Sulphur Monochloride.—When the tube was filled with the vapour of this substance at a low pressure, the chlorine lines were brighter on the *negative* side of the plate than at the positive, while the sulphur lines were brighter at the positive side than at the negative. Thus the chlorine in this substance behaves in the opposite way to the chlorine in HCl; in the latter compound the chlorine iron has a charge of negative electricity, while in the sulphur monochloride it has a charge of positive electricity.

Influence of the Chemical Constitution of a Compound on the Sign of the Charge of Electricity on one of its Constituent Atoms.—In many organic compounds an atom of the electro-positive element hydrogen can be replaced by an atom of the electro-negative element chlorine without altering the type of the compound. Thus, for example, we can replace the four hydrogen atoms in CH_4 by chlorine atoms, getting successively the compound CH_3Cl, CH_2Cl_2, $CHCl_3$, and CCl_4. It seemed of interest to investigate what was the sign of the change of electricity on the chlorine atom in these compounds. The point is of some historical interest, as the possibility of substituting an electro-negative element in a compound for an electro-positive one was one of the chief objections assigned against the electro-chemical theory of Berzelius.

When the vapour of chloroform, $CHCl_3$, was placed in the tube, it was found that both the hydrogen and the chlorine lines were bright on the negative side of the plate, while they were absent from the positive side, and that any increase in the brightness of the hydrogen lines was accompanied by an increase in the brightness of those due to chlorine. The spectrum on the positive side of the plate was that called the carbonic oxide spectrum; when first the discharge passed through the tube, the spectrum on the positive side was the so-called candle spectrum, but this very rapidly changed to the carbonic oxide spectrum. The appearance of the hydrogen and chlorine spectra at the same side of the plate was also observed in methylene chloride and in ethylene chloride. Even when all the hydrogen in CH_4 was replaced by chlorine, as in carbon tetrachloride, CCl_4, the chlorine spectra still clung to the *negative* side of the plate. To test the point still further, I tried the analogous compound silicon tetrachloride, inserting a small jar in the circuit to brighten the spectrum. The chlorine spectrum was again brightest at the *negative* side of the plate, while the silicon spectrum was brightest at the positive. This is a very favourable case for the application of this method, as there are two silicon lines (wave-lengths 5058, 5043) quite close to two chlorine ones (wave-lengths 5102, 5078), so that their relative brightness can easily be compared. The experiment with the silicon tetrachloride is more conclusive than those with the carbon compounds, as with the latter the spectrum on the positive side of the plate is a band spectrum, and since the potential gradient when the discharge is passing is very much steeper on the negative side of the plate than on the positive, the effects observed might be supposed to be due to the circumstances on the negative side being better adapted for the production of line spectra than those on the positive. This explanation is not, however, applicable to the case of silicon tetrachloride, where the spectra on both sides of the plate are line spectra.

From these experiments it would appear that the chlorine atoms in the chlorine derivatives of methane are charged with electricity of the same sign as the hydrogen atoms they displace.

When we can determine the signs of the electrical charges carried by the atoms in a molecule of a compound, we can ascertain whether any given chemical reaction does or does not imply interchange between the electric charges on the atoms taking part in the reaction. Thus take the reaction

$$CH_4 + Cl_2 = CH_3Cl + HCl.$$

If we represent the sign of the charge of electricity carried by an atom by + or − placed below the symbol representing that atom, we may write the last reaction as

$$\underset{-\,+}{CH_4} + \underset{+\,-}{ClCl} = \underset{-\,+\,+}{CH_3Cl} + \underset{+\,-}{HCl},$$

so that this reaction could be produced by a rearrangement of the atoms without any alterations of their electrical charges.

If, however, we take the reaction—

$$\underset{+\,-}{HH} + \underset{+\,-}{ClCl} = 2\underset{+\,-}{HCl},$$

we see that in addition to a rearrangement of the atoms there must in this case be an interchange of electric charges between the atoms; for before combination half the hydrogen atoms had a negative charge, and half the chlorine atoms a positive one, whereas after combination no hydrogen atom has a negative charge, and no chlorine atom a positive one. We may thus distinguish between two classes of chemical reactions, (1) those which do not necessarily require any interchange of the electrical charges carried by the atom, and (2) those which do. It might, perhaps, repay investigation to see whether the occurrence of chemical change is affected by the presence of a third substance in the same way in these classes of chemical combination.

Another point to be considered is the effect of this difference between the chemical actions on the amount of heat developed during chemical combination. When hydrogen and chlorine combine the heat produced may be regarded as the joint effect of three processes:—

(1) The splitting up of the molecules $(\underset{+\,-}{H\ H})$ and $(\underset{+\,-}{Cl\ Cl})$ into the atoms $\underset{+}{H}$, $\underset{-}{H}$, $\underset{+}{Cl}$, $\underset{-}{Cl}$.

(2) A transference of electricity by which the negative charge on one atom of hydrogen is replaced by an equal positive charge, while the positive charge on an atom of chlorine is replaced by an equal negative charge.

(3) The combination of the positively electrified hydrogen atoms with the negatively electrified chlorine ones to form hydrochloric acid.

In that class of chemical action where the atoms retain their charge (2) is absent, so that if the change in energy occurring in the process (2) were considerable compared with the changes occurring in processes (1) and (3), the thermal effects of the two types of chemical combination ought to differ considerably. If the changes in energy occurring in the process (2) had a great preponderance over those occurring in (1) and (3), the thermal effects produced by the combination of two elements ought to follow very simple laws. For if 2 {H} is the excess of the energy of an atom of hydrogen charged with the negative electron over the energy of the atom charged with the positive electron, 2 {Cl} the excess of the energy of an atom of chlorine charged with the positive electron over the energy of the atom charged with the negative electron, then if we could neglect the energy changes in (1) and (3) compared with those in (2), the

mechanical equivalent of the heat developed when a molecule of hydrogen combines with one of chlorine to form two molecules of hydrochloric acid would be equal to 2{H} + 2{Cl}. Thus we see that if the energy changes in (2) preponderated largely over those in (1) and (3), the heat produced when an element A combined with another element B to form the compound AB, could be expressed as the sum of two numbers {A}, and {B}, where {A} depends solely on the element A, {B} solely on the element B. In some cases of chemical combination between dilute solutions there seems evidence that the heat produced can be expressed in this way (see Lothar Meyer, "The Evolution of the Doctrine of Affinity," *Phil. Mag.*, vol. xxiii. p. 504), but when we attempt to apply the same law to combination between gases, it seems utterly to break down; indicating that in such cases the greater part of the changes in energy occur in the splitting up of one set of molecules and the subsequent formation of others. This view seems to be supported by the phenomena attending the discharge of electricity through rarefied gases, for the smallest difference of potential which can send a discharge through an electrified gas (which we have reason to believe involves the splitting up of molecules into atoms), is very many times the electromotive force required to liberate the ions from an electrolyte, though the latter progress requires changes in the electrical charges on the ions. These reasons seem to indicate that we can hardly expect to get any clear indication of the charges carried by the atoms in gaseous compounds from the study of the thermal changes which occur when gases enter into chemical combination.

Vapours of Organic Compounds.—These show very interesting differences between the spectra on the two sides of the plate when the discharge passes through them. Thus when the discharge first passes through the vapour of ethyl alcohol, C_2H_6O, the spectrum on the positive side of the plate is the candle spectrum, that on the negative side the carbonic oxide spectrum. For some little time after the discharge commenced I could not detect any hydrogen lines on either side of the plate; after a time, however, they appeared on the negative side but not on the positive. If the discharge was kept running for some time without letting a fresh supply of alcohol into the tube the "candle spectrum" on the positive side of the plate was replaced by the CO spectrum, which now occurred on both sides of the plate accompanied on the negative side by the hydrogen spectrum. This is the appearance presented by all the compounds of carbon, oxygen, and hydrogen which I examined, when the spark had been passing through them for a considerable time, and it is what would occur if the vapour were decomposed by the spark into carbonic acid, water, and hydrogen.

The appearance of the candle spectrum on the positive side of the plate with the CO on the negative was observed in many other cases. Thus on sparking through a tube filled with CO, I could not detect any difference between the spectra on the two sides of the plate, but when a little hydrogen was let into the tube the "candle spectrum" appeared on the positive side of the plate, the carbonic oxide spectrum on the negative. The same effect was observed in a tube filled with cyanogen mixed with a little hydrogen. When the tube was filled with the vapour of methyl alcohol, CH_3OH, the candle spectrum was on the positive side of the plate, the carbonic oxide and hydrogen spectra on the negative; with this vapour, unlike that of ethyl alcohol, I could not detect any stage when the hydrogen spectrum was absent.

The first explanation which occurs to one of this phenomenon is that it is owing to the potential gradient at the negative side of the plate being steeper than that on the positive, so that we may imagine we have a fierce spark on the negative side, a mild one on the positive, and that the fierce spark gives the CO spectrum, the mild one the candle spectrum. There are, however, some phenomena which seem inconsistent with this explanation: in the first place, if the current is reversed after running in one direction, traces of the former spectra linger for some time at the sides of the plates, and, secondly, if the difference is due to the greater decomposition at the negative side of the plate, how is it that in the case of the vapour of ethyl alcohol the hydrogen spectrum is not seen, at the commencement of the discharge, on the negative side of the plate, and only appears after the discharge has passed through for some time, when hydrogen has probably been set free by the decomposition of the vapour by the discharge. If the absence of the candle spectrum from the negative side of the plate is due to the spark being so intense that the hydro-carbon which is supposed to be the origin of this spectrum cannot exist, then we ought to see the spectra of the substances which result from the decomposition of the hydro-carbon, *i.e.* we ought to see the hydrogen spectrum at the negative electrode. The view which seems most in accordance with the results of observations on the discharge through these vapours is that the "candle spectrum" is the spectrum of carbon when the atom is charged with negative electricity, or of some compound of carbon in which its atom is negatively charged, while the "carbonic oxide" spectrum is the spectrum of carbon when the atom is charged with positive electricity, or of some compound in which the carbon atom is positively charged.

Discharge through an Elementary Gas.—It has long been known that when the discharge passes through some elementary gases, the spectra at the two electrodes are different. This was first shown to be the case for nitrogen, then Dr. Schuster showed that the same thing occurred with oxygen, and recently Mr. Crookes has shown that it is also true in the case of argon. I have observed a very striking change in the relative brilliancy of the red and green hydrogen lines at the two electrodes. When the tube with the plate across it was filled with hydrogen at a low pressure, then on the positive side of the plate the red line tends to be brighter than the green, while on the negative side the green line tends to be brighter than the red; in some tubes this was so marked that on the positive side of the plate the red line was bright, and the green invisible, while on the negative side of the plate the green line was bright, and the red invisible. The spectroscope I was using weakened the red rays much more than the green, so that I cannot be sure that the red rays were really altogether obliterated on the negative side of the plate; the above experiment is, however, sufficient to show that on the positive side of the plate the red rays are more easily excited than the green, while on the negative side the green line is more easily excited than the red. On the negative side of the plate we have an excess of positively charged hydrogen atoms, while on the positive side of the plate there is an excess of negatively charged hydrogen atoms, and I am inclined to attribute the difference in the spectra partly at any rate to the difference in properties between a positively and a negatively charged hydrogen atom. The reason I do not attribute it wholly to the difference in the potential gradient on the two sides of the plate is that the effect is not reversed immediately, but only gradually on reversing the coil, the former spectra clinging for some time to the sides of the plate.

Chlorine.—I have made a great many experiments to see if there is any difference between the spectra given by chlorine on the two sides of the plate, but with negative results. Chlorine seems a gas in which we might expect to find this effect, for as Dr. Schuster, in his Report on Spectrum Analysis, says, the behaviour of its spectrum indicates that we have several spectra superposed. I have not, however, been able to affect a separation of its spectra, the differences I observed between the spectra on the two sides of the plate were irregular, and due, I think, to impurities producing effects like those observed when the discharge passes through a compound gas. However, as has been mentioned before, there is even in the case of gases where distinct evidence of separation can be obtained, a region of pressure within which the effects are irregular, and I ascribe my failure to observe separation in the case of chlorine to my having failed to get the relation between the intensity of the discharge and the pressure so adjusted as to get outside this irregular region. The cases, however, in which distinct differences between the spectra of a single gas occur at the two electrodes, seem to indicate that the spectrum given by an element is influenced by the sign of the electrical charge carried by its atoms.

I have made some experiments to determine whether there was any separation produced in a mixture of equal volumes of hydrogen and chlorine kept in the dark, when a considerable difference of potential though not sufficient to produce discharge was maintained between the two electrodes. The parts of the tube adjacent to the two electrodes could be shut off from each other by a tap, and the amount of chlorine in the two sides was determined by absorbing it by caustic potash. The mixture was at atmospheric pressure, and the electrodes were maintained at a potential difference of about 1200 volts by connecting them to a large battery of small storage cells. The potential difference between the terminals was maintained for about sixteen hours on three separate occasions, but on analysing the vessels surrounding the two electrodes, the amount of chlorine in the vessel adjacent

to the negative electrode did not differ from that in the vessel adjacent to the positive electrode by more than 1 per cent., and this could be accounted for by errors of experiments, as test experiments, in which the mixture had not been exposed to the electric field, gave differences comparable with these. We should conclude from the preceding experiments that the molecules of a gas are not acted on by any appreciable translational force tending to move them from one place to another, when they are near to a body charged with electricity. To test this point further, two large terminals were placed in bulbs which were connected by a horizontal capillary tube, in which a drop of sulphuric acid was placed; a difference in the pressure of the gas would cause the sulphuric acid to move, and the arrangement acts as a very delicate pressure gauge. The bulbs and tube were filled with chlorine at atmospheric pressure. The terminals were then connected to the electrodes of a battery giving a potential difference of 1200 volts, but not the slightest movement of the drop of acid could be detected.

I wish to acknowledge the help I have received in making the preceding experiment from my assistant, Mr. E. Everett.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

IN the ninth session of Edinburgh Summer Meeting, which was opened by Lord Reay on August 5, and has just concluded, natural science was represented by Elisée Reclus ("On the Evolution of Cities"), Dr. W. W. J. Nicol ("On Every-day Chemistry"), Mr. J. G. Goodchild ("On the Geology of Edinburgh"), Mr. A. J. Herbertson ("On the Geography of the District"), Dr. Louis Irvine ("On the Nervous System"), Mr. J. Arthur Thomson ("On the Biology of the Seasons"), Mr. R. Turnbull ("On Applied Botany"); Prof. Lloyd Morgan lectured "On Evolution Ethics," Prof. Haddon "On the Savage Mind," and Prof. Geddes "On Life and Thought."

MR. JOSEPH BISSETT, who was for two years lecturer at the Agricultural College, Aspatria, has been appointed Agricultural Lecturer to the County of Ayr.

MR. F. G. JONES goes to the Huddersfield Technical School as Lecturer in Physics, Applied Mechanics and Steam, and Mr. J. Brierley is to fill the post of Assistant Master in Chemistry and Physics at the same school.

THE Calendars for the Session 1895–96 of the University College, Bristol, and the Glasgow and West of Scotland Technical College have just been published, and may be obtained, respectively, of Arrowsmith, Bristol, and Anderson, Glasgow.

THE *Educational Times* understands that Mr. Arthur Milman will retire early in 1896 from the Registrarship of London University, under the Civil Service regulation as to age.

SCIENTIFIC SERIALS.

American Meteorological Journal, August.—The principal articles are:—Relation of clouds to rainfall, by H. Helm Clayton. A special study of cloud-forms before and after rain was made at the Blue Hill Observatory, and it was found that the most frequent succession of clouds preceding rain was cirrus, cirro-stratus, alto-stratus, and nimbus; the first which appeared in advance of the rain being usually cirrus. Rain was observed to fall from four classes of clouds: (1) a high cloud sheet (alto-nimbus); (2) a low, ragged cloud sheet (nimbus); (3) long, low rolls of cloud, giving light intermittent showers; and (4) a towering cloud of the cumulus type (cumulo-nimbus). Following rain, the most frequent clouds were strato-cumulus, in long, low rolls, while above there was most frequently cirrus or cirro-stratus. The result of the investigation showed that cloud-forms cannot, in general, be used in predicting rain for more than twenty-four hours in advance, but that, for a few hours in advance, the existence of certain clouds frequently furnish most trustworthy indications of coming rain.—The meteorograph for the Harvard Observatory on El Misti, Peru, by S. P. Fergusson. It has been found impossible to maintain observers at this elevated station (19,300 feet), and during the rainy season, which lasts three or four months, no ascent can be made. A meteorograph, on the principle of Richard's well known instruments, has been constructed at the request of Prof. Pickering, which will work for four months, and will be installed on the summit of the mountain this summer. The record drum revolves once during three days, giving to the paper a speed of three inches in a day, and the paper used for the records is rolled upon a removable reel under the record drum. An illustration of the apparatus is given in the journal.

Bulletins de la Société d'Anthropologie de Paris, 1895, fas. 1. Discussion of the *Pithecanthropus erectus* as the presumed precursor of man, by L. Manouvrier.—This paper contains a critical examination of the remains recently discovered by M. Dubois in Java, upon which an article by Prof. Cunningham has already appeared in NATURE. The dolmen of Ethiau, by M. Lionel Bonnemère. After a careful examination of the marks upon the dolmen, the author has come to the conclusion that they are not due to atmospheric action but to the hand of man.—Lower terrace of Villefranche-sur-Saône, by M. G. de Mortillet. Many worked flints have been found associated with teeth of *Elephas primigenius* and *Rhinoceros tichorhinus*. At Chelles, the molars *E. antiquus* are common and characteristic, and the teeth of rhinoceros, which are very abundant, appear to belong to a small variety of *R. Merkii*.—The engraved stones of New Caledonia, by M. L. Bonnemère. The author exhibited, in the name of M. Glaumont, collector at Coron, a most interesting series of drawings made by him representing certain remarkable objects from the colony. Many large stones are covered with designs that were evidently executed before the European occupation of the island.

L'Anthropologie, 1895, No. 3. General considerations on the Yellow Races, by Dr. E. T. Hamy. The opening lecture of the course of Anthropology at the Museum.—Infantilism, feminism, and antique hermaphrodites, by Henry Meige. Several cases in illustration of this paper have been drawn from the patients of Salpêtrière.—Studies in prehistoric ethnography, by Ed. Piette. Many archæologists have imagined that between the quaternary period and the modern era there was a long interval of desolation, during which the lands of Western Europe were devoid of inhabitants, and the record of human life was interrupted. They named it the *hiatus*. The author traces the history of the harpoon during this period, and shows that no such *hiatus* occurred. Sculpture in Europe before the Greek-Roman Influences, by M. Salomon Reinach. In this section of M. Reinach's valuable monograph, the subject of gesture is treated, and numerous illustrations of bronze figures are given in illustration of the author's argument.

Bollettino della Società Sismologica Italiana, i., 1895, No. 4.—Vesuvian notices (1894), by G. Mercalli.—On the propagation in Italy of the Lubiana earthquake of April 14, 1895, by M. Baratta. A brief account, with a map showing the course of the isoseismal lines in Italy.—Notices of Italian earthquakes (April 1895). A valuable list of records, principally of the earthquake which forms the subject of the preceding paper.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 20.—"On the Refractive Index of Water at Temperatures between 0° and 10°." By Sir John Conroy, Bart., F.R.S.

In 1856, Jamin (*Comptes rendus*, vol. xliii. p. 1191) published an account of observations he made on the refractive index of water at temperatures between 30 and 0°. He used an interference method, and found that as the water cooled the index increased; similar results have been obtained by other observers, but although it appears to be proved that the refractive index of water increases with the decrease of temperature until the freezing point is reached, few determinations of the values of refractive indices of water near its point of maximum density have been published.

The method employed was the ordinary one, the determination of the angle of minimum deviation for a ray of definite wave-length passing through a hollow glass prism containing water at a known temperature.

The prism was filled with distilled water which had been recently boiled and allowed to cool under reduced pressure, and

was surrounded by a water-jacket, through which a stream of brine, cooled by a freezing mixture, could be passed.

The determinations were made exclusively with sodium light. In the first column of the table the values of the refractive indices, relative to air, for each degree are given to five places; in the second the values as found by Walter, and in the third and fourth those for sodium light, given by Gladstone and Dale, and Rühlmann.

Refractive Indices of Water.

t.	C.	W.	t.	G and D.	t.	R.
°			°		°	
0	1·33397	1·33401	0·0	1·33374	0·0	1·33375
1	1·33397	1·33400	4·0	1·33367		1·33380
2	1·33396	1·33398	6·5	1·33356	1·5	1·33375
3	1·33394	1·33396	9·0	1·33342	4·0	1·33372
4	1·33392	1·33393	—	—	5·0	1·33371
5	1·33389	1·33390	—	—	5·8	1·33368
6	1·33385	1·33387	—	—	9·9	1·33355
7	1·33382	1·33383	—	—	10·0	1·33353
8	1·33378	1·33379	—	—	—	
9	1·33375	1·33374	—	—	—	—

The values show that the refractive index of water, as was first announced by Jamin, increases continuously up to the freezing point, the rate of increase, however, seems to change about 4°, the temperature of maximum density, as was pointed out by Gladstone and Dale, and that no formula representing the variation of the refractive index of water with the temperature, as a function of the density only, can be a complete expression of the facts of the case.

PARIS.

Academy of Sciences, August 26.—M. Fizeau in the chair. Truffles (*Terfas*) from Cyprus (*Terfezia Claveryi*), Smyrna, and La Calle (*Terfezia leonis*), by M. Ad. Chatin.—Observations of Swift's comet (August 20, 1895), made at Lyons Observatory by means of the coudé equatorial (0·32 m.), by M. G. Le Cadet. The remark is recorded that this comet appears as a very diffuse and feeble nebulosity almost equally spread in every direction. By oblique vision a nearly central feeble condensation can be distinguished.—Observations of the planet Phaeo (—), made at Marseilles Observatory by means of the 0·26 m. equatorial, by M. Borrelly.—On regular pencils and the *équitentes* of the *n*th order, by M. Paul Serret.—Heat of solution and of formation of sodium and potassium cyanurates, by M. Paul Lemoult. A detailed thermochemical study. The difficulty of forming the trimetallic salts is emphasised, and it is shown that the sodium and potassium series do not differ essentially. Water does not appear to decompose these salts.—On *apiculé* fermentation, and on the influence of aeration in elliptic fermentation at a high temperature, by MM. M. Rietsch and M. Herselin. Alcohol formed from *apiculé* yeasts requires more sugar for its production than that produced by the agency of elliptic yeasts. Cooling the must to just below 30 and aeration both favour the economical production of alcohol.—On aluminium utensils, by M. Balland. These utensils in ordinary camp use stand wear fairly well, and are not much attacked by foods during the short time they are in contact therewith. They should not be soldered or brought into contact with other metals. In the process of manufacture, treatment with soda should be avoided; the fine matt surface produced is more easily attacked than a polished surface.—On the rôle of the liver in the anticoagulant action of peptone, by MM. E. Gley and V. Pachon. The results of the authors' experiments appear to show that peptone does not itself exert any anticoagulant effect, but that it stimulates the production by the liver of some substance possessing anticoagulant properties.—The ellipsoidal stethoscope, by M. Ch. V. Zenger. A solid homogeneous wooden ellipsoid of revolution is cut by two planes perpendicular to the principal axis, and passing through the two foci. One flat end being placed as usual on the body surface, the ear perceives at the other focus the sounds of organ movements with remarkable intensity, and free from parasitic sounds formed in the air space of the ordinary instrument.—The electrodynamic system of the world, by M. Ch. V. Zenger.

GOTTINGEN.

Royal Society of Sciences.—The *Nachrichten*, part 2 for 1895, contains the following memoirs of scientific interest:—

May 25.—O. Holder: On groups whose order is free from squares.

June 15—A. Hurwitz: A fundamental theorem in the arithmetical theory of algebraic magnitudes. A. von Koenen: On the selection of points near Gottingen at which differences in the intensity of gravity may be expected in trial pendulum experiments. W. Schur: On the results of the first pendulum trials. W. Voigt: *In memoriam* F. E. Neumann.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Annual Report of the Department of Mines and Agriculture, N.S.W., for the Year 1894 (Sydney).—Diseases of Personality: Prof. Ribot, translated (Chicago, Open Court Publishing Company).—Analytical Key to the Natural Orders of Flowering Plants: F. Thonner (Sonnenschein).—Studies in the Evolutionary Psychology of Feeling: H. M. Stanley (Sonnenschein).—University College, Bristol, Calendar for the Session 1895-1896 (Bristol, Arrowsmith).—Origin of Plant Structures: Rev. G. Henslow (K. Paul).—Bourne's Handy Assurance Manual, 1895: W. Schooling (London).

PAMPHLETS.—Stenopaic or Pin-hole Photography: F. W. Mills and A. C. Panton (Dawbarn).—University Correspondence College, Lon. Inter. Science and Prel. Sci. Guide, No. vii. (Red Lion Square). Ditto Inter. Arts Guide, No. x. (Red Lion Square).

SERIALS.—Indian Museum Notes, Vol. 3, Nos. 4 and 5 (Calcutta).—Chambers's Journal, September (Chambers).—Contemporary Review, September (Isbister).—Good Words, September (Isbister).—Sunday Magazine, September (Isbister).—Humanitarian, September (Hutchinson).—National Review, September (Arnold).—Scribner's Magazine, September (Low).—Fortnightly Review, September (Chapman and Hall).—Clinical Sketches, No. 8, Vol. 2 (Smith, Elder).—Notes from the Leyden Museum, July (Leyden, Brill).—Journal of the Royal Microscopical Society, August (Williams and Norgate).—A Monograph of the Land and Freshwater Mollusca: J. W. Taylor, Part 2 (Leeds, Taylor).—Bulletin de l'Académie Royale des Sciences de Belgique, 65e Année, No. 7 (Bruxelles).—Zeitschrift für Physikalische Chemie, xvii. Band, 4 Heft (Leipzig).—Katalog der Bibliothek der K. Leopoldisch-Carolinischen Deutschen Akademie der Naturforscher, Sechste Liefg. (Halle).—Ditto Repertorium zu den Acta und Nova Acta der Akademie, Erster Band (Halle).—The Asclepiad, No. 43, Vol. xi. (Longmans).

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THURSDAY, SEPTEMBER 12, 1895.

A NEW STANDARD DICTIONARY.

A Standard Dictionary of the English Language. Vol. ii. Prepared under the supervision of Dr. I. K. Funk, Dr. F. A. March, and Dr. D. S. Gregory. (New York and London: Funk and Wagnall Co., 1895.)

SINCE the appearance of the first volume of this work, noticed in NATURE, vol. l. p. 146, we have often had occasion to refer to it. and have formed opinions as to its merits and faults. In many respects the dictionary is a very good one within its compass, though it does not contain much that is really new.

Before going further, it may be well to state briefly the magnitude of the work, and to give a general idea of its characteristics. The two volumes run into 2338 pages and contain 301,865 vocabulary terms, embellished by 5000 illustrations. A point upon which great stress is put is that more than two hundred editors and specialists have assisted in the production of the work, though it is not clear to what extent this assistance was given. Their services, with those of the five hundred readers for quotations, who are said to have been engaged upon this work, have helped to bring the cost up to one million dollars! Considering how little there is in the dictionary that is not in the "Century," "International," and other American dictionaries, one wonders where the money has gone. This, however, is by the way, and we only mention the matter because the large amount stated to have been spent in the production of the dictionary is put forward as a claim to favour.

A few definitions from the work will be the best means of indicating its merits. A whole column of the dictionary is taken up with definitions, and examples, of the use of the word science and its synonyms. The first two of the six definitions given are as follows:—

Science.—(1) Knowledge gained and verified by exact observation and correct thinking, especially as methodically formulated and arranged in a rational system; also, the sum of universal knowledge.

(2) Any department of knowledge in which the results of investigation have been worked out and systematised; an exact and systematic statement of knowledge concerning some subject or group of subjects; especially, a system of ascertained facts and principles covering and attempting to give adequate expression to a great natural group or division of knowledge.

The sciences are divided in the dictionary into (1) the mathematical, treating of quantity; (2) the physical, treating of matter and its properties; (3) the biological, treating of the phenomena of life; (4) the anthropological, treating of man; and (5) the theological, treating of the Deity. All the divisions are fully treated under their respective heads. Thus, under physical sciences, the classification of them as sciences of energy is given; the biological sciences are fully tabulated and their relation to one another shown with all their sub-divisions, and anthropology is made to embrace all the sciences relating to man. The departments of anthropology presented in the dictionary are (*a*) Somatology, (*b*) Ethnology, (*c*) Archæology. It is worth while printing the definition of the third of these for the benefit of unscientific archæologists.

Archæology.—The science of antiquities; in its widest sense, the branch of anthropology, embracing archæography, concerned with the systematic investigation of the relics of man and of his industries, and the classification and treatment of ancient remains and records of any or every kind, whether historic or prehistoric, of ancient places, customs, arts, &c.

In popular signification, archæology refers mainly to the collection or investigation of the materials from which a knowledge of the particular country under investigation may be obtained, which materials may be divided into *written*, *monumental*, and *traditional.* Scientific archæology is (1) general, including (*a*) the geology of the epoch of man and (*b*) the prehistoric ages; and (2) special, including the study of separate nations and areas.

These examples, which could be multiplied many times, are sufficient to show the generally trustworthy character, and the fulness, of the definitions, so far as science is concerned. The work has an attractive appearance, and offers every facility for consultation, and is altogether a desirable addition to a library.

THE CHEMISTRY OF LIGHTING.

Chemical Technology, or Chemistry in its Applications to Arts and Manufactures. Edited by C. E. Groves, F.R.S., and W. Thorp, B.Sc. Vol. ii. Lighting. (London: J. & A. Churchill, 1895.)

THE second volume of this important work possesses great intrinsic worth. Section i., dealing with fats and oils, by W. Y. Dent, contains much information concisely and clearly expressed. It may be noted that, in connection with the determination of specific gravity, the Sprengel tube is described, but no mention is made of the modification of this apparatus having the capillary arms at right angles and provided with expansion bulbs, although the latter form would always be used where accuracy combined with ease of manipulation were desired. When specific gravities are given to four significant figures, correction to a vacuum is necessary, or the fourth figure has no meaning. No mention is made of this in the text, and the specific gravities given are termed densities, a misuse of the latter term which occurs much too often.

The second Section, on stearine, by J. McArthur, puts forth the main processes for the decomposition of fats in a very explicit form. The writer wisely confines the term "saponification" to decomposition by means of a base.

The account of the candle manufacture, by L. and F. A. Field, given in Section iii., is highly interesting, and will be read with profit by many who have no connection with such matters, as well as by specialists. Producers of gas may well believe that their product will be in increasing demand when the candle industry flourishes in spite of the introduction of later forms of lighting. Doubtless candles owe their present hold on the public favour largely to the great improvements in quality effected by recent advances in the methods of manufacture. How great these advances are may be gathered from even a rapid perusal of the pages before us.

The description, in Section iv., of the petroleum industry, by Boverton Redwood, is both graphic and

complete. It forms the best monograph on the subject yet written. The origin of petroleum is so treated as to present the various theories put forward to account for its occurrence; necessarily, no authoritative decision can be given on this very debatable question. Concerning the occurrence of sulphur in the petroleums from Ohio and Canada, those interested would do well to supplement the bare mention of the fact here given by reference to the July number of the *Journal* of the Franklin Institute, where C. F. Mabery gives an account in which the subject is treated as its importance requires. Warren is stated by Mr. Redwood to have isolated hydrocarbons of the C_nH_{2n} series, termed naphthenes by Markownikoff. Mr. Mabery shows that the Ohio and Canadian petroleums do not yield the naphthenes of Markownikoff and Ogloblin, but give hydrocarbons of the C_nH_{2n+2} series of similar boiling points. This writer also proves conclusively the presence of benzene, toluene, and xylenes in these petroleums.

The manufacture of shale oil gives yet another instance of the application of continuous processes; the text contains very lucid descriptions of these, well and sufficiently illustrated. Few of the general public can have any adequate conception of the number and variety of lamps in existence for use with oils. An exhaustive account is given of these, and the advantages or disadvantages characteristic of the main types of oil-lamps are dwelt upon at sufficient length to enable an intelligent judgment to be formed as to the suitability of any particular lamp for the work required from it.

The Section on safety-lamps, with which this volume concludes, has been contributed by D. A. Louis, in conjunction with Boverton Redwood. It gives by no means the least interesting reading. Although the excellent account of the lamp-indication of fire-damp is highly technical, and calculated to be eminently useful to specialists, the general reader will find no difficulty in grasping the principles involved, and will much appreciate the clearness with which this important subject is treated.

It may be hoped that the high standard exhibited in this volume will be maintained in volume iii., announced as to appear shortly. The editors are certainly to be congratulated on the excellent production now before us.

W. T.

OUR BOOK SHELF.

Science Readers. By Vincent T. Murché. Book iv. Pp. 216. (London: Macmillan and Co., 1895.)

THE conversational method of instruction, which used to be so general in school books, is not one that leads to pleasant memories. Mr. Murché has created two boy prodigies in his "Science Readers," and they ask and answer questions of a teacher whose laudable ambition is to elicit and impart all kinds of scientific knowledge upon every suitable or unsuitable occasion. We reverence that teacher for his patience and for his ability to find texts in everything. The pity of it is, that lessons given in this way on all and sundry topics lack the quality which lies at the base of all true scientific knowledge, viz. the orderly arrangement of facts. A lesson on solids, liquids, and gases precedes one on our bodies, another on gravity precedes a lesson on vertebrates and invertebrates. A lesson on the classification of invertebrates is wedged between two on hydrostatic pressure, and so on throughout the book. Possibly the variety is introduced to charm the youthful mind, but it is not a desirable attribute of the book; for the method must result in the acquisition of unconnected information, and such knowledge has little to commend it. In the matter of illustration, and simplicity of language, the book leaves little to be desired.

A Garden of Pleasure. By E. V. B. Pp. 220. (London: Elliot Stock, 1895.)

A FEW chapters fresh with the fragrance of common country flowers, and breathing the life of "lustrous woodland." Here and there the authoress lapses into sentiment, but, taken as a whole, her language is attractive in its simplicity. The changes that go on in organic nature from month to month are drawn with careful touch, and many students of botany would derive benefit from the contemplation of the sketches.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The "4026·5" Line and D_3.

MAY I call attention to the fact that the line at 4026·5, now recognised as belonging to the spectrum of helium, and conspicuous in the Orion stars, is also prominent in the spectrum of the solar chromosphere. Although not given in the catalogue of chromosphere lines (which dates from 1872), it was observed and published as long ago as 1883 (*Am. Jour. Sci. and Art.*, November 1883), in connection with another line at 4092, seen at the same time. Since then the 4026 line has been observed repeatedly, and might be given a place in the catalogue with a relative frequency of about 15, and a brightness of 3 or 4. Like the other helium lines it has no dark analogue in the ordinary solar spectrum. The 4092 line falls upon a strong double line shown upon Rowland's map, but I am not sure to which of the two components it belongs; it is faint, and seldom seen.

While D_3 rarely appears as a dark line upon the solar spectrum, yet in the course of over twenty years I am able to count up a considerable number of instances; certainly not less than twenty or thirty. The phenomenon occurs usually in the penumbral region of an active sun-spot, which in its nucleus reverses the lines of hydrogen, magnesium, and sodium, and sometimes D_3 itself. By a slight motion of the telescope as one passes away from the nucleus, it crosses regions where D_3 appears as a smoky shade: on page 130 of "The Sun" I have figured a typical case.

I have not yet been fortunate enough to see the duplicity of D_3 myself, but Prof. Reed has observed it on several occasions.

Hanover, N.H., August 26. C. A. YOUNG.

On the Temperature Variation of the Thermal Conductivity of Rocks.

NATURE reproduces the results obtained by Lord Kelvin, P.R.S., and J. R. Erskine Murray, a paper read at the Royal Society, May 30, "On the Temperature Variation of the Thermal Conductivity of Rocks." These gentlemen arrived at the following results: "(§ 13). . . . that for slate with lines of fluor parallel to cleavage planes, the mean conductivity in the range from 123° C. to 202° C. is 91 per cent. of the mean conductivity in the range from 50° C. to 123° C., and for granite the mean conductivity in the range from 145° C. to 214° C. is 88 per cent. of the mean conductivity in the range from 81° C. to 145° C."

These results are so widely different from those I obtained by another method, and which Lord Kelvin had the kindness to publish in NATURE, March 7, 1895, p. 439, that I must be allowed to introduce here a word of objection.

It seems to me that details of experimental dispositions are important enough, and should be trustworthy. It is however, not opportune to discuss them minutely now.

The experimenters based their work on the case of Fourier's

"indefinite wall," which is characterised by the fact that temperature differences in the parallel planes are exactly proportional to the distances of these planes. According to the experiment, they get the result that this proportionality does not exist, and that conductibility varies much according to temperature.

In my opinion, this absence of proportionality arrived at, proves rather that the experimental conditions were defective, and are in contradiction with the hypothesis of the "indefinite wall" case.

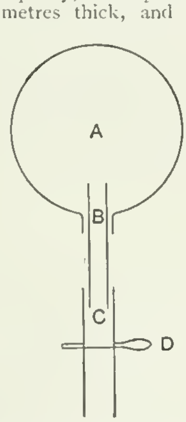
I admit, in principle, the employed method, but I think it should be modified until—for the same temperature of the bath, the said proportionality should be obtained; then, in a new experiment, the temperature of the bath being higher, it should be verified if the proportionality and the conductibility remain, or if the last increases or diminishes with the temperature.

Neuchâtel, August. ROBERT WEBER.

Experimental Mountain-building.

PROF. JOHANNES WALTHER, of Jena, requests me to communicate to you the following details regarding an interesting experiment which he has recently devised for teaching purposes: it is intended to explain mountain-formation.

He compares the system of folds on the surface of our slowly-ageing earth to the wrinkles which form on the skin of a drying apple, and points out that the height of our mountain-chains in relation to the mass of the globe is precisely comparable to the wrinkles on the skin of the apple. In order to demonstrate the formation of these folds, he takes an indiarubber balloon (A), and attaches to it a bit of glass tubing (B). On to this is stretched a piece of indiarubber tubing (C), which is pinched close by the stopcock (D). When the indiarubber balloon is blown out to its full capacity, it is spread over with a layer of flour-paste two millimetres thick, and is then dipped and



twirled round and round in dry wheaten flour until a perfectly smooth crust, three to four millimetres in thickness, covers the whole sphere. The balloon is then placed on a tripod, so that the indiarubber tubing (C) dips exactly into a glass of water standing below. Thereupon the stopcock is turned open, and the air is allowed to escape in single bubbles: the volume of the ball is lessened, and lateral pressure makes itself immediately felt in the paste-crust. Small folds gradually grow bigger, single folds unite to form systems of folds, flat areas of depression sink deeper and deeper, and the neighbouring folds twirl and cross over the depression. The features of the Cordilleras, of the Jura, and many other well-known tectonic relations are thus reproduced with striking accuracy. Whenever it is desired to repeat the experiment, one need simply blow the balloon out again, smooth out the folded surface by dipping and twirling in dry flour, and all is ready for another demonstration.

London, August 26. L. BELINFANTE.

Joseph Thomson.

IN Mr. Gregory's sympathetic notice (NATURE, p. 440) of Joseph Thomson, he hardly does justice to the memory of the deceased traveller in relation to the scientific results of his expeditions; at least so far as botany is concerned. During his too short career Thomson presented three considerable collections of dried plants to Kew. The first, which appears to have been made on his own initiative, chiefly between Lake Nyassa and Lake Tanganyika, was secured for Kew in 1880, through the instrumentality of the late Colonel J. A. Grant, F.R.S. This was not the subject of a special paper: yet it contained a number of interesting novelties, some of which have from time to time been published in Hooker's "Icones Plantarum" and elsewhere. Before going out again Thomson carefully studied the means by which his collecting opportunities might be turned to the greatest advantage. Armed with this knowledge he collected even more successfully in the Kilimanjaro and other mountains of Eastern Equatorial Africa. This second collection reached Kew in September 1884, and proved of the greatest scientific importance, being the first adequate illustration of the mountain flora of that region. It contained scarcely 150 species; but the specimens were selected with admirable judgment, and were sufficient for all purposes. It was worked out by Sir Joseph D. Hooker and Prof. D. Oliver, and the very important results recorded in the twenty-first volume of the *Journal* of the Linnean Society. This paper and Thomson's collection will always rank among the classical documents for the study of the phytogeography of Central Africa. Subsequently Mr. Thomson sent to Kew the botanical fruits of his journey to the Atlas Mountains, and although they contained very few previously unknown plants, they were none the less instructive as a sample of the flora of that comparatively little-known part of the world. Had he preserved his health Thomson might have taken his place in the first rank of botanical explorers. He had acquired the rare gift of selection in collecting; of knowing what to secure and what to neglect.

W. BOTTING HEMSLEY.

Late Nestlings.

TO-DAY I observed nests of the house-martin underneath the eaves of the clock-tower at Lamlash Pier, on the south and west sides. The parents were busy feeding their young, whose cries I heard. Surely this is a late date for a migratory bird. How are these nestlings to get across the ocean? JAS. SHAW.

Barrhead, September 7.

THE INSTITUTE OF FRANCE.

IN a few weeks, at the end of October, the Institut National de France is to celebrate its first centenary. Some words concerning its origin and organisation may be of interest at the present moment.

The Institute is the outcome of a previous scientific society, entirely due to individual initiative. During the first half of the seventeenth century, a few men, between whom love of science was a firm bond, agreed to meet at regular intervals at the house of one of their number, informally, in order to exchange views, to keep each other posted up on their various researches, and to make up an unconventional assembly of congenial spirits. It was more of a temporary or intermittent club than a real society, as we understand the latter now. These men were mostly mathematicians and physicists—for at that time natural science was more in the *werden* than in the *sein* state—and Mersenne, Descartes, Blaise Pascal, Gassendi, are some of them. Their meetings soon attracted public attention, and the great Colbert, anxious for the development of the arts of peace after the Pyrenees treaty had put an end to the war, considered them as being of sufficient importance and utility to take an interest in them, and to support the incipient society officially.

Colbert even made out a full plan of what was to be realised 200 years later; what he organised was a body of scientific men who were to meet at regular intervals, and were divided into three classes—historical scholars, literary men, and, finally, scientific men. The private society of mathematicians and physicists grew into the Académie des Sciences, and each of the three academies met separately in the Bibliothèque du Roi, at Colbert's own residence. The king, as a sign of his approval, gave some money for experiments, and some pensions.

Among the members (no one knows how they were appointed) were Huyghens, Mariotte, Pecquet, Picard, Roberval. The Academy of Sciences, the Académie Française, and the Académie des Inscriptions et Médailles thus lived in harmony, each having its particular pursuits. The history of these academies would take too much space; it is enough to have shown how they originated. They lived on till the Revolution, when they were organised on a new basis, and the Institute came into existence. The whole constitution of France being altered, that of the academies had also to be changed.

The Institute was founded in 1795. Article 298 of the *Constitution du 5 Fructidor, an* iii. (August 22, 1795) gave it the mission of "registering discoveries, and perfecting arts and sciences," while later laws provided for the details of the scheme, that of the 3 *Brumaire, an* iv., *i.e.* October 25, 1795. According to this law, the Institut National—a new name applied to, practically, an old thing—was divided into three classes—scientific (10 sections); moral and political (6 sections); literary and artistic (8 sections). Bonaparte (3 *Pluviose, an* xi., January 23, 1803) altered this plan, and added a fourth class, so that the Institute comprised the class of mathematical and physical sciences, with 11 sections; that of French language and literature (no sections); that of ancient language and literature (no sections); and that of fine arts (5 sections). In 1816, upon the return of monarchy, the general plan was respected, but in 1832 a fifth class was added: that of moral and political sciences, which had disappeared in 1803. Those five classes still exist, under the names of Académie Française, Académie des Sciences, Académie des Beaux-Arts, Académie des Sciences Morales et Politiques, Académie des Inscriptions et Belles Lettres. They still dwell in the Palais des Quatre Nations on the Seine, where Bonaparte housed them in 1805.

At present, the Institute is a society of men of eminence, divided into five distinct sub-societies, or academies, each member being at the same time, and as a matter of course, member of this or that particular academy, and of the Institute as a whole. Each academy has its definite purpose, and meets each week on fixed and different days; the Institute, as a whole, meets once a year, in October.

As a whole, the Institute is regulated by a committee of delegates, elected by, and in, the five academies, while each academy has its own president and secretary.

Two points must be noticed in reference to the academies. The one is that the Académie de Medécine has nothing at all to do with the Institute; it is a separate society of medical men only (quite distinct, without the slightest relationship to any of the above-mentioned academies, or to the Institute. The other is that there is no connection whatever between the Institute or academies which make up the Institute, and the title of Officier d'Académie. To be Officier d'Académie is to have received from the Department of Public Instruction a special decoration (of the *Palmes Académiques*) which is, theoretically at least, more specially destined to persons who serve the cause of education and instruction. The Officiers d'Académie are thousands in number; they have nothing at all to do with the Academies.

Now, as to the membership of the latter.

New members are always elected by the members of each academy. A man considers himself as eligible for such or such academy; all he has to do, when a vacancy occurs in the academy or in the section to which he should belong, considering his previous work, is to declare himself a candidate by a letter addressed to the president of the latter, and to prepare a pamphlet in which he gives the list of his scientific or literary titles, of his works, of his researches or discoveries, of the functions he has occupied, &c.; and this pamphlet he sends or carries to each of the members of the academy. It is customary for every candidate to pay a visit to each of the latter, and then he waits for the result; in the meantime canvassing, in order to secure this or that member's vote when things do not seem to run smoothly. A very amusing book might be written of the anecdotes which are current upon the devices suggested to the candidates by what is called the "green fever," *la fièvre verte*, the fever which takes hold of a man anxious to wear the green-laced uniform which the members of the Institute wear upon official occasions. But such a book could be published only after the death of the author and of those concerned. Generally speaking, however, the Académie des Sciences would contribute little to the making of this book. Each election must be approved by the President of the Republic, and is approved as a matter of course. Each member receives a small *indemnité* of £60 a year.

Each academy has a limited number of members, but in most academies there are different classes of membership. The Académie Française, for literary men, comprises 40 immortals all told, one of whom is perpetual (life) secretary. It has no associates nor corresponding members, and while the members have little or nothing to do as members, save the preparation of a dictionary, and examining works which compete for various prizes, it is the custom for each new member to deliver a very elaborate speech concerning his predecessor, and one of the members answers this *discours de réception* by a speech concerning the works of the new-comer.

The Académie des Inscriptions et Belles Lettres, for men who deal specially with history, comprises 40 members (of whom one is life-secretary), 10 free members, 8 foreign associates, 30 foreign and 20 national corresponding members. Among the foreign associates are Prof. Max Müller, Sir Henry Rawlinson, W. Stokes; among foreign corresponding members, Mr. R. S. Poole, Sir J. Evans, M. A. Neubauer, Sir E. M. Thompson.

The Académie des Beaux-Arts is divided into five sections (painting, sculpture, architecture, engraving, music), and comprises 41 members (one of whom is life-secretary). There are besides 10 free members, 10 foreign associates, and 50 correspondents. Among the associates are Sir J. E. Millais, Sir F. Leighton, Mr. Alma Tadema; among the corresponding members, Prof. H. Herkomer, Sir E. Burne-Jones, Mr. Waterhouse, Mr. R. W. Macbeth.

The Académie des Sciences Morales et Politiques is divided into five sections (philosophy, morals, law, political economy, history), and comprises 40 members (of whom one is life-secretary), 16 free members, 6 foreign associates, 48 corresponding members. Among the foreign associates are Right Hon. W. E. Gladstone and Mr. Henry Reeve; Mr. Robert Flint, Right Hon. J. Bryce, Sir Fredk. Pollock, Right Hon. G. J. Goschen, Bishop Stubbs, and Mr. Lecky are corresponding members.

Last, but by no means least, comes the Académie des Sciences, which certainly exerts the largest influence, and is the most highly considered in public opinion. Divided into eleven sections, it comprises 68 members (of whom two are life-secretaries), 10 free members, 8 foreign associates, 100 corresponding members. Lord Kelvin, Sir Joseph Lister, and Dr. E. Frankland are among the associates. The British Correspondants are as follows:—Mathematical sciences (geometry: Prof. J. J. Sylvester, Rev. Prof. Salmon; astronomy: Dr. J. R. Hind, Mr. Norman Lockyer, Dr. W. Huggins; geography and navigation: Sir George Henry Richards; general physics: Sir G. G. Stokes, Lord Rayleigh. Physical sciences (chemistry: Prof. A. W. Williamson, Sir Henry Roscoe, Prof. W. Ramsay; mineralogy: Dr. J. Prestwich, Sir A. Geikie; botany: Sir Joseph D. Hooker, Dr. Maxwell Masters; rural economy: Sir J. B. Lawes, Sir J. H. Gilbert; anatomy and zoology: Sir W. Flower;

medicine and surgery: Sir James Paget. Prof. Huxley was a corresponding member also.

Each academy has more or less money left to it in order to distribute prizes for different subject-matters; the Académie des Sciences and Académie Française are the richest. The Duke d'Aumale has agreed to leave the splendid residence of Chantilly, with the books and collections it contains, to the Institute, and this handsome gift is accompanied by a sum of money to help to keep the castle in good order. It is estimated that, all paid, the Institute will be 100,000 francs richer each year for this gift.

English corresponding members and associates will have a good opportunity of visiting the fine chateau of Chantilly, for on October 26 the Duke opens the doors to all members of the Institute, and bids them welcome. The celebration of the centenary, to which *all* members of each Academy, *all* corresponding members and associates in every country have been, or are being, invited, will last four days. The programme has been given in NATURE (August 8) in full. The only new feature I can introduce, is the programme of the afternoon performance at the Comédie Française, where the best actors of the best theatre in France will play *Les Horaces* (Corneille), *Les Femmes Savantes* (Molière), and recite a piece of poetry by Sully-Prudhomme. The railway fares will be reduced 50 per cent. for all foreigners invited.

All may be sure to receive a hearty welcome. If the Institut de France does not contain *all* our "best men" in the different departments of knowledge or art, it contains only men of recognised authority. They are men whose aims are noble, and their feelings can but be most cordial towards those whose aims are the same towards their fellow-workers, whatever language they speak, whatever country they come from, towards all whose work and character are high enough to have secured for them the highest recognition French science can award.

HENRY DE VARIGNY.

THE IPSWICH MEETING OF THE BRITISH ASSOCIATION.

Coming after the Oxford year, the meeting at Ipswich is in numbers a comparatively small one; but, from a scientific point of view, everything augurs well. The papers promise to be of more than usual interest, and are so numerous that most of the Sections will have to sit early and late in order to get through all the work before them.

We have previously referred at some length to the work proposed for Sections A, B, C, D, G, and H.

Section D is this year reserved entirely to zoology and animal physiology, under the presidency of Prof. W. A. Herdman.

Prof. A. C. Haddon will read a paper on the exploration of the isles of the Pacific. Dr. Bashford Deane, of New York, is to read two papers—one on an apparatus for catching oyster spat, the other on the ganoids of North America. Prof. McIntosh will open a discussion on British fisheries. A paper will be read by the Rev. T. R. R. Stebbing, on zoological nomenclature and publication. Special interest is likely to be taken in a paper by the President and Prof. Boyce on the subject of oysters and typhoid, by those who propose to join in the excursion to the Colne Oyster Fishery (Colchester), which has just been added to the programme for Wednesday. It is intended to make a large use of the lantern for illustrating papers in the Section.

The provisional programme in Section E (Geography) makes it evident that the Section is, as usual, to be a popular one. After the address of the President, Mr. H. J. Mackinder, an account will be given, by Mr. H. S. Cowper, of a journey over Tarhuna and Gharian in Tripoli; and Mr. J. Batalka-Reis will discuss how to consider and write the history of the discovery of the world. On Friday, the papers will be given by Mr. C. E. Borchgrevink, describing his voyage to the Antarctic Sea; by Mr. H. N. Dickson, on oceanographical research in the North Sea; by Mr. W. B. Blaikie, on the cosmosphere; and by Mr. John Dodd, on Formosa. On Monday, Mr. E. G. Ravenstein will present a report on the climate of tropical Africa; and there will be papers by Mr. G. F. Scott Elliot, on Ruwenzori and East Africa; by Captain S. L. Hinde, on the Congo State; by Mr. J. T. P. Keatly, on the port of the Upper Nile in relation to the highways of commerce; and by Mr. J. L. Myres, on the maps of Herodotus. On Tuesday, Mr. Weston will deal with the New Zealand Alps, and Mr. J. L. Myres with Asia Minor, whilst Mr. A. Trevor Battye will give an account of Kolguev.

In Section F (Economic Science and Statistics), over which Mr. L. L. Price presides, bimetallism appears early on the scene, the arrangement being to devote Friday morning to a monetary discussion, in which representatives of the Bimetallic League and of the Gold Standard Defence Association, and others, are expected to take part. Monday will be given up to a discussion on the state of agriculture, on which question Captain E. G. Pretyman, M.P., will read a paper from the landlord's point of view, and Mr. Herman Biddell one from the tenant's point of view. This discussion has unfortunately been fixed for the same day as the discussion on the relation of chemistry to agriculture in Section B, but it is hoped that by an arrangement of the hours the two discussions may not clash. Other contributions in Section F will be by Mr. H. W. Wolff, on land banks; Mr. H. Moore, on co-operation in agriculture; Mr. E. Cannan, on population; Mr. H. Higgs, on the climbing ratio; and Rev. Frome Wilkinson, on the State and the labourer.

In Section H (Anthropology), in which Prof. W. M. Flinders Petrie presides, ethnology is to play a prominent part. The Section will, therefore, feel all the more the absence of Mr. E. W. Brabrook, who is unable to come to Ipswich on account of the very sad bereavement he has so recently suffered. It has been arranged that the Section shall sit each morning till 12.30 or 1, and then reassemble at 2, on each day except Saturday for a lecture illustrated by the lantern.

Botany is sitting for the first time as a distinct Section (K), under the presidency of Mr. W. T. Thiselton-Dyer. Amongst the papers will be one on Sporangia by Prof. F. O. Bower. Dr. D. H. Scott will speak on fossil botany, with special reference to the researches of the late Prof. Williamson. A paper on fossil botany will also be read by Prof. Solms-Laubach, of Strasbourg. Prof. E. C. Hansen, of Copenhagen, promises a paper on the variation of yeast cells, and Mr. A. C. Seward one on the Wealden Flora. Amongst other foreign botanists attending the meeting is Dr. T. M. Treub, of Java. A special botanical excursion, not figuring as one of the regular excursions, is being arranged.

INAUGURAL ADDRESS BY SIR DOUGLAS GALTON, K.C.B., D.C.L., F.R.S., PRESIDENT.

My first duty is to convey to you, Mr. Mayor, and to the inhabitants of Ipswich, the thanks of the British Association for your hospitable invitation to hold our sixty-fifth meeting in your ancient town, and thus to recall the agreeable memories of the similar favour which your predecessors conferred on the Association forty-four years ago.

In the next place I feel it my duty to say a few words on the great loss which science has recently sustained—the death of

the Right Hon. Thomas Henry Huxley. It is unnecessary for me to enlarge, in the presence of so many to whom his personality was known, upon his charm in social and domestic life; but upon the debt which the Association owes to him for the assistance which he rendered in the promotion of science I cannot well be silent. Huxley was pre-eminently qualified to assist in sweeping away the obstruction by dogmatic authority, which in the early days of the Association fettered progress in certain branches of science. For, whilst he was an eminent leader in biological research, his intellectual power, his original and intrepid mind, his vigorous and masculine English, made him a writer who explained the deepest subject with transparent clearness. And as a speaker his lucid and forcible style was adorned with ample and effective illustration in the lecture-room; and his energy and wealth of argument in a more public arena largely helped to win the battle of evolution, and to secure for us the right to discuss questions of religion and science without fear and without favour.

It may, I think, interest you to learn that Huxley first made the acquaintance of Tyndall at the meeting of the Association held in this town in 1851.

About forty-six years ago I first began to attend the meetings of the British Association; and I was elected one of your general secretaries about twenty-five years ago.

It is not unfitting, therefore, that I should recall to your minds the conditions under which science was pursued at the formation of the Association, as well as the very remarkable position which the Association has occupied in relation to science in this country.

Between the end of the sixteenth century and the early part of the present century several societies had been created to develop various branches of science. Some of these societies were established in London, and others in important provincial centres.

In 1831, in the absence of railways, communication between different parts of the country was slow and difficult. Science was therefore localised; and in addition to the universities in England, Scotland, and Ireland, the towns of Birmingham, Manchester, Plymouth and York each maintained an important nucleus of scientific research.

ORIGIN OF THE BRITISH ASSOCIATION.

Under these social conditions the British Association was founded in September 1831.

The general idea of its formation was derived from a migratory society which had been previously formed in Germany; but whilst the German society met for the special occasion on which it was summoned, and then dissolved, the basis of the British Association was continuity.

The objects of the founders of the British Association were enunciated in their earliest rules to be:—

"To give a stronger impulse and a more systematic direction to scientific inquiry; to promote the intercourse of those who cultivated science in different parts of the British Empire with one another, and with foreign philosophers; to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress."

Thus the British Association for the Advancement of Science based its utility upon the opportunity it afforded for combination.

The first meeting of the Association was held at York with 353 members.

As an evidence of the want which the Association supplied, it may be mentioned that at the second meeting, which was held at Oxford, the number of members was 435. The third meeting, at Cambridge, numbered over 900 members, and at the meeting at Edinburgh in 1834 there were present 1298 members.

At its third meeting, which was held at Cambridge in 1833, the Association, through the influence it had already acquired, induced the Government to grant a sum of £500 for the reduction of the astronomical observations of Baily. And at the same meeting the General Committee commenced to appropriate to grants to research the surplus from the subscriptions of its members. The committees on each branch of science were desired "to select definite and important objects of science, which they may think most fit to be advanced by an application of the funds of the society, either in compensation for labour, or in defraying the expense of apparatus, or otherwise, stating their reasons for their selection, and, when they may think proper, designating individuals to undertake the desired investigations."

The several proposals were submitted to the Committee of Recommendations, whose approval was necessary before they could be passed by the General Committee. The regulations then laid down still guide the Association in the distribution of its grants. At that early meeting the Association was enabled to apply £600 to these objects.

I have always wondered at the foresight of the framers of the constitution of the British Association, the most remarkable feature of which is the lightness of the tie which holds it together. It is not bound by any complex central organisation. It consists of a federation of Sections, whose youth and energy are yearly renewed by a succession of presidents and vice-presidents, whilst in each Section some continuity of action is secured by the less movable secretaries.

The governing body is the General Committee, the members of which are selected for their scientific work; but their controlling power is tempered by the law that all changes of rules, or of constitution, should be submitted to, and receive the approval of, the Committee of Recommendations. This committee may be described as an ideal Second Chamber. It consists of the most experienced members of the Association.

The administration of the Association in the interval between annual meetings is carried on by the Council, an executive body, whose duty it is to complete the work of the annual meeting (*a*) by the publication of its proceedings; (*b*) by giving effect to resolutions passed by the General Committee; (*c*) it also appoints the Local Committee and organises the *personnel* of each Section for the next meeting.

I believe that one of the secrets of the long-continued success and vitality of the British Association lies in this purely democratic constitution, combined with the compulsory careful consideration which must be given to suggested organic changes.

The Association is now in the sixty-fifth year of its existence. In its origin it invited the philosophical societies dispersed throughout Great Britain to unite in a co-operative union.

Within recent years it has endeavoured to consolidate that union.

At the present time almost all important local scientific societies scattered throughout the country, some sixty-six in number, are in correspondence with the Association. Their delegates hold annual conferences at our meetings. The Association has thus extended the sphere of its action: it places the members of the local societies engaged in scientific work in relation with each other, and brings them into co-operation with members of the Association and with others engaged in original investigations, and the papers which the individual societies publish annually are catalogued in our Report. Thus by degrees a national catalogue will be formed of the scientific work of these societies.

The Association has, moreover, shown that its scope is coterminous with the British Empire by holding one of its annual meetings at Montreal, and we are likely soon to hold a meeting in Toronto.

CONDITION OF CERTAIN SCIENCES AT THE FORMATION OF THE BRITISH ASSOCIATION.

The Association, at its first meeting, began its work by initiating a series of reports upon the then condition of the several sciences.

A rapid glance at some of these reports will not only show the enormous strides which have been made since 1831 in the investigation of facts to elucidate the laws of nature, but it may afford a slight insight into the impediments offered to the progress of investigation by the mental condition of the community, which has been for so long satisfied to accept assumptions without undergoing the labour of testing their truth by ascertaining the real facts. This habit of mind may be illustrated by two instances selected from the early reports made to the Association. The first is afforded by the report made in 1832, by Mr. Lubbock, on "Tides."

This was a subject necessarily of importance to England as a dominant power at sea. But in England records of the tides had only recently been commenced at the dockyards of Woolwich, Sheerness, Portsmouth, and Plymouth, on the request of the Royal Society, and no information had been collected upon the tides on the coasts of Scotland and Ireland.

The British Association may feel pride in the fact that within three years of its inception, viz. by 1834, it had induced the Corporation of Liverpool to establish two tide gauges, and the

Government to undertake tidal observations at 500 stations on the coasts of Britain.

Another cognate instance is exemplified by a paper read at the second meeting, in 1832, upon the State of Naval Architecture in Great Britain. The author contrasts the extreme perfection of the carpentry of the internal fittings of the vessels with the remarkable deficiency of mathematical theory in the adjustment of the external form of vessels, and suggests the benefit of the application of refined analysis to the various practical problems which ought to interest shipbuilders—problems of capacity, of displacement, of stowage, of velocity, of pitching and rolling, of masting, of the effects of sails and of the resistance of fluids; and, moreover, suggests that large-scale experiments should be made by Government, to afford the necessary data for calculation.

Indeed, when we consider how completely the whole habit of mind of the populations of the Western world has been changed, since the beginning of the century, from willing acceptance of authority as a rule of life to a universal spirit of inquiry and experimental investigation, is it not probable that this rapid change has arisen from society having been stirred to its foundations by the causes and consequences of the French Revolution?

One of the earliest practical results of this awakening in France was the conviction that the basis of scientific research lay in the accuracy of the standards by which observations could be compared; and the following principles were laid down as a basis for their measurements of length, weight, and capacity: viz. (1) that the unit of linear measure applied to matter in its three forms of extension, viz. length, breadth, and thickness, should be the standard of measures of length, surface, and solidity; (2) that the cubic contents of the linear measure in decimetres of pure water at the temperature of its greatest density should furnish at once the standard weight and the measure of capacity.[1] The metric system did not come into full operation in France till 1840; and it is now adopted by all countries on the continent of Europe except Russia.

The standards of length which were accessible in Great Britain at the formation of the Association were the Parliamentary standard yard lodged in the Houses of Parliament (which was destroyed in 1834 in the fire which burned the Houses of Parliament); the Royal Astronomical Society's standard; and the 10-foot bar of the Ordnance Survey.

The first two were assumed to afford exact measurements at a given temperature. The Ordnance bar was formed of two bars on the principle of a compensating pendulum, and afforded measurements independent of temperature. Standard bars were also disseminated throughout the country, n possession of the corporations of various towns.

The British Association early recognised the importance of uniformity in the record of scientific facts, as well as the necessity for an easy method of comparing standards and for verifying differences between instruments and apparatus required by various observers pursuing similar lines of investigation. At its meeting at Edinburgh in 1834 it caused a comparison to be made between the standard bar at Aberdeen, constructed by Troughton, and the standard of the Royal Astronomical Society, and reported that the scale "was exceedingly well finished; it was about $\frac{1}{500}$th of an inch shorter than the 5-feet of the Royal Astronomical Society's scale, but it was evident that a great number of minute, yet important, circumstances have hitherto been neglected in the formation of such scales, without an attention to which they cannot be expected to accord with that degree of accuracy which the present state of science demands." Subsequently, at the meeting at Newcastle in 1863, the Association appointed a committee to report on the best means of providing for a uniformity of weights and measures with reference to the interests of science. This committee recommended the metric decimal system a recommendation which has been endorsed by a committee of the House of Commons in the last session of last Parliament.

British instrument-makers had been long conspicuous for accuracy of workmanship. Indeed, in the eighteenth century practical astronomy had been mainly in the hands of British observers; for although the mathematicians of France and other countries on the continent of Europe were occupying the foremost place in mathematical investigation, means of astronomical observation had been furnished almost exclusively by English artisans.

[1] The litre is the volume of a kilogramme of pure water at its maximum density, and is slightly less than the litre was intended to be, viz. one cubic decimetre. The weight of a cubic decimetre of pure water is 1·000013 kilogrammes.

The sectors, quadrants, and circles of Ramsden, Bird, and Cary were inimitable by continental workmen.

But the accuracy of the mathematical-instrument maker had not penetrated into the engineer's workshop. And the foundation of the British Association was coincident with a rapid development of mechanical appliances.

At that time a good workman had done well if the shaft he was turning, or the cylinder he was boring, "was right to the $\frac{1}{32}$nd of an inch." This was, in fact, a degree of accuracy as fine as the eye could usually distinguish.

Few mechanics had any distinct knowledge of the method to be pursued for obtaining accuracy; nor, indeed, had practical men sufficiently appreciated either the immense importance or the comparative facility of its acquisition.

The accuracy of workmanship essential to this development of mechanical progress required very precise measurements of length, to which reference could be easily made. No such standards were then available for the workshops. But a little before 1830 a young workman named Joseph Whitworth realised that the basis of accuracy in machinery was the making of a true plane. The idea occurred to him that this could only be secured by making three independent plane surfaces: if each of these would lift the other, they must be planes, and they must be true.

The true plane rendered possible a degree of accuracy beyond the wildest dreams of his contemporaries in the construction of the lathe and the planing machine, which are used in the manufacture of all tools.

His next step was to introduce an exact system of measurement, generally applicable in the workshop.

Whitworth felt that the eye was altogether inadequate to secure this, and appealed to the sense of touch for affording a means of comparison. If two plugs be made to fit into a round hole, they may differ in size by a quantity imperceptible to the eye, or to any ordinary process of measurement, but in fitting them into the hole the difference between the larger and the smaller is felt immediately by the greater ease with which the smaller one fits. In this way a child can tell which is the larger of two cylinders differing in thickness by no more than $\frac{1}{5000}$th of an inch.

Standard gauges, consisting of hollow cylinders with plugs to fit, but differing in diameter by the $\frac{1}{1000}$th or the $\frac{1}{10000}$th of an inch, were given to his workmen, with the result that a degree of accuracy inconceivable to the ordinary mind became the rule of the shop.

To render the construction of accurate gauges possible, Whitworth devised his measuring machine, in which the movement was effected by a screw; by this means the distance between two true planes might be measured to the one-millionth of an inch.

These advances in precision of measurement have enabled the degree of accuracy which was formerly limited to the mathematical-instrument maker to become the common property of every machine shop. And not only is the latest form of steam-engine, in the accuracy of its workmanship, little behind the chronometer of the early part of the century, but the accuracy in the construction of experimental apparatus which has thus been introduced has rendered possible recent advances in many lines of research.

Lord Kelvin said, in his Presidential Address at Edinburgh, "Nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient, long-continued labour in the sifting of numerical results." The discovery of argon, for which Lord Rayleigh and Prof. Ramsay have been awarded the Hodgkin prize by the Smithsonian Institution, affords a pregnant illustration of the truth of this remark. Indeed, the provision of accurate standards not only of length, but of weight, capacity, temperature, force, and energy, are amongst the foundations of scientific investigation.

In 1842, the British Association obtained the opportunity of extending its usefulness in this direction.

In that year the Government gave up the Royal Observatory at Kew, and offered it to the Royal Society, who declined it. But the British Association accepted the charge. Their first object was to continue Sabine's valuable observations upon the vibrations of a pendulum in various gases, and to promote pendulum observations in different parts of the world. They subsequently extended it into an observatory for comparing and verifying the various instruments which recent discoveries in physical science had suggested for continuous meteorological and magnetic observations, for observations and experiments on atmospheric electricity, and for the study of solar physics.

This new departure afforded a means for ascertaining the advantages and disadvantages of the several varieties of scientific instruments; as well as for standardising and testing instruments, not only for instrument-makers, but especially for observers by whom simultaneous observations were then being carried on in different parts of the world; and also for training observers proceeding abroad on scientific expeditions.

Its special object was to promote original research, and expenditure was not to be incurred on apparatus merely intended to exhibit the necessary consequences of known laws.

The rapid strides in electrical science had attracted attention to the measurement of electrical resistances, and in 1859 the British Association appointed a special committee to devise a standard. The standard of resistance proposed by that committee became the generally accepted standard, until the requirements of that advancing science led to the adoption of an international standard.

In 1866 the Meteorological Department of the Board of Trade entered into close relations with the Kew Observatory.

And in 1871 Mr. Gassiot transferred £10,000 upon trust to the Royal Society for the maintenance of the Kew Observatory, for the purpose of assisting in carrying on magnetical, meteorological, and other physical observations. The British Association thereupon, after having maintained this Observatory for nearly thirty years, at a total expenditure of about £12,000, handed the Observatory over to the Royal Society.

The *Transactions* of the British Association are a catalogue of its efforts in every branch of science, both to promote experimental research and to facilitate the application of the results to the practical uses of life.

But probably the marvellous development in science which has accompanied the life-history of the Association will be best appreciated by a brief allusion to the condition of some of the branches of science in 1831 as compared with their present state.

GEOLOGICAL AND GEOGRAPHICAL SCIENCE.

Geology.

At the foundation of the Association geology was assuming a prominent position in science. The main features of English geology had been illustrated as far back as 1821, and, among the founders of the British Association, Murchison and Phillips, Buckland, Sedgwick and Conybeare, Lyell and De la Beche, were occupied in investigating the data necessary for perfecting a geological chronology by the detailed observations of the various British deposits, and by their co-relation with the continental strata. They were thus preparing the way for those large generalisations which have raised geology to the rank of an inductive science.

In 1831 the Ordnance maps published for the southern counties had enabled the Government to recognise the importance of a geological survey by the appointment of Mr. De la Beche to affix geological colours to the maps of Devonshire and portions of Somerset, Dorset and Cornwall; and in 1835, Lyell, Buckland and Sedgwick induced the Government to establish the Geological Survey Department, not only for promoting geological science, but on account of its practical bearing on agriculture, mining, the making of roads, railways, and canals, and on other branches of national industry.

Geography.

The Ordnance Survey appears to have had its origin in a proposal of the French Government to make a joint measurement of an arc of the meridian. This proposal fell through at the outbreak of the Revolution; but the measurement of the base for that object was taken as a foundation for a national survey. In 1831, however, the Ordnance Survey had only published the 1-inch map for the southern portion of England, and the great triangulation of the kingdom was still incomplete.

In 1834 the British Association urged upon the Government that the advancement of various branches of science was greatly retarded by the want of an accurate map of the whole of the British Isles; and that, consequently, the engineer and meteorologist, the agriculturist and the geologist, were each fettered in their scientific investigations by the absence of those accurate data which now lie ready to his hand for the measurement of length, of surface, and of altitude.

Yet the first decade of the British Association was coincident with a considerable development of geographical research. The Association was persistent in pressing on the Government the scientific importance of sending the expedition of Ross to the Antarctic and of Franklin to the Arctic regions. We may trust that we are approaching a solution of the geography of the North Pole; but the Antarctic regions still present a field for the researches of the meteorologist, the geologist, the biologist, and the magnetic observer, which the recent voyage of M. Borchgrevink leads us to hope may not long remain unexplored.

In the same decade the question of an alternative route to India by means of a communication between the Mediterranean and the Persian Gulf was also receiving attention, and in 1835 the Government employed Colonel Chesney to make a survey of the Euphrates valley in order to ascertain whether that river would enable a practicable route to be formed from Iskanderoon, or Tripoli, opposite Cyprus, to the Persian Gulf. His valuable surveys are not, however, on a sufficiently extensive scale to enable an opinion to be formed as to whether a navigable waterway through Asia Minor is physically practicable, or whether the cost of establishing it might not be prohibitive.

The advances of Russia in Central Asia have made it imperative to provide an easy, rapid, and alternative line of communication with our Eastern possessions, so as not to be dependent upon the Suez Canal in time of war. If a navigation cannot be established, a railway between the Mediterranean and the Persian Gulf has been shown by the recent investigations of Messrs. Hawkshaw and Hayter, following on those of others, to be perfectly practicable and easy of accomplishment; such an undertaking would not only be of strategical value, but it is believed it would be commercially remunerative.

Speke and Grant brought before the Association, at its meeting at Newcastle in 1863, their solution of the mystery of the Nile basin, which had puzzled geographers from the days of Herodotus; and the efforts of Livingstone and Stanley and others have opened out to us the interior of Africa. I cannot refrain here from expressing the deep regret which geologists and geographers, and indeed all who are interested in the progress of discovery, feel at the recent death of Joseph Thomson. His extensive, accurate, and trustworthy observations added much to our knowledge of Africa, and by his premature death we have lost one of its most competent explorers.

CHEMICAL, ASTRONOMICAL AND PHYSICAL SCIENCE.

Chemistry.

The report made to the Association on the state of the chemical sciences in 1832, says that the efforts of investigators were then being directed to determining with accuracy the true nature of the substances which compose the various products of the organic and inorganic kingdoms, and the exact ratios by weight which the different constituents of these substances bear to each other.

But since that day the science of chemistry has far extended its boundaries. The barrier has vanished which was supposed to separate the products of living organisms from the substances of which minerals consist, or which could be formed in the laboratory. The number of distinct carbon compounds obtainable from organisms has greatly increased; but it is small when compared with the number of such compounds which have been artificially formed. The methods of analysis have been perfected. The physical, and especially the optical, properties of the various forms of matter have been closely studied, and many fruitful generalisations have been made. The form in which these generalisations would now be stated may probably change, some, perhaps, by the overthrow or disuse of an ingenious guess at nature's workings, but more by that change which is the ordinary growth of science—namely, inclusion in some simpler and more general view.

In these advances the chemist has called the spectroscope to his aid. Indeed, the existence of the British Association has been practically coterminous with the comparatively newly developed science of spectrum analysis, for though Newton,[1] Wollaston, Fraunhofer, and Fox Talbot had worked at the subject long ago, it was not till Kirchhoff and Bunsen set a seal on the prior labours of Stokes, Angstrom, and Balfour Stewart that the spectra of terrestrial elements have been mapped out and grouped; that by its help new elements have been discovered,

[1] Joannes Marcus Marci, of Kronland in Bohemia, was the only predecessor of Newton who had any knowledge of the formation of a spectrum by a prism. He not only observed that the coloured rays diverged as they left the prism, but that a coloured ray did not change in colour after transmission through a prism. His book, *Thaumantias, liber de arcu cœlesti deque colorum apparentium natura*, Prag. 1648, was, however, not known to Newton, and had no influence upon future discoveries.

and that the idea has been suggested that the various orders of spectra of the same element are due to the existence of the element in different molecular forms—allotropic or otherwise—at different temperatures.

But great as have been the advances of terrestrial chemistry through its assistance, the most stupendous advance which we owe to the spectroscope lies in the celestial direction.

Astronomy.

In the earlier part of this century, whilst the sidereal universe was accessible to investigators, many problems outside the solar system seemed to be unapproachable.

At the third meeting of the Association, at Cambridge, in 1833, Dr. Whewell said that astronomy is not only the queen of science, but the only perfect science. which was "in so elevated a state of flourishing maturity that all that remained was to determine with the extreme of accuracy the consequences of its rules by the profoundest combinations of mathematics; the magnitude of its data by the minutest scrupulousness of observation."

But in the previous year, viz. 1832, Airy, in his report to the Association on the progress of astronomy, had pointed out that the observations of the planet Uranus could not be united in one elliptic orbit; a remark which turned the attention of Adams to the discovery of Neptune. In his report on the position of optical science in 1832, Brewster suggested that with the assistance of adequate instruments "it would be possible to study the action of the elements of material bodies upon rays of artificial light, and thereby to discover the analogies between their affinities and those which produce the fixed lines in the spectra of the stars; and thus to study the effects of the combustions which light up the suns of other systems."

This idea has now been realised. All the stars which shine brightly enough to impress an image of the spectrum upon a photographic plate have been classified on a chemical basis. The close connection between stars and nebulæ has been demonstrated; and while on the one hand the modern science of thermodynamics has shown that the hypothesis of Kant and Laplace on stellar formation is no longer tenable, inquiry has indicated that the true explanation of stellar evolution is to be found in the gradual condensation of meteoritic particles, thus justifying the suggestions put forward long ago by Lord Kelvin and Prof. Tait.

We now know that the spectra of many of the terrestrial elements in the chromosphere of the sun differ from those familiar to us in our laboratories. We begin to glean the fact that the chromospheric spectra are similar to those indicated by the absorption going on in the hottest stars, and Lockyer has not hesitated to affirm that these facts would indicate that in those localities we are in the presence of the actions of temperatures sufficiently high to break up our chemical elements into finer forms. Other students of these phenomena may not agree in this view, and possibly the discrepancies may be due to default in our terrestrial chemistry. Still, I would recall to you that Dr. Carpenter, in his Presidential Address at Brighton in 1872, almost censured the speculations of Frankland and Lockyer in 1868 for attributing a certain bright line in the spectrum of solar prominences (which was not identifiable with that of any known terrestrial source of light) to a hypothetical new substance which they proposed to call "helium," because "it had not received that verification which, in the case of Crookes' search for thallium, was afforded by the actual discovery of the new metal." Ramsay has now shown that this gas is present in dense minerals on earth; but we have now also learned from Lockyer that it and other associated gases are not only found with hydrogen in the solar chromosphere, but that these gases, with hydrogen, form a large percentage of the atmospheric constituents of some of the hottest stars in the heavens.

The spectroscope has also made us acquainted with the motions and even the velocities of those distant orbs which make up the sidereal universe. It has enabled us to determine that many stars, single to the eye, are really double, and many of the conditions of these strange systems have been revealed. The rate at which matter is moving in solar cyclones and winds is now familiar to us. And I may also add that quite recently this wonderful instrument has enabled Prof. Keeler to verify Clerk Maxwell's theory that the rings of Saturn consist of a marvellous company of separate moons—as it were, a cohort of courtiers revolving round their queen—with velocities proportioned to their distances from the planet.

Physics.

If we turn to the sciences which are included under physics, the progress has been equally marked.

In optical science, in 1831, the theory of emission as contrasted with the undulatory theory of light was still under discussion.

Young, who was the first to explain the phenomena due to the interference of the rays of light as a consequence of the theory of waves, and Fresnel, who showed the intensity of light for any relative position of the interference-waves, both had only recently passed away.

The investigations into the laws which regulate the conduction and radiation of heat, together with the doctrine of latent and of specific heat, and the relations of vapour to air, had all tended to the conception of a material heat, or caloric, communicated by an actual flow and emission.

It was not till 1834 that improved thermometrical appliances had enabled Forbes and Melloni to establish the polarisation of heat, and thus to lay the foundation of an undulatory theory for heat similar to that which was in progress of acceptation for light.

Whewell's report, in 1832, on magnetism and electricity shows that these branches of science were looked upon as cognate, and that the theory of two opposite electric fluids was generally accepted.

In magnetism, the investigations of Hansteen, Gauss, and Weber in Europe, and the observations made under the Imperial Academy of Russia over the vast extent of that Empire, had established the existence of magnetic poles, and had shown that magnetic disturbances were simultaneous at all the stations of observation.

At their third meeting the Association urged the Government to establish magnetic and meteorological observatories in Great Britain and her colonies and dependencies in different parts of the earth, furnished with proper instruments, constructed on uniform principles, and with provisions for continued observations at those places.

In 1839 the British Association had a large share in inducing the Government to initiate the valuable series of experiments for determining the intensity, the declination, the dip, and the periodical variations of the magnetic needle which were carried on for several years, at numerous selected stations over the surface of the globe, under the directions of Sabine and Lefroy.

In England systematic and regular observations are still made at Greenwich, Kew, and Stonyhurst. For some years past similar observations by both absolute and self-recording instruments have also been made at Falmouth—close to the home of Robert Were Fox, whose name is inseparably connected with the early history of terrestrial magnetism in this country—but under such great financial difficulties that the continuance of the work is seriously jeopardised. It is to be hoped that means may be forthcoming to carry it on. Cornishmen, indeed, could found no more fitting memorial of their distinguished countryman, John Couch Adams, than by suitably endowing the magnetic observatory in which he took so lively an interest.

Far more extended observation will be needed before we can hope to have an established theory as to the magnetism of the earth. We are without magnetic observations over a large part of the southern hemisphere. And Prof. Rücker's recent investigations tell us that the earth seems as it were alive with magnetic forces, be they due to electric currents or to variations in the state of magnetised matter; that the disturbances affect not only the diurnal movement of the magnet, but that even the small part of the secular change which has been observed, and which has taken centuries to accomplish, is interfered with by some slower agency. And, what is more important, he tells us that none of these observations stand as yet upon a firm basis, because standard instruments have not been in accord; and much labour, beyond the power of individual effort, has hitherto been required to ascertain whether the relations between them are constant or variable.

In electricity, in 1831, just at the time when the British Association was founded, Faraday's splendid researches in electricity and magnetism at the Royal Institution had begun with his discovery of magneto-electric induction, his investigation of the laws of electro-chemical decomposition, and of the mode of electrolytical action.

But the practical application of our electrical knowledge was then limited to the use of lightning-conductors for buildings and ships. Indeed, it may be said that the applications of elec-

tricity to the use of man have grown up side by side with the British Association.

One of the first practical applications of Faraday's discoveries was in the deposition of metals and electro-plating, which has developed into a large branch of national industry; and the dissociating effect of the electric arc, for the reduction of ores, and in other processes, is daily obtaining a wider extension.

But probably the application of electricity which is tending to produce the greatest change in our mental, and even material condition, is the electric telegraph and its sister, the telephone. By their agency not only do we learn, almost at the time of their occurrence, the events which are happening in distant parts of the world, but they are establishing a community of thought and feeling between all the nations of the world which is influencing their attitude towards each other, and, we may hope, may tend to weld them more and more into one family.

The electric telegraph was introduced experimentally in Germany in 1833, two years after the formation of the Association. It was made a commercial success by Cooke and Wheatstone in England, whose first attempts at telegraphy were made on the line from Euston to Camden Town in 1837, and on the line from Paddington to West Drayton in 1838.

The submarine telegraph to America, conceived in 1856, became a practical reality in 1861 through the commercial energy of Cyrus Field and Pender, aided by the mechanical skill of Latimer Clark, Gooch, and others, and the scientific genius of Lord Kelvin. The knowledge of electricity gained by means of its application to the telegraph largely assisted the extension of its utility in other directions.

The electric light gives, in its incandescent form, a very perfect hygienic light. Where rivers are at hand the electrical transmission of power will drive railway trains and factories economically, and might enable each artisan to convert his room into a workshop, and thus assist in restoring to the labouring man some of the individuality which the factory has tended to destroy.

In 1843 Joule described his experiments for determining the mechanical equivalent of heat. But it was not until the meeting at Oxford, in 1847, that he fully developed the law of the conservation of energy, which, in conjunction with Newton's law of the conservation of momentum, and Dalton's law of the conservation of chemical elements, constitutes a complete mechanical foundation for physical science.

Who, at the foundation of the Association, would have believed some far-seeing philosopher if he had foretold that the spectroscope would analyse the constituents of the sun and measure the motions of the stars; that we should liquefy air and utilise temperatures approaching to the absolute zero for experimental research; that, like the magician in the "Arabian Nights," we should annihilate distance by means of the electric telegraph and the telephone; that we should illuminate our largest buildings instantaneously, with the clearness of day, by means of the electric current; that by the electric transmission of power we should be able to utilise the Falls of Niagara to work factories at distant places; that we should extract metals from the crust of the earth by the same electrical agency to which, in some cases, their deposition has been attributed?

These discoveries and their applications have been brought to their present condition by the researches of a long line of scientific explorers, such as Dalton, Joule, Maxwell, Helmholtz, Herz, Kelvin, and Rayleigh, aided by vast strides made in mechanical skill. But what will our successors be discussing sixty years hence? How little do we yet know of the vibrations which [illegible] light and heat! Far as we have advanced in the application of electricity to the uses of life, we know but little even yet of its real nature. We are only on the threshold of the knowledge of molecular action, or of the constitution of the all-pervading ether. Newton, at the end of the seventeenth century, in his preface to the "Principia," says: "I have [illegible] the motion of the planets by mathematical reasoning from [illegible]; and I would that we could derive the other phenomena of nature from mechanical principles by the same mode of [illegible]. For many things move me, so that I somewhat [illegible] may depend on certain forces by which the [illegible], through causes not yet known, are either [illegible] according to regular figures, or are [illegible] from each other; and these forces being [illegible], philosophers have hitherto made their attempts on [illegible] in vain."

In 1848 Faraday [illegible]: "How rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develop more and more their importance!

"A few years ago magnetism was an occult force, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relation with electricity, heat, chemical action, light, crystallisation; and through it the forces concerned in cohesion. We may feel encouraged to continuous labours, hoping to bring it into a bond of union with gravity itself."

But it is only within the last few years that we have begun to realise that electricity is closely connected with the vibrations which cause heat and light, and which seem to pervade all space—vibrations which may be termed the voice of the Creator calling to each atom and to each cell of protoplasm to fall into its ordained position, each, as it were, a musical note in the harmonious symphony which we call the universe.

Meteorology.

At the first meeting, in 1831, Prof. James D. Forbes was requested to draw up a report on the State of Meteorological Science, on the ground that this science is more in want than any other of that systematic direction which it is one great object of the Association to give.

Prof. Forbes made his first report in 1832, and a subsequent report in 1840. The systematic records now kept in various parts of the world of barometric pressure, of solar heat, of the temperature and physical conditions of the atmosphere at various altitudes, of the heat of the ground at various depths, of the rainfall, of the prevalence of winds, and the gradual elucidation not only of the laws which regulate the movements of cyclones and storms, but of the influences which are exercised by the sun and by electricity and magnetism, not only upon atmospheric conditions, but upon health and vitality, are gradually approximating meteorology to the position of an exact science.

England took the lead in rainfall observations. Mr. G. J. Symons organised the British Rainfall System in 1860 with 178 observers, a system which until 1876 received the help of the British Association. Now Mr. Symons himself conducts it, assisted by more than 3000 observers, and these volunteers not only make the observations, but defray the expense of their reduction and publication. In foreign countries this work is done by Government officers at the public cost.

At the present time a very large number of rain gauges are in daily use throughout the world. The British Islands have more than 3000, and India and the United States have nearly as many; France and Germany are not far behind; Australia probably has more—indeed, one colony alone, New South Wales, has more than 1100.

The storm warnings now issued under the excellent systematic organisation of the Meteorological Committee may be said to have had their origin in the terrible storm which broke over the Black Sea during the Crimean War, on November 27, 1855. Leverrier traced the progress of that storm, and seeing how its path could have been reported in advance by the electric telegraph, he proposed to establish observing stations which should report to the coasts the probability of the occurrence of a storm. Leverrier communicated with Airy, and the Government authorised Admiral FitzRoy to make tentative arrangements in this country. The idea was also adopted on the continent, and now there are few civilised countries north or south of the equator without a system of storm warning.[1]

Biological Science.

Botany.

The earliest Reports of the Association which bear on the biological sciences were those relating to botany.

In 1831 the controversy was yet unsettled between the advantages of the Linnean, or Artificial system, as contrasted with the Natural system of classification. Histology, morphology, and physiological botany, even if born, were in their early infancy.

Our records show that von Mohl noted cell division in 1835, the presence of chlorophyll corpuscles in 1837; and he first described protoplasm in 1846.

[1] It has often been supposed that Leverrier was also the first to issue a daily weather map, but that was not the case, for in the Great Exhibition of [illegible] the Electric Telegraph Company sold daily weather maps, copies of which are still in existence, and the data for them were, it is believed, obtained by Mr. James Glaisher, F.R.S., at that time Superintendent of the Meteorological Department at Greenwich.

Vast as have been the advances of physiological botany since that time, much of its fundamental principles remain to be worked out, and I trust that the establishment, for the first time, of a permanent Section for botany at the present meeting will lead the Association to take a more prominent part than it has hitherto done in the further development of this branch of biological science.

Animal Physiology.

In 1831 Cuvier, who during the previous generation had, by the collation of facts followed by careful inductive reasoning, established the plan on which each animal is constructed, was approaching the termination of his long and useful life. He died in 1832; but in 1831 Richard Owen was just commencing his anatomical investigations and his brilliant contributions to palæontology.

The impulse which their labours gave to biological science was reflected in numerous reports and communications, by Owen and others, throughout the early decades of the British Association, until Darwin propounded a theory of evolution which commanded the general assent of the scientific world. For this theory was not absolutely new. But just as Cuvier had shown that each bone in the fabric of an animal affords a clue to the shape and structure of the animal, so Darwin brought harmony into scattered facts, and led us to perceive that the moulding hand of the Creator may have evolved the complicated structures of the organic world from one or more primeval cells.

Richard Owen did not accept Darwin's theory of evolution, and a large section of the public contested it. I well remember the storm it produced—a storm of praise by my geological colleagues, who accepted the result of investigated facts; a storm of indignation such as that which would have burned Galileo at the stake from those who were not yet prepared to question the old authorities; but they diminish daily.

We are, however, as yet only on the threshold of the doctrine of evolution. Does not each investigation, even into the embryonic stage of the simpler forms of life, suggest fresh problems?

Anthropology.

The impulse given by Darwin has been fruitful in leading others to consider whether the same principle of evolution may not have governed the moral as well as the material progress of the human race. Mr. Kidd tells us that nature as interpreted by the struggle for life contains no sanction for the moral progress of the individual, and points out that if each of us were allowed by the conditions of life to follow his own inclination the average of each generation would distinctly deteriorate from that of the preceding one; but because the law of life is ceaseless and inevitable struggle and competition, ceaseless and inevitable selection and rejection, the result is necessarily ceaseless and inevitable progress. Evolution, as Sir William Flower said, is the message which biology has sent to help us on with some of the problems of human life, and Francis Galton urges that man, the foremost outcome of the awful mystery of evolution, should realise that he has the power of shaping the course of future humanity by using his intelligence to discover and expedite the changes which are necessary to adapt circumstances to man, and man to circumstances.

In considering the evolution of the human race, the science of preventive medicine may afford us some indication of the direction in which to seek for social improvement. One of the early steps towards establishing that science upon a secure basis was taken in 1835 by the British Association, who urged upon the Government the necessity of establishing registers of mortality showing the causes of death "on one uniform plan in all parts of the King's dominions, as the only means by which general laws touching the influence of causes of disease and death could be satisfactorily deduced." The general registration of births and deaths was commenced in 1838. But a mere record of death and its proximate cause is insufficient. Preventive medicine requires a knowledge of the details of the previous conditions of life and of occupation. Moreover, death is not our only or most dangerous enemy, and the main object of preventive medicine is to ward off disease. Disease of body lowers our useful energy. Disease of body or of mind may stamp its curse on succeeding generations.

The anthropometric laboratory affords to the student of anthropology a means of analysing the causes of weakness, not only in bodily, but also in mental life.

Mental actions are indicated by movements and their results. Such signs are capable of record, and modern physiology has shown that bodily movements correspond to action in nerve-centres, as surely as the motions of the telegraph-indicator express the movements of the operator's hands in the distant office.

Thus there is a relation between a defective status in brain power and defects in the proportioning of the body. Defects in physiognomical details, too finely graded to be measured with instruments, may be appreciated with accuracy by the senses of the observer; and the records show that these defects are, in a large degree, associated with a brain status lower than the average in mental power.

A report presented by one of your committees gives the results of observations made on 100,000 school-children examined individually in order to determine their mental and physical condition for the purpose of classification. This shows that about 16 per 1000 of the elementary school population appear to be so far defective in their bodily or brain condition as to need special training to enable them to undertake the duties of life, and to keep them from pauperism or crime.

Many of our feeble-minded children, and much disease and vice, are the outcome of inherited proclivities. Francis Galton has shown us that types of criminals which have been bred true to their kind are one of the saddest disfigurements of modern civilisation; and he says that few deserve better of their country than those who determine to lead celibate lives through a reasonable conviction that their issue would probably be less fitted than the generality to play their part as citizens.

These considerations point to the importance of preventing those suffering from transmissible disease, or the criminal, or the lunatic, from adding fresh sufferers to the teeming misery in our large towns. And in any case, knowing as we do the influence of environment on the development of individuals, they point to the necessity of removing those who are born with feeble minds, or under conditions of moral danger, from surrounding deteriorating influences.

These are problems which materially affect the progress of the human race, and we may feel sure that, as we gradually approach their solution, we shall more certainly realise that the theory of evolution, which the genius of Darwin impressed on this century, is but the first step on a biological ladder which may possibly eventually lead us to understand how in the drama of creation man has been evolved as the highest work of the Creator.

Bacteriology.

The sciences of medicine and surgery were largely represented in the earlier meetings of the Association, before the creation of the British Medical Association afforded a field for their more intimate discussion. The close connection between the different branches of science is causing a revival in our proceedings of discussions on some of the highest medical problems, especially those relating to the spread of infectious and epidemic disease.

It is interesting to contrast the opinion prevalent at the foundation of the Association with the present position of the question.

A report to the Association in 1834, by Prof. Henry, on contagion, says:—

"The notion that contagious emanations are at all connected with the diffusion of animalculæ through the atmosphere is at variance with all that is known of the diffusion of volatile contagion."

Whilst it had long been known that filthy conditions in air, earth and water fostered fever, cholera, and many other forms of disease, and that the disease ceased to spread on the removal of these conditions, yet the reason for their propagation or diminution remained under a veil.

Leeuwenhoek in 1680 described the yeast-cells, but Schwann in 1837 first showed clearly that fermentation was due to the activity of the yeast-cells; and, although vague ideas of fermentation had been current during the past century, he laid the foundation of our exact knowledge of the nature of the action of ferments, both organised and unorganised. It was not until 1860, after the prize of the Academy of Sciences had been awarded to Pasteur for his essay against the theory of spontaneous generation, that his investigations into the action of ferments[1] enabled him to show that the effects of the yeast-cell

[1] In speaking of ferments one must bear in mind that there are two classes of ferments: one, living beings, such as yeast "organised" ferments, as they are sometimes called the other the products of living beings themselves, such as pepsin, &c. "unorganised ferments. Pasteur worked with the former, very little with the latter

are indissolubly bound up with the activities of the cell as a living organism, and that certain diseases, at least, are due to the action of ferments in the living being. In 1865 he showed that the disease of silkworms, which was then undermining the silk industry in France, could be successfully combated. His further researches into anthrax, fowl cholera, swine fever, rabies, and other diseases, proved the theory that those diseases are connected in some way with the introduction of a microbe into the body of an animal; that the virulence of the poison can be diminished by cultivating the microbes in an appropriate manner; and that when the virulence has been thus diminished their inoculation will afford a protection against the disease.

Meanwhile it had often been observed in hospital practice that a patient with a simple-fractured limb was easily cured, whilst a patient with a compound fracture often died from the wound. Lister was thence led, in 1865, to adopt his antiseptic treatment, by which the wound is protected from hostile microbes.

This investigation, followed by the discovery of the existence of a multitude of micro-organisms and the recognition of some of them—such as the bacillus of tubercle and the comma bacillus of cholera—as essential factors of disease; and by the elaboration of Koch and others of methods by which the several organisms might be isolated, cultivated, and their histories studied, have gradually built up the science of bacteriology. Amongst later developments are the discovery of various so-called antitoxins, such as those of diphtheria and tetanus, and the utilisation of these for the cure of disease. Lister's treatment formed a landmark in the science of surgery, and enabled our surgeons to perform operations never before dreamed of; whilst later discoveries are tending to place the practice of medicine on a firm scientific basis. And the science of bacteriology is leading us to recur to stringent rules for the isolation of infectious disease, and to the disinfection (by superheated steam) of materials which have been in contact with the sufferer.

These microbes, whether friendly or hostile, are all capable of multiplying at an enormous rate under favourable conditions. They are found in the air, in water, in the soil; but, fortunately, the presence of one species appears to be detrimental to other species, and sunshine, or even light from the sky, is prejudicial to most of them. Our bodies, when in health, appear to be furnished with special means of resisting attack, and, so far as regards their influence in causing disease, the success of the attack of a pathogenic organism upon an individual depends, as a rule, in part at least, upon the power of resistance of the individual.

But notwithstanding our knowledge of the danger arising from a state of low health in individuals, and of the universal prevalence of these micro-organisms, how careless we are in guarding the health conditions of every-day life! We have ascertained that pathogenic organisms pervade the air. Why, therefore, do we allow our meat, our fish, our vegetables, our easily contaminated milk, to be exposed to their inroads, often in the foulest localities? We have ascertained that they pervade the water we drink, yet we allow foul water from our dwellings, our pigsties, our farmyards, to pass into ditches without previous clarification, whence it flows into our streams and pollutes our rivers. We know the conditions of occupation which foster ill-health. Why, whilst we remove outside sources of impure air, do we permit the occupation of foul and unhealthy dwellings?

The study of bacteriology has shown us that although some of these organisms may be the accompaniments of disease, yet we owe it to the operation of others that the refuse caused by the cessation of animal and vegetable life is reconverted into food for fresh generations of plants and animals.

These considerations have formed a point of meeting where the biologist, the chemist, the physicist, and the statistician unite with the sanitary engineer in the application of the science of preventive medicine.

ENGINEERING.

Sewage Purification.

The early reports to the Association show that the laws of hydrostatics, hydrodynamics, and hydraulics necessary to the supply and removal of water through pipes and conduits had long been investigated by the mathematician. But the modern sanitary engineer has been driven by the needs of an increasing population to call in the chemist and the biologist to help him to provide pure water and pure air.

The purification and the utilisation of sewage occupied the attention of the British Association as early as 1864, and between 1869 and 1876 a committee of the Association made a series of valuable reports on the subject. The direct application of sewage to land, though effective as a means of purification, entailed difficulties in thickly settled districts, owing to the extent of land required.

The chemical treatment of sewage produced an effluent harmless only after having been passed over land, or if turned into a large and rapid stream, or into a tidal estuary; and it left behind a large amount of sludge to be dealt with.

Hence it was long contended that the simplest plan in favourable localities was to turn the sewage into the sea, and that the consequent loss to the land of the manurial value in the sewage would be recouped by the increase in fish-life.

It was not till the chemist called to his aid the biologist, and came to the help of the engineer, that a scientific system of sewage purification was evolved.

Dr. Frankland many years ago suggested the intermittent filtration of sewage; and Mr. Baldwin Latham was one of the first engineers to adopt it. But the valuable experiments made in recent years by the State Board of Health in Massachusetts have more clearly explained to us how by this system we may utilise micro-organisms to convert organic impurity in sewage into food fitted for higher forms of life.

To effect this we require, in the first place, a filter about five feet thick of sand and gravel, or, indeed, of any material which affords numerous surfaces or open pores. Secondly, that after a volume of sewage has passed through the filter, an interval of time be allowed, in which the air necessary to support the life of the micro-organisms is enabled to enter the pores of the filter. Thus this system is dependent upon oxygen and time. Under such conditions the organisms necessary for purification are sure to establish themselves in the filter before it has been long in use. Temperature is a secondary consideration.

Imperfect purification can invariably be traced either to a lack of oxygen in the pores of the filter, or to the sewage passing through so quickly that there is not sufficient time for the necessary processes to take place. And the power of any material to purify either sewage or water depends almost entirely upon its ability to hold a sufficient proportion of either sewage or water in contact with a proper amount of air.

Smoke Abatement.

Whilst the sanitary engineer has done much to improve the surface conditions of our towns, to furnish clean water, and to remove our sewage, he has as yet done little to purify town air. Fog is caused by the floating particles of matter in the air becoming weighted with aqueous vapour; some particles, such as salts of ammonia or chloride of sodium, have a greater affinity for moisture than others. You will suffer from fog so long as you keep refuse stored in your towns to furnish ammonia, or so long as you allow your street surfaces to supply dust, of which much consists of powdered horse manure, or so long as you send the products of combustion into the atmosphere. Therefore, when you have adopted mechanical traction for vehicles in your towns, you may largely reduce one cause of fog. And if you diminish your black smoke, you will diminish black fogs.

In manufactories you may prevent smoke either by care in firing, by using smokeless coal, or by washing the soot out of the products of consumption in its passage along the flue leading to the main chimney-shaft.

The black smoke from your kitchen may be avoided by the use of coke or of gas. But so long as we retain the hygienic arrangement of the open fire in our living-rooms I despair of finding a fireplace, however well constructed, which will not be used in such a manner as to cause smoke, unless, indeed, the chimneys were reversed and the fumes drawn into some central shaft, where they might be washed before being passed into the atmosphere.

Electricity as a warming and cooking agent would be convenient, cleanly, and economical when generated by water power, or possibly wind power, but it is at present too dear when it has to be generated by means of coal. I can conceive, however, that our descendants may learn so to utilise electricity that they in some future century may be enabled by its means to avoid the smoke in their towns.

Mechanical Engineering.

In other branches of civil and mechanical engineering, the reports in 1831 and 1832 on the state of this science show that

the theoretical and practical knowledge of the strength of timber had obtained considerable development. But in 1830, before the introduction of railways, cast iron had been sparingly used in arched bridges for spans of from 160 to 200 feet, and wrought iron had only been applied to large-span iron bridges on the suspension principle, the most notable instance of which was the Menai Suspension Bridge, by Telford. Indeed, whilst the strength of timber had been patiently investigated by engineers, the best form for the use of iron girders and struts was only beginning to attract attention, and the earlier volumes of our *Transactions* contained numerous records of the researches of Eaton Hodgkinson, Barlow, Rennie, and others. It was not until twenty years later that Robert Stephenson and William Fairbairn erected the tubular bridge at Menai, followed by the more scientific bridge erected by Brunel at Saltash. These have now been entirely eclipsed by the skill with which the estuary of the Forth has been bridged with a span of 1700 feet by Sir John Fowler and Sir Benjamin Baker.

The development of the iron industry is due to the association of the chemist with the engineer. The introduction of the hot blast by Neilson, in 1829, in the manufacture of cast iron had effected a large saving of fuel. But the chemical conditions which affect the strength and other qualities of iron, and its combinations with carbon, silicon, phosphorus, and other substances, had at that time scarcely been investigated.

In 1856 Bessemer brought before the British Association at Cheltenham his brilliant discovery for making steel direct from the blast furnace, by which he dispensed with the laborious process of first removing the carbon from pig-iron by puddling, and then adding by cementation the required proportion of carbon to make steel. This discovery, followed by Siemens's regenerative furnace, by Whitworth's compressed steel, and by the use of alloys and by other improvements too numerous to mention here, have revolutionised the conditions under which metals are applied to engineering purposes.

Indeed, few questions are of greater interest, or possess more industrial importance, than those connected with metallic alloys. This is especially true of those alloys which contain the rarer metals; and the extraordinary effects of small quantities of chromium, nickel, tungsten and titanium on certain varieties of steel have exerted profound influence on the manufacture of projectiles and on the construction of our armoured ships.

Of late years, investigations on the properties and structure of alloys have been numerous, and among the more noteworthy researches may be mentioned those of Dewar and Fleming on the distinctive behaviour, as regards the thermo-electric powers and electrical resistance, of metals and alloys at the very low temperatures which may be obtained by the use of liquid air.

Prof. Roberts-Austen, on the other hand, has carefully studied the behaviour of alloys at very high temperatures, and by employing his delicate pyrometer has obtained photographic curves which afford additional evidence as to the existence of allotropic modifications of metals, and which have materially strengthened the view that alloys are closely analogous to saline solutions. In this connection it may be stated that the very accurate work of Heycock and Neville on the lowering of the solidifying points of molten metals, which is caused by the presence of other metals, affords a valuable contribution to our knowledge.

Prof. Roberts-Austen has, moreover, shown that the effect of any one constituent of an alloy upon the properties of the principal metal has a direct relation to the atomic volumes, and that it is consequently possible to foretell, in a great measure, the effect of any given combination.

A new branch of investigation, which deals with the microstructure of metals and alloys, is rapidly assuming much importance. It was instituted by Sorby in a communication which he made to the British Association in 1864, and its development is due to many patient workers, among whom M. Osmond occupies a prominent place.

Metallurgical science has brought aluminium into use by cheapening the process of its extraction; and if by means of the wasted forces in our rivers, or possibly of the wind, the extraction be still further cheapened by the aid of electricity, we may not only utilise the metal or its alloys in increasing the spans of our bridges, and in affording strength and lightness in the construction of our ships, but we may hope to obtain a material which may render practicable the dreams of Icarus and of Maxim, and for purposes of rapid transit enable us to navigate the air.

Long before 1831 the steam-engine had been largely used on rivers and lakes, and for short sea passages, although the first Atlantic steam-service was not established till 1838.

As early as 1820 the steam-engine had been applied by Gurney, Hancock, and others to road traction. The absurd impediments placed in their way by road trustees, which, indeed, are still enforced, checked any progress. But the question of mechanical traction on ordinary roads was practically shelved in 1830, at the time of the formation of the British Association, when the locomotive engine was combined with a tubular boiler and an iron road on the Liverpool and Manchester Railway.

Great, however, as was the advance made by the locomotive engine of Robert Stephenson, these earlier engines were only toys compared with the compound engines of to-day which are used for railways, for ships, or for the manufacture of electricity. Indeed, it may be said that the study of the laws of heat, which have led to the introduction of various forms of motive power, are gradually revolutionising all our habits of life.

The improvements in the production of iron, combined with the developed steam-engine, have completely altered the conditions of our commercial intercourse on land; whilst the changes caused by the effects of these improvements in shipbuilding, and on the ocean carrying trade, have been, if anything, still more marked.

At the foundation of the Association all ocean ships were built by hand, of wood, propelled by sails and manœuvred by manual labour; the material limited their length, which did not often exceed 100 feet, and the number of English ships of over 500 tons burden was comparatively small.

In the modern ships steam power takes the place of manual labour. It rolls the plates of which the ship is constructed, bends them to the required shape, cuts, drills, and rivets them in their place. It weighs the anchor; it propels the ship in spite of winds or currents; it steers, ventilates, and lights the ship when on the ocean. It takes the cargo on board and discharges it on arrival.

The use of iron favours the construction of ships of a large size, of forms which afford small resistance to the water, and with compartments which make the ships practically unsinkable in heavy seas, or by collision. Their size, the economy with which they are propelled, and the certainty of their arrival, cheapens the cost of transport.

The steam-engine, by compressing air, gives us control over the temperature of cool chambers. In these not only fresh meat, but the delicate produce of the Antipodes, is brought across the ocean to our doors without deterioration.

Whilst railways have done much to alter the social conditions of each individual nation, the application of iron and steam to our ships is revolutionising the international commercial conditions of the world; and it is gradually changing the course of our agriculture, as well as of our domestic life.

But great as have been the developments of science in promoting the commerce of the world, science is asserting its supremacy even to a greater extent in every department of war. And perhaps this application of science affords at a glance, better than almost any other, a convenient illustration of the assistance which the chemical, physical, and electrical sciences are affording to the engineer.

The reception of warlike stores is not now left to the uncertain judgment of "practical men," but is confided to officers who have received a special training in chemical analysis, and in the application of physical and electrical science to the tests by which the qualities of explosives, of guns, and of projectiles can be ascertained.

For instance, take explosives. Till quite recently black and brown powders alone were used, the former as old as civilisation, the latter but a small modern improvement adapted to the increased size of guns. But now the whole family of nitro-explosives are rapidly superseding the old powder. These are the direct outcome of chemical knowledge; they are not mere chance inventions, for every improvement is based on chemical theories, and not on random experiment.

The construction of guns is no longer a haphazard operation. In spite of the enormous forces to be controlled and the sudden violence of their action, the researches of the mathematician have enabled the just proportions to be determined with accuracy; the labours of the physicist have revealed the internal conditions of the materials employed, and the best means of their favourable employment. Take, for example, Longridge's coiled-wire system, in which each successive layer of which the gun is formed receives the exact proportion of tension which enables

all the layers to act in unison. The chemist has rendered it clear that even the smallest quantities of certain ingredients are of supreme importance in affecting the tenacity and trustworthiness of the materials.

The treatment of steel to adapt it to the vast range of duties it has to perform is thus the outcome of patient research. And the use of the metals—manganese, chromium, nickel, molybdenum—as alloys with iron has resulted in the production of steels possessing varied and extraordinary properties. The steel required to resist the conjugate stresses developed, lightning fashion, in a gun necessitates qualities that would not be suitable in the projectile which that gun hurls with a velocity of some 2500 feet per second against the armoured side of a ship. The armour, again, has to combine extreme superficial hardness with great toughness, and during the last few years these qualities are sought to be attained by the application of the cementation process for adding carbon to one face of the plate, and hardening that face alone by rapid refrigeration.

The introduction of quick-firing guns from ·303 (*i.e.* about one-third) of an inch to 6-inch calibre has rendered necessary the production of metal cartridge-cases of complex forms drawn cold out of solid blocks or plate of the material; this again has taxed the ingenuity of the mechanic in the device of machinery, and of the metallurgist producing a metal possessed of the necessary ductility and toughness. The cases have to stand a pressure at the moment of firing of as much as twenty-five tons to the square inch—a pressure which exceeds the ordinary elastic limits of the steel of which the gun itself is composed.

There is nothing more wonderful in practical mechanics than the closing of the breech openings of guns, for not only must they be gas-tight at these tremendous pressures, but the mechanism must be such that one man by a single continuous movement shall be able to open or close the breech of the largest gun in some ten or fifteen seconds.

The perfect knowledge of the recoil of guns has enabled the recoil of the discharge to be utilised in compressing air or springs by which guns can be raised from concealed positions in order to deliver their fire, and then made to disappear again for loading; or the same force has been used to run up the guns automatically immediately after firing, or, as in the case of the Maxim gun, to deliver in the same way a continuous stream of bullets at the rate of ten in one second.

In the manufacture of shot and shell cast iron has been almost superseded by cast and wrought steel, though the hardened Palliser projectiles still hold their place. The forged-steel projectiles are produced by methods very similar to those used in the manufacture of metal cartridge-cases, though the process is carried on at a red heat and by machines much more powerful.

In every department concerned in the production of warlike stores electricity is playing a more and more important part. It has enabled the passage of a shot to be followed from its seat in the gun to its destination.

In the gun, by means of electrical contacts arranged in the bore, a time-curve of the passage of the shot can be determined. From this the mathematician constructs the velocity-curve, and from this, again, the pressures producing the velocity are determined, and used to check the same indications obtained by other means. The velocity of the shot after it has left the gun is easily ascertained by the Boulangé apparatus.

Electricity and photography have been laid under contribution for [illegible] records of the flight of projectiles and the effects of [illegible] at the moment of their occurrence. Many of you [illegible] Mr. Vernon Boys' marvellous photographs showing [illegible] of the shot driving before it waves of air in its [illegible].

[illegible] and photography also record the properties of [illegible] or alloys as determined by curves of cooling.

[illegible] with which electrical energy can be converted [illegible] has been taken advantage of for the firing of [illegible] in their turn can, by the same agency, be laid on [illegible] of range finders placed at a distance and in [illegible] positions; while the electric light is [illegible] at night, as well as to search out [illegible].

[illegible] of the glow-lamp, the brightness of the [illegible] that the light is not due to combustion, [illegible] of Dr. [illegible] to the examination [illegible] the interior of shells, and other similar [illegible] threat of a [illegible].

INFLUENCE OF INTERCOMMUNICATION AFFORDED BY THE BRITISH ASSOCIATION ON SCIENCE PROGRESS.

The advances in engineering which have produced the steam-engine, the railway, the telegraph, as well as our engines of war, may be said to be the result of commercial enterprise rendered possible only by the advances which have taken place in the several branches of science since 1831. Having regard to the intimate relations which the several sciences bear to each other, it is abundantly clear that much of this progress could not have taken place in the past, nor could further progress take place in the future, without intercommunication between the students of different branches of science.

The founders of the British Association based its claims to utility upon the power it afforded for this intercommunication. Mr. Vernon Harcourt (the uncle of your present General Secretary), in the address he delivered in 1832, said: "How feeble is man for any purpose when he stands alone—how strong when united with other men!

"It may be true that the greatest philosophical works have been achieved in privacy, but it is no less true that these works would never have been accomplished had the authors not mingled with men of corresponding pursuits, and from the commerce of ideas often gathered germs of apparently insulated discoveries, and without such material aid would seldom have carried their investigations to a valuable conclusion."

I claim for the British Association that it has fulfilled the objects of its founders, that it has had a large share in promoting intercommunication and combination.

Our meetings have been successful because they have maintained the true principles of scientific investigation. We have been able to secure the continued presence and concurrence of the master-spirits of science. They have been willing to sacrifice their leisure, and to promote the welfare of the Association, because the meetings have afforded them the means of advancing the sciences to which they are attached.

The Association has, moreover, justified the views of its founders in promoting intercourse between the pursuers of science, both at home and abroad, in a manner which is afforded by no other agency.

The weekly and sessional reunions of the Royal Society, and the annual soirées of other scientific societies, promote this intercourse to some extent, but the British Association presents to the young student during its week of meetings easy and continuous social opportunities for making the acquaintance of leaders in science, and thereby obtaining their directing influence.

It thus encourages, in the first place, opportunities of combination, but, what is equally important, it gives at the same time material assistance to the investigators whom it thus brings together.

The reports on the state of science at the present time, as they appear in the last volume of our *Transactions*, occupy the same important position, as records of science progress, as that occupied by those reports in our earlier years. We exhibit no symptom of decay.

SCIENCE IN GERMANY FOSTERED BY THE STATE AND MUNICIPALITIES.

Our neighbours and rivals rely largely upon the guidance of the State for the promotion of both science teaching and of research. In Germany the foundations of technical and industrial training are laid in the Realschulen, and supplemented by the Higher Technical Schools. In Berlin that splendid institution, the Royal Technical High School, casts into the shade the facilities for education in the various Polytechnics which we are now establishing in London. Moreover, it assists the practical workman by a branch department, which is available to the public for testing building materials, metals, paper, oil, and other matters. The standards of all weights and measures used in trade can be purchased from or tested by the Government Department for Weights and Measures.

For developing pure scientific research and for promoting new applications of science to industrial purposes the German Government, at the instance of von Helmholtz, and aided by the munificence of Werner von Siemens, created the Physikalische Reichsanstalt at Charlottenburg.

This establishment consists of two divisions. The first is charged with pure research, and is at the present time engaged in various thermal, optical, and electrical and other physical investigations. The second branch is employed in operations of delicate standardising to assist the wants of research students—

for instance, dilatation, electrical resistances, electric and other forms of light, pressure gauges, recording instruments, thermometers, pyrometers, lenses, tuning-forks, glass, oil-testing apparatus, viscosity of glycerine, &c.

Dr. Kohlrausch succeeded Helmholtz as president, and takes charge of the first division. Prof. Hagen, the director under him, has charge of the second division. A professor is in charge of each of the several sub-departments. Under these are various subordinate posts, held by younger men, selected for previous valuable work, and usually for a limited time.

The general supervision is under a Council consisting of a president, who is a Privy Councillor, and twenty-four members, including the president and director of the Reichsanstalt; of the other members, about ten are professors or heads of physical and astronomical observatories connected with the principal universities in Germany. Three are selected from leading firms in Germany representing mechanical, optical, and electric science, and the remainder are principal scientific officials connected with the Departments of War and Marine, the Royal Observatory at Potsdam, and the Royal Commission for Weights and Measures.

This Council meets in the winter, for such time as may be necessary, for examining the research work done in the first division during the previous year, and for laying down the scheme for research for the ensuing year; as well as for suggesting any requisite improvements in the second division. As a consequence of the position which science occupies in connection with the State in continental countries, the services of those who have distinguished themselves either in the advancement or in the application of science are recognised by the award of honours; and thus the feeling for science is encouraged throughout the nation.

Assistance to Scientific Research in Great Britain.

Great Britain maintained for a long time a leading position among the nations of the world by virtue of the excellence and accuracy of its workmanship, the result of individual energy; but the progress of mechanical science has made accuracy of workmanship the common property of all nations of the world. Our records show that hitherto, in its efforts to maintain its position by the application of science and the prosecution of research, England has made marvellous advances by means of voluntary effort, illustrated by the splendid munificence of such men as Gassiot, Joseph Whitworth, James Mason, and Ludwig Mond; and, whilst the increasing field of scientific research compels us occasionally to seek for Government assistance, it would be unfortunate if by any change voluntary effort were fettered by State control.

The following are the principal voluntary agencies which help forward scientific research in this country:—The Donation Fund of the Royal Society, derived from its surplus income. The British Association has contributed £60,000 to aid research since its formation. The Royal Institution, founded in the last century, by Count Rumford, for the promotion of research, has assisted the investigations of Davy, of Young, of Faraday, of Frankland, of Tyndall, of Dewar, and of Rayleigh. The City Companies assist scientific research and foster scientific education both by direct contributions and through the City and Guilds Institute. The Commissioners of the Exhibition of 1851 devote £6000 annually to science research scholarships, to enable students who have passed through a college curriculum and have given evidence of capacity for original research to continue the prosecution of science, with a view to its advance or to its application to the industries of the country. Several scientific societies, as, for instance, the Geographical Society and the Mechanical Engineers, have promoted direct research, each in their own branch of science, out of their surplus income; and every scientific society largely assists research by the publication, not only of its own proceedings, but often of the work going on abroad in the branch of science which it represents.

The growing abundance of matter year by year increases the burden thus thrown on their finances, and the Treasury has recently granted to the Royal Society £1000 a year, to be spent in aid of the publication of scientific papers not necessarily limited to those of that Society.

The Royal Society has long felt the importance to scientific research of a catalogue of all papers and publications relating to pure and applied science, arranged systematically both as to authors' names and as to subject treated, and the Society has been engaged for some time upon a catalogue of that nature. But the daily increasing magnitude of these publications, coupled with the necessity of issuing the catalogue with adequate promptitude, and at appropriate intervals, renders it a task which could only be performed under International co-operation. The officers of the Royal Society have therefore appealed to the Government to urge Foreign Governments to send delegates to a Conference to be held next July to discuss the desirability and the scope of such a catalogue, and the possibility of preparing it.

The universities and colleges distributed over the country, besides their function of teaching, are large promoters of research, and their voluntary exertions are aided in some cases by contributions from Parliament in alleviation of their expenses.

Certain executive departments of the Government carry on research for their own purposes, which in that respect may be classed as voluntary. The Admiralty maintains the Greenwich Observatory, the Hydrographical Department, and various experimental services; and the War Office maintains its numerous scientific departments. The Treasury maintains a valuable chemical laboratory for Inland Revenue, Customs, and agricultural purposes. The Science and Art Department maintains the Royal College of Science, for the education of teachers and students from elementary schools. It allows the scientific apparatus in the national museum to be used for research purposes by the professors. The Solar Physics Committee, which has carried on numerous researches in solar physics, was appointed by and is responsible to this Department. The Department also administers the Sir Joseph Whitworth engineering research scholarships. Other scientific departments of the Government are aids to research, as, for instance, the Ordnance and the Geological Surveys, the Royal Mint, the Natural History Museum, Kew Gardens, and other lesser establishments in Scotland and Ireland; to which may be added, to some extent, the Standards Department of the Board of Trade, as well as municipal museums, which are gradually spreading over the country.

For direct assistance to voluntary effort the Treasury contributes £4000 a year to the Royal Society for the promotion of research, which is administered under a board whose members represent all branches of science. The Treasury, moreover, contributes to marine biological observatories, and in recent years has defrayed the cost of various expeditions for biological and astronomical research, which in the case of the *Challenger* expedition involved very large sums of money.

In addition to these direct aids to science, Parliament, under the Local Taxation Act, handed over to the County Councils a sum, which amounted in the year 1893 to £615,000, to be expended on technical education. In many country districts, so far as the advancement of real scientific technical progress in the nation is concerned, much of this money has been wasted for want of knowledge. And whilst it cannot be said that the Government or Parliament have been indifferent to the promotion of scientific education and research, it is a source of regret that the Government did not devote some small portion of this magnificent gift to affording an object-lesson to County Councils in the application of science to technical instruction, which would have suggested the principles which would most usefully guide them in the expenditure of this public money.

Government assistance to science has been based mainly on the principle of helping voluntary effort. The Kew Observatory was initiated as a scientific observatory by the British Association. It is now supported by the Gassiot Trust Fund, and managed by the Kew Observatory Committee of the Royal Society. Observations on magnetism, on meteorology, and the record of sun-spots, as well as experiments upon new instruments for assisting meteorological, thermometrical, and photographic purposes, are being carried on there. The Committee has also arranged for the verification of scientific measuring instruments, the rating of chronometers, the testing of lenses and of other scientific apparatus. This institution carries on to a limited extent some small portion of the class of work done in Germany by that magnificent institution, the Reichsanstalt of Charlottenburg, but its development is fettered by want of funds. British students of science are compelled to resort to Berlin and Paris when they require to compare their more delicate instruments and apparatus with recognised standards. There could scarcely be a more advantageous addition to the assistance which Government now gives to science than for it to allot a substantial annual sum to the extension of the Kew Observatory, in order to develop it on the model of the Reichsanstalt. It

might advantageously retain its connection with the Royal Society, under a Committee of Management representative of the various branches of science concerned, and of all parts of Great Britain.

CONCLUSION.

The various agencies for scientific education have produced numerous students admirably qualified to pursue research; and at the same time almost every field of industry presents openings for improvement through the development of scientific methods. For instance, agricultural operations alone offer openings for research to the biologist, the chemist, the physicist, the geologist, the engineer, which have hitherto been largely overlooked. If students do not easily find employment, it is chiefly attributable to a want of appreciation for science in the nation at large.

This want of appreciation appears to arise from the fact that those who nearly half a century ago directed the movement of national education were trained in early life in the universities, in which the value of scientific methods was not at that time fully recognised. Hence our elementary, and even our secondary and great public schools, neglected for a long time to encourage the spirit of investigation which develops originality. This defect is diminishing daily.

There is, however, a more intangible cause which may have had influence on the want of appreciation of science by the nation. The Government, which largely profits by science, aids it with money, but it has done very little to develop the national appreciation for science by recognising that its leaders are worthy of honours conferred by the State. Science is not fashionable, and science students—upon whose efforts our progress as a nation so largely depends—have not received the same measure of recognition which the State awards to services rendered by its own officials, by politicians, and by the Army and by the Navy, whose success in future wars will largely depend on the effective applications of science.

The Reports of the British Association afford a complete chronicle of the gradual growth of scientific knowledge since 1831. They show that the Association has fulfilled the objects of its founders in promoting and disseminating a knowledge of science throughout the nation.

The growing connection between the sciences places our annual meeting in the position of an arena where representatives of the different sciences have the opportunity of criticising new discoveries and testing the value of fresh proposals, and the Presidential and Sectional Addresses operate as an annual stock-taking of progress in the several branches of science represented in the Sections. Every year the field of usefulness of the Association is widening. For, whether with the geologist we seek to write the history of the crust of the earth, or with the biologist to trace out the evolution of its inhabitants, or whether with the astronomer, the chemist, and the physicist we endeavour to unravel the constitution of the sun and the planets or the genesis of the nebulæ and stars which make up the universe, on every side we find ourselves surrounded by mysteries which await solution. We are only at the beginning of work.

I have, therefore, full confidence that the future records of the British Association will chronicle a still greater progress than that already achieved, and that the British nation will maintain its leading position amongst the nations of the world, if it will energetically continue its voluntary efforts to promote research, supplemented by that additional help from the Government which ought never to be withheld when a clear case of scientific utility has been established.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. W. M. HICKS, M.A., D.Sc., F.R.S., PRESIDENT OF THE SECTION.

In making a choice of subject for my address the difficulty is not one of finding material but of making selection. The field covered by this Section is a wide one. Investigation is active in every part of it, and is being rewarded with a continuous stream of new discoveries and with the growth of that coordination and correlation of facts which is the surest sign of real advancement in science. The ultimate aim of pure science is to be able to explain the most complicated phenomena of nature as flowing by the fewest possible laws from the simplest fundamental data. A statement of a law is either a confession of ignorance or a mnemonic convenience. It is the latter, if it is deducible by logical reasoning from other laws. It is the former when it is only discovered as a fact to be a law. While, on the one hand, the end of scientific investigation is the discovery of laws, on the other, science will have reached its highest goal when it shall have reduced ultimate laws to one or two, the necessity of which lies outside the sphere of our cognition. These ultimate laws—in the domain of physical science at least—will be the dynamical laws of the relations of matter to number, space, and time. The ultimate data will be number, matter, space, and time themselves. When these relations shall be known, all physical phenomena will be a branch of pure mathematics. We shall have done away with the necessity of the conception of potential energy, even if it may still be convenient to retain it; and—if it should be found that all phenomena are manifestations of motion of one single continuous medium—the idea of force will be banished also, and the study of dynamics replaced by the study of the equation of continuity.

Before, however, this can be attained, we must have the working drawings of the details of the mechanism we have to deal with. These details lie outside the scope of our bodily senses; we cannot see, or feel, or hear them, and this, not because they are unseeable, but because our senses are too coarse-grained to transmit impressions of them to our mind. The ordinary methods of investigation here fail us; we must proceed by a special method, and make a bridge of communication between the mechanism and our senses by means of hypotheses. By our imagination, experience, intuition we form theories; we deduce the consequences of these theories on phenomena which come within the range of our senses, and reject or modify and try again. It is a slow and laborious process. The wreckage of rejected theories is appalling; but a knowledge of what actually goes on behind what we can see or feel is surely if slowly being attained. It is the rejected theories which have been the necessary steps towards formulating others nearer the truth. It would be an extremely interesting study to consider the history of these discarded theories; to show the part they have taken in the evolution of truer conceptions, and to trace the persistence and modification of typical ideas from one stratum of theories to a later. I propose, however, to ask your attention for a short time to one of these special theories—or rather to two related theories—on the constitution of matter and of the ether. They are known as the vortex atom theory of matter, and the vortex sponge theory of the ether. The former has been before the scientific world for a quarter of a century, since its first suggestion by Lord Kelvin in 1867, the second for about half that time. In what I have to say I wish to take the position not of an advocate for or against, but simply as a prospector attempting to estimate what return is likely to be obtained by laying down plant to develop an unknown district. This is, in fact, the state of these two theories at present. Extremely little progress has been made in their mathematical development, and until this has been done more completely we cannot test them as to their powers of adequately explaining physical phenomena.

The theory of the rigid atom has been a very fruitful one, especially in explaining the properties of matter in the gaseous state; but it gives no explanation of the apparent forces which hold atoms together, and in many other respects it requires supplementing. The elastic solid ether explained much, but there are difficulties connected with it—especially in connection with reflection and refraction—which decide against it. The mathematical rotational ether of MacCullagh is admirably adapted to meet these difficulties, but he could give no physical conception of its mechanism. Maxwell and Faraday proposed a special ether for electrical and magnetic actions. Maxwell's identification of the latter with the luminiferous ether, his deduction of the velocity of propagation of light and of indices of refraction in terms of known electrical and magnetic constants, will form one of the landmarks in the history of science. This ether requires the same mathematical treatment as that of MacCullagh. Lord Kelvin's gyrostatic model of an ether is also of the MacCullagh type. Lastly, we have Lord Kelvin's labile ether, which again avoids the objections to the elastic solid ether. In MacCullagh's type of ether the energy of the medium when disturbed depends only on the twists produced in it. This ether has recently been mathematically discussed by Dr. Larmor, who has shown that it is adequate to explain all the various phenomena of light, electricity, and magnetism. To this I hope to return later. Meanwhile, it may be borne in mind that the vortex sponge ether belongs to MacCullagh's type.

Already before a formal theory of a fluid ether had been

attempted, Lord Kelvin ("Vortex Atoms," *Proc. Roy. Soc., Edin.*, vi. 94; *Phil. Mag.* (4), 34) had proposed his theory of vortex atoms. The permanence of a vortex filament with its infinite flexibility, its fundamental simplicity with its potential capacity for complexity, struck the scientific imagination as the thing which was wanted. Unfortunately the mathematical difficulties connected with the discussion of these motions, especially the reactions of one on another, have retarded the full development of the theory. Two objections in chief have been raised against it, viz. the difficulty of accounting for the densities of various kinds of matter, and the fact that in a vortex ring the velocity of translation decreases as the energy increases. There are two ways of dealing with a difficulty occurring in a general theory—one is to give up the theory, the other is to try and see if it can be modified to get over the difficulty. Such difficulties are to be welcomed as means of help in arriving at greater exactness in details. It is a mistake to submit too readily to crucial experiments. The very valid crucial objection of Stokes to MacCullagh's ether is a case in point. It drew away attention from a theory which, in the light of later developments, gives great hope of leading us to correct ideas. As Larmor has pointed out, this objection vanishes when we have intrinsic rotations in the ether itself. A special danger to guard against is the importation into one theory of ideas which have grown out of one essentially different. This remark has reference to the apparent difficulty of decrease of velocity with increased energy.

Maxwell was, I believe, the first to point out the difficulty of explaining the masses of the elements on the vortex atom hypothesis. To me it has always appeared one of the greatest stumbling-blocks to the acceptance of the theory. We have always been accustomed to regard the ether as of extreme tenuity, as of a density extremely though not infinitely smaller than that of gross matter, and we carry in our minds that Lord Kelvin has given an inferior limit of about 10^{-19}. There are two directions in which to seek a solution. The first is to cut the knot by supposing that the atoms of gross matter are composed of filaments whose rotating cores are of much greater density than the ether itself. The second is to remember that Lord Kelvin's number was obtained on the supposition of elastic solid ether, and does not necessarily apply to the vortex sponge. Unfortunately, however, for the first explanation, the mathematical discussion[1] shows that a ring cannot be stable unless the density of the fluid outside the core is equal to, or greater than, that inside. This instability also cannot be cured by supposing an additional circulation added outside the core. Unless, therefore, some modification of the theory can be made to secure stability this idea of dense fluid cores must be given up.

We seem, then, forced back to the conclusion that the density of the ether must be comparable with that of ordinary matter. The effective mass of any atom is not composed of that of its core alone, but also of that portion of the surrounding ether which is carried along with it as it moves through the medium. Thus a rigid sphere moving in a liquid behaves as if its mass were increased by half that of the displaced liquid. In the case of a vortex filament the ratio of effective to actual mass may be much larger. In this explanation the density of the matter composing an atom is the same for all, whilst their masses depend on their volumes and configurations combined. Now the configuration alters with the energy, and this would make the mass depend to some extent at least on the temperature. However repugnant this may be to current ideas, we are not entitled to deny its possibility, although such an effect must be small or it would have been detected. Such a variation, if it exists, is not to be looked for by means of the ordinary gravitation balance, but by the inertia or ballistic balance. The mass of the core itself remains, of course, constant, but the effective mass—that which we can measure by the mechanical effects which the moving vortex produces—is a much more complicated matter, and requires much fuller consideration than has been given to it.

The conditions of stability allow us to assume vacuous cores or cores of less density than the rest of the medium. If we do this, then the density of the ether itself may be greater than that of gross matter. Until, however, we meet with phenomena whose explanation requires this assumption, it would seem preferable to take the density everywhere the same. In this case the density of the ether must be rather less than the apparent density of the lightest of any of the elements, taking the apparent density to mean the effective mass of a vortex atom per its volume. This will probably be commensurable with the density of the matter in its most compressed state, and will lie between ·5 and 1—comparable, that is to say, with the density of water. Larmor,[1] from a special form of hypothesis for a magnetic field in the rotationally elastic ether, is led to assign a density of the same order of magnitude. If the density be given it is easy to calculate the intrinsic energy per c.c. in the medium. The velocity of propagation of light in a vortex sponge ether, as deduced by Lord Kelvin,[2] is ·47 times the mean square velocity of the intrinsic motion of the medium. This gives for the mean square velocity $6·3 \times 10^{10}$ cm. per second. If we follow Lord Kelvin and use for comparison the energy of radiation per c.c. near the sun, or say 1·8 erg per c.c., the resulting density will be 10^{-21}. The energy per c.c. in a magnetic field of 15,000 c.g.s. units is about 1 joule. If we take this for comparison we get a density of 10^{-14}. But the intrinsic energy of the fluid must be extremely great compared with the energy it has to transmit. If it were a million times greater the density would still only amount to 10^{-8}—comparable with the density of the residual gas in our highest vacua. To account for the density of gross matter on the supposition that it is built up out of the same material as the ether leads to a density between ·5 and 1. This gives the enormous energy of 10^{14} joules per c.c. In other words, the energy contained in one cubic centimetre of the ether is sufficient to raise a kilometre cube of lead 1 metre high against its weight. Thus the difficulty in explaining the mass of ordinary matter seems to reduce itself to a difficulty in believing that the ether possesses such an enormous store of energy. It may be that there are special reasons against such a large density. Larmor refers to the large forcives which would be called into play by hydrodynamical motions. Perhaps an answer to this may be found in the remark that where all the matter is of the same density the motions are kinematically deducible from the configuration at the instant, and are independent of the density. It is only where other causes act, such, *e.g.*, as indirectly depend on the mean pressure of the fluid or where vacuous spaces occur, that the actual value of the density may modify the measurable forcives.

Ever since Prof. J. J. Thomson proved that a vortex atom theory of matter is competent to serve as a basis of a kinetic theory of gases, it has been urged by various persons as a fatal objection that the translation velocity of the atoms falls off as the temperature rises. I must confess this objection has never appealed to me. Why should not the velocity fall off? The velocity of gaseous molecules has never been directly observed, nor has it been experimentally proved that it increases with rise of temperature. We have no right to import ideas based on the kinetic theory of hard discrete atoms into the totally distinct theory of mobile atoms in continuity with the medium surrounding them. Doubtless the molecules of a gas effuse through a small orifice more quickly as the temperature rises, but it is natural to suppose that a vortex ring would do the same as its energy increases. To make the objection valid, it is necessary to show that a vortex ring passing through a small tube, comparable with its own diameter, would pass through more slowly the greater its energy. It is not, however, necessarily the case that in every vortex aggregate the velocity decreases as the energy increases. The mathematical treatment of thin vortex filaments is comparatively easy, and little attention has been paid to other cases. Let us attempt to trace the life history as to translation velocity and energy of a vortex ring. We start with the energy large; the ring now has a very large aperture, and has a very thin filament. As the energy decreases the aperture becomes smaller, the filament thicker, and the velocity of translation greater. We can trace quantitatively the whole of this part of its history until the thickness of the ring has increased to about four times the diameter of the aperture, or perhaps a little further. Then the mathematical treatment employed fails us or becomes very laborious to apply. Till eighteen months ago, this was the only portion of its history we could trace. Then Prof. M. J. M. Hill ("On a Spherical Vortex," *Phil. Trans.*, 1894) published his beautiful discovery of the existence of a spherical vortex. This consists of a spherical

[1] An error in the expression on p. 768 of "Researches in the Theory of Vortex Rings," *Phil. Trans.*, pt. ii. 1885, vitiates the conclusion there drawn. If this be corrected the result mentioned above follows. See also Basset, "Treatise on Hydrodynamics," § 338, and *Amer. Jour. Math.*

[1] "A Dynamical Theory of the Electric and Luminiferous Medium," *Phil. Trans.*, 1894, A. p. 779.

[2] "On the Propagation of Laminar Motion through a Turbulently Moving Inviscid Liquid," *Phil. Mag.*, October 1887.

mass of fluid in vortical motion and moving bodily through the surrounding fluid, precisely as it it were a rigid sphere. This enables us to catch a momentary glimpse, as it were, of our vortex ring some little time after it has passed out of our ken. The aperture has gone on contracting, the ring thickening, and altering the shape of its cross section in a manner whose exact details have not yet been calculated. At last we just catch sight of it again as the aperture closes up. We find the ring has changed into a spherical ball, with still further diminished energy and increased velocity. We then lose sight of it again, but it now lengthens out, and towards the end of its course approximates to the form of a rod moving parallel to its length through the fluid with energy and velocity which again can be approximately determined. In this part of its life the velocity of translation decreases with decrease of energy. I believe it will be found, when the theory is completely worked out, that the spherical atom is the stage where this reversal of property takes place.

Even in the ring state, however, the change of velocity with energy is very small; much smaller, I think, than is generally recognised. When the energy is increased to twenty times that of the spherical vortex, the velocity is only diminished to two-thirds its previous value. If at ordinary temperatures, say 20° C., the vortex was in the spherical shape, then at 3000° C. its velocity of translation would only have been reduced to four-fifths its value at the lower temperature, whilst the aperture of the ring would have a radius about 1·4 times that of the sphere. At 2000° C. the velocity would not differ by much more than one-twentieth from its original value. In fact, near the spherical state the alteration in velocity of translation is very slow. It is therefore possible, that if the atoms of matter be vortex aggregates, the state in which we can experimentally test our theory is just that in which the mathematical discussion fails us. Other modifications tend to diminish this change of velocity. I will refer here to three only. The first is that of hollow vortices. We must not, however, postulate vacuous atoms without any rotational core at all; for in this case we should probably lose the essential property of permanence. The question has not been fully investigated, but there can be little doubt that by diminishing the energy of a completely hollow vortex we can cause it to disappear. We can certainly create one in a perfect fluid. Secondly, J. J. Thomson has shown that if a molecule be composed of linked filaments, the energy increases as the components move further apart. In such a case an extra supply of energy goes to expanding the molecule, and less, if any, to increasing the aperture. Lastly, a modification of the atomic motion to which I shall refer later, and which seems called for to explain the magnetic rotation of the plane of polarisation of light, will also tend to lessen the change of size, and therefore change of velocity with change of energy, even if it does not reverse the property.

If we pass on to consider how a vortex atom theory lends itself to the explanation of physical and chemical properties of matter independently of what may be called ether relations, we find that we owe almost all our knowledge on this point to the work of Prof. J. J. Thomson ("A Treatise on the Motion of Vortex Rings," Macmillan, 1883), which obtained the Adams' Prize in 1882. This, however, is confined to the treatment of *thin* vortex rings, still leaving a wide field for future investigations in connection with thick rings and with vortex aggregates which produce no cyclosis in the surrounding medium. His work is an extremely suggestive one. He shows that such a theory is capable not only of explaining the gaseous laws of a so-called perfect gas, but possibly also the slight deviations therefrom. Quite as striking is his explanation of chemical combination—an explanation which flows quite naturally from the theory. A vortex filament can be linked on itself: two or more can be linked together, like helices drawn on an anchor ring; [illegible], several can be arranged together like parallel rings [illegible] threading one another. In the latter case, for such [illegible] to be permanent, the strengths of each ring [illegible] and further, not more than six can thus be [illegible] together. The linked vortices will be in permanent [illegible] of their linkedness; the other arrangement [illegible] subject to no external actions. If, however, they are disturbed by the presence of other vortices they may [illegible]. When atoms are thus combined to form a [illegible] will always be dis[illegible]; the compound will be permanent when the ratio of the average paired time to the unpaired time of any atom is large. Thomson considers every filament to be of the same strength. Then an atom consisting of two links will behave like a ring of twice the strength, one of three links, of three times the strength, and so on. On this theory chemical compounds are to be regarded as systems of rings, not linked into one another but close together, and all engaged in the operation of threading each other. The conditions for permanence are: (1) the strength of each ring must be the same, (2) the number must be less than 6. Now apply this. H and Cl have equal linkings, therefore equal strength. Consequently we can have molecules of HCl, or any combinations up to 6 atoms per molecule, although the simpler one is the most likely. O has twice the linking, therefore the strength double. Hence one of H and one of O cannot revolve in permanent connection. We require first to arrange two of H together to form one system. This system has the same strength as O, they can therefore revolve in permanent connection, and we get the water molecule. *Or* we may take two of the O atoms and one of the double H molecule, and they can form a triple system of three rings threading one another in permanent connection, and we get the molecule H_2O_2. This short example will be sufficient to indicate how the theory gives a complete account of valency.

The energy of rings thus combined is less than when free; consequently they are stable, and the act of combination sets free energy. Further, Thomson points out that for two rings to combine their sizes must be about the same when they come into proximity; consequently combination can only occur between two limits of temperature corresponding to the energies within which the radii of both kinds of rings are near an equality.

We can easily extend Thomson's reasoning to explain the combination of two elements by the presence of a third neutral substance. Call the two elements which are to combine A and B, and the neutral substance C. The radii of A and B are to be supposed too unequal to allow them to come close enough together to combine. If now at the given temperature the C atom has a radius intermediate to those of A and B, it is more nearly equal to each than they are to one another; C picks up one of A, and after a short time drops it; A will leave C with its radius brought up (say) to closer equality with it. The same thing happens with the B atoms, and they leave C with their radii brought down to closer equality with it. The result is that A and B are brought into closer equality with one another, and if this is of sufficient amount, they can combine and do so, while C remains as before and apparently inert.

Thomson's theory of chemical combination applies only to thin rings. Something analogous may hold also for thick rings, but it is clearly inapplicable to vortex aggregates similar to that of Hill's. We are not confined, however, to this particular kind of association of vortex atoms in a molecule. For instance, I have recently found (not yet published) that one of Hill's vortices can swallow up another and retain it inside in relative equilibrium. The matter requires fuller discussion, but it seems to open up another mode of chemical combination.

A most important matter which has not yet been discussed at all is the relation between the mean energy of the vortex cores, and the energy of the medium itself when the atoms are close enough to affect each other's motions (as in a gas). The fundamental ideas are quite different from those underlying the well-known kinetic theory of gases of hard atoms. Nevertheless, many of the results must be very similar, based as both are on dynamical ideas. Whether it will avoid certain difficulties of the latter, especially those connected with the ratio of the specific heats, remains to be seen. The first desideratum is the determination of the equilibrium of energy between vortices and medium, and before this is done it is useless to speculate further in this region.

A vortex atom theory of matter carries with it the necessity of a fluid ether. If such a fluid is to transmit transversal radiations, some kind of quasi-elasticity must be produced in it. This can be done by supposing it to possess energetic rotational motions whose mean velocity is zero, within a volume whose linear dimension is small compared with the wave-length of light, but whose velocity of mean square is considerable. That an ether thus constituted is capable of transmitting transverse vibrations I showed before this Section at the Aberdeen meeting of the Association ("On the Constitution of the Luminiferous Ether on the Vortex Atom Theory," *Brit. Asso. Reports*, 1885, p. 930), by considering a medium composed of closely packed discrete small vortex rings. Lord Kelvin ("On the Vortex Theory of the Luminiferous Ether," *Brit. Asso. Reports*, 1887, p. 486, also

Phil. Mag., October 1887, p. 342) at the Manchester meeting discussed the question much more thoroughly and satisfactorily, and deduced that the velocity of propagation was $\sqrt{2/3}$ times the velocity of mean square of the turbulent motion. We can make little further progress until we know something of the arrangement of the small motions which confer the quasi-rigidity. This may be completely irregular and unsteady, or arranged in some definite order of steady motions. I am inclined to the view that the latter is nearer the truth. In this case we should expect a regular structure of small cells in which the motions are all similar. By the word cell I do not mean a small vessel bounded by walls, but a portion of the fluid in which the motion is a complete system in itself. Such a theory might be called a cell theory of the ether. The simplest type, perhaps, is to suppose the medium spaced into rectangular boxes, in each of which the motion may be specified as follows: Holding the box with one set of faces horizontal the fluid streams up in the centre of the box, then turns round, flows down the sides and up the centre again. In fact, it behaves like a Hill's vortex squeezed from a spherical into a box form. Each box has thus rotational circulation complete in itself. The six adjoining compartments have their motion the same in kind, but in the reverse direction, and so on. In this way we get continuous and energetic small motions throughout the medium, and the state is a stable one. If there is a shear, so that each cell becomes slightly rhomboidal, the rotational motions inside tend to prevent it, and thus propagate the disturbance, but the cells produce no effect on the general irrotational motion of the fluid, at least when the irrotational velocities are small compared with those of the propagation of light. In this case the rate at which the cells adjust themselves to an equilibrium position is far quicker than the rate at which this equilibrium distribution is disturbed by the gross motions. The linear dimensions of the cells must be small compared with the wave-lengths of light. They must probably be small also compared with the atoms of gross matter, which are themselves small compared with the same standard.

We may regard each cell as a dynamical system by itself, into which we pour or take away energy. This added energy will depend only on the shape into which the box is deformed. We may then, for our convenience in considering the gross motions of the medium as a whole, *i.e.* our secondary medium, regard these as interlocked systems, neglect the direct consideration of the motions inside them, but regard the energy which they absorb as a potential function for the general motion. This potential function will contain terms of two kinds, one involving the shear of the cells, and this shear will be the *same* as the rotational deformation in the secondary medium. The second will depend on alterations in the ratios of the edges of the cells (including other changes of form involving no rotations). The former will give rise to waves of transversal displacements. The second cannot be transmitted as waves, but may produce local effects. If a continuous solid be placed in such a medium, the cells will rearrange themselves so as to keep the continuity of their motions. The cells will become distorted (but without resultant shear), and a static stress will be set up. We have then to deal with the primary stuff itself, whose rotation gives a structure to the ether, and the structural ether itself. The former we may call the primary medium. The ether which can transmit transversal disturbances, and which is built up out of the first, we may call the secondary medium. Whether an atom of matter is to be considered as a vortical mass of the primary or of the secondary medium is a matter to be left open in the present state of the theory.

At the Bath meeting of this Association, I sketched out a theory of the electrical action of a fluid ether in which electrical lines of force were vortex filaments combined with an equivalent number of hollow vortices of the same vortical strength. ("A Vortex Analogue of Static Electricity," *Brit. Assoc. Rep.*, 1888, p. 577.) An electric charge on a body depended on the number of ends of filaments abutting on it, the sign being determined by the direction of rotation of the filament looked at from the body. This theory gave a complete account of electrostatic actions, both quantitatively and qualitatively, and a more speculative one as to currents and magnetism. I could only succeed in proving at that time that if the filaments were distributed according to the same laws as electric lines of force, the distribution would be one of equilibrium. Larmor ("A Dynamical Theory of the Electric and Luminiferous Medium," *Phil. Trans.*, 1894, p. 748) has recently proved that this is also the necessary distribution for any type of a rotationally elastic ether, and consequently also for this particular case. Currents along a wire were supposed to consist of the ends of filaments running along it, with disappearance of the hollow companions, the filaments producing at the same time a circulation round the wire. A magnetic field was thus to be produced by a flow of the ether, but probably with the necessary accompaniment of rotational elements in it.

This latter, however, was clearly wrong, because each kind of filament would produce a circulation in opposite directions. The correct deduction would have been to lay stress on the fact that the field is due to the motion through the stationary ether of the vortex filaments, the field being perpendicular to the filament and to its direction of motion. This motion would doubtless produce stresses in the cell-ether due to deformations of the cells, and be the proximate cause of the mechanical forces in the field. In any case, it is not difficult to show that a magnetic field cannot be due to an irrotational flow of the ether alone.[1] Such electrostatic and magnetic fields produce states of motion in the medium, but no bodily flow in it; consequently we ought not to expect an effect to be produced on the velocity of transmission of light through it.

The fundamental postulate underlying this explanation of electric action is that when two different kinds of matter are brought into contact a distribution of vortex filaments in the neighbourhood takes place, so that a larger number stretch from one to the other than in the opposite direction—the distinction between positive and negative ends being that already indicated. To see how such a distribution may be caused, let us consider each vortex atom to be composed of a vortical mass of our secondary medium or cell-structure ether. The atom is much larger than a cell, and contains practically an infinite number of them. It is a dynamical system of these cells with equilibrium of energy throughout its volume. The second atom is a dynamical system with a different equilibrium of energy. Where they come into contact there will be a certain surface rearrangement, which will show itself as a surface distribution of energy in a similar manner to that which exists between a molar collection of one kind of molecules in contact with one of another, and which shows itself in the phenomenon which we call surface tension. In the present case the effect may take place at the interface of two atomic systems in actual contact, or be a difference effect between the two interfaces of the ether and each atom when the latter are sufficiently close. The surface effect we are now considering shows itself as contact electricity.

Such a distribution of small vortex filaments, stretching from one atom to another, will tend to hold them together. We therefore get an additional cause for aggregation of atoms. This does not exclude the others already referred to. They may all act concurrently, some producing one effect, some another—one combining, perhaps, unknown primitive atoms into elements, one elements into chemical compounds, and another producing the cohesion of matter into masses.

On this theory the difference between a conductor and a dielectric is that in a dielectric the ends of the filaments cannot pass from atom to atom, possibly because the latter never come into actual contact. In a conductor, however, we are to suppose that the atomic elements can do so. When a current is flowing, a filament and its equivalent hollow stretch between two neighbouring atoms, they are pulled into contact, or their motions bring them into contact, the hollow disappears, and the rotational filament joins its two ends and sails away as a small neutral vortex ring into the surrounding medium, or returns to its function as an ether cell. The atoms being free are now pulled back to perform a similar operation for other filaments. The result is that the atoms are set into violent vibrations, causing the heating of the conductor. When, however, the metal is at absolute zero of temperature, there is no motion, the atoms are already in contact, and there is no resistance, as the observation of Dewar and Fleming tends to show. Further, as the resistance depends on the communication of motion from molecule to molecule, we should expect the electrical conduc-

[1] To prove this, consider a straight conductor moving parallel to itself and perpendicular to a uniform magnetic field. There exists a permanent potential difference between its ends. If, however, the field consists of a flow of ether, the effect is the same as if the conductor is at rest, and the direction of the magnetic field shifted through an angle. But this is the case of a conductor at rest in a field, and there is therefore no potential difference between the ends. Hence a magnetic field must consist of some structure across which the conductor cuts. A field may possibly demand a flow of the ether, but, if so, it must carry in it some structure definitely oriented at each point to the direction of flow.

tivity of a substance to march with its thermal conductivity. Again, on this theory the resistance clearly increases with increase of distance between atoms—*i.e.* with increase of temperature. On the contrary, in electrolytic conduction the same junction of filament ends is brought about, not by oscillations of molecule to molecule, but by disruption of the molecule and passage of atom to atom. In this case conduction is easier the more easily a molecule is split up, and thus resistance decreases with increase of temperature. To explain the laws of electrolysis it is only necessary to assume that the strengths of all filaments are the same. A similar hypothesis, as we have seen, lies at the basis of J. J. Thomson's explanation of chemical combination, although it is not necessarily the case that we are dealing with the same kind of filaments. It is evident that the theory easily lends itself to his views as to the mechanism of the electric discharge through gases. The *modus operandi* of the production of the mechanical forcive on a conductor carrying a current in a magnetic field and of electrodynamic induction is not clear. Probably the full explanation is to be found in the stresses produced in the ether owing to the deformation of the cells by the passage of the filaments through them. The fluid moves according to the equation of continuity without slip, and subject to the surface conditions at the conductors. This motion, however, distorts the cells, and stresses are called into play. Any theory which can explain the mechanical forcives and also Ohm's law, must, on the principles of the conservation of energy, also explain the induction of currents.

The magnetic rotation of the plane of polarisation of light does not depend on the structure of the ether, or on the magnetic field itself, but is a result of the atomic configuration of the matter in the field modified by the magnetism. It is generally recognised as caused by something in the field rotating round the direction of the magnetic lines of force. Now the vortex atom, as usually pictured, is incapable of exhibiting this property. It is, however, an interesting fact, and one which I hope to demonstrate to this Section during the meeting, that a vortex ring can have two simultaneous and independent cyclic motions—one the ordinary one, and another which is capable of producing just the action on light which shows itself as a rotation of the plane of polarisation. The motion is rather a complicated one to describe without a diagram, but an idea of its nature may be obtained by considering the case of a straight cylindrical vortex. The ordinary straight vortex consists, as every one knows, of a cylinder of fluid revolving like a solid, and surrounded by a fluid in irrotational motion. In the core the velocity increases from zero at the axis to a maximum at its surface. Thence it continuously decreases in the outer fluid as the distance increases. Everywhere the motion is in a plane perpendicular to the axis. Let us now consider a quite different kind of vortical motion. Suppose the fluid is flowing along the core like a viscous fluid through a pipe: the velocity is zero at the surface and a maximum at the axis. Everywhere it is parallel to the axis, the vortex lines are circles in planes perpendicular to the axis, and concentric with it. Since the velocity at the surface of the core is zero, the surrounding fluid is also at rest. Now superpose this motion on the previous one, and it will be found to be steady. If a short length of this vortex be supposed cut off, bent into the shape of a circle and the ends joined, we shall have very a rough idea of the compound vortex ring of which I speak. I say a very rough idea, because the actual state of motion in a ring vortex or a Hill's vortex is not quite so simple as the analogy might lead one to think.

Now a compound vortex atom of this kind is just what we want to produce rotation of the plane of polarisation of light. The light passing through such a vortex has the direction of vibration twisted in the wave front. In ordinary matter no such rotation is produced, because the various atoms are indifferently directed, and they neutralise each other's effects. Let, however, a magnetic field be produced, and they will range themselves so that, on the average, the primary[1] circulations through the apertures will point in the direction of the field. Consequently the average direction of the secondary spin will be in planes perpendicular to it, and will rotate the plane of polarisation of any light whose wave front passes them. The rotation is produced only in the light which is transmitted *through* the vortex. The rotation, therefore, is a resultant effect. In fact it is clear that in the case of refraction the optical media belong to the type in which every particle transmits the light, and not to the type in which refraction is produced by opaque bodies embedded in the ether. The atoms are only opaque if they contain vacuous cores. The question of the grip of the particles on the ether does not enter, but difference of quality—showing itself in refraction and dispersion—is due to difference in average rotational quasi-elasticity produced by the atomic circulations, and possibly absorption is due to precessional and nutational motion set up by the secondary spins. These, however, are perhaps rather vague speculations.

Instead of attempting to invent ethers, to deduce their properties from their specifications, and then seeing whether they fit in with experience, we may begin half-way. We may assume different forms for the function giving the energy of the medium when disturbed, apply general dynamical methods, and distinguish between those which are capable of explaining the phenomena we are investigating and those which are not. Invention is then called upon to devise a medium for which the desired energy-function is appropriate. This was the method applied by MacCullagh to the luminiferous ether. He obtained an algebraical form of the energy function which completely satisfied the conditions for a luminiferous ether; its essential property being that the energy depended only on the rotational displacements of its small parts. He was unable, however, to picture a stable material medium which would possess this property. We recognise now that such a medium is possible if the rotational rigidity is produced by intrinsic motions in the small parts of the medium of a gyrostatic nature. In a most masterly manner Larmor ("A Dynamical Theory of the Electric and Luminiferous Medium," *Phil. Trans.*, 1894) has recently investigated by general dynamical methods the possibility of explaining electric and magnetic phenomena by means of the same energy function. Electric lines of force are rotational filaments in the ether,[1] similar in fact to those I suggested at Bath, while a magnetic field consists of a flow of the ether. The same difficulty in accounting for electro-dynamic induction arises, but the general form of the equations for the electrodynamic and magnetic fields are the same as those generally received.

Towards the end of this paper he is led to postulate a theory of electrons whose convection through the ether constitutes an electric current. Two rotating round each other are supposed to produce the same effect as a vortex ring. The mass of ordinary matter is attributed to the electric inertia of these electrons. The electron itself is a centre or nucleus of rotational strain. If I express a doubt as to the possibility of the existence of these nuclei as specified, I do so with great diffidence.[2] Whether they can or cannot exist, however, the general results of the investigation are not affected.

Since this paper was published Larmor has read a second one on the same subject before the Royal Society, developing further his theory of the electron. The publication of this will be awaited with interest. It is impossible in an address such as this to go *seriatim* into the numerous points which he takes up and illuminates, because the mathematical treatment of the general question does not lend itself easily to oral exposition even to an audience composed of professed mathematicians. There is no doubt but that this paper has put the theory of a rotationally elastic ether—and with it that of a fluid vortex ether—on a sounder basis, and will lead to its discussion and elucidation by a wider circle of investigators.

One further class of physical phenomena yet remains, viz. those of gravitation. The ether must be capable of transmitting gravitational forces as well as electric and optical effects. Does the rotational ether give any promise of doing this? No satisfactory explanation of gravitation on any theory has yet been offered. Perhaps the least unsatisfactory is that depending on the vortex atom theory of matter ("On the Problem of Two Pulsating Spheres in a Fluid," *Proc. Camb. Phil. Soc.*, iii. p. 283), which attributes it to pulsations of hollow vortex atoms. But this necessitates that they should all pulsate with the same period and in the same phase. It is very difficult to conceive how this can happen, unless, as Larmor suggests, all matter is built

[1] "Primary" refers to the motion as usually understood; "secondary" to the superposed motion described above.

[1] The necessity that the filaments shall be in pairs does not seem to be recognised. This is, however, essential. Moreover, if the complementary terminations of the filaments between (say) a plate condenser be placed elsewhere than in the same region, the filaments between the plates must rotate as a whole; that is, an electric field would always be combined with a magnetic one.

[2] It would appear that the same results would flow if two particles oppositely electrified, *i.e.* joined by two complementary filaments, as already described, were to rotate round each other.

up of constant elements like his electrons, whose periods are necessarily all alike. It is possible that the vortex cell theory of the ether, of which I have already spoken, may suffice to explain gravitation also. The cells, besides their rotational rigidity, have, in addition, as we saw, a peculiar elasticity of form. To get an idea of how this theory may account for weight, let us suppose the simplest case where all the cells are exactly alike, and the medium is in equilibrium. Now suppose one of the cells begins to grow. It forces the medium away on all sides; the cells will be distorted in some definite way, and a strain set up. Further, this strain will be transmitted from the centre, so that the total amount across any concentric sphere will be the same. Stresses will therefore be set up in the whole medium. If a second cell begins to grow at another place it will produce also a state of strain, the total strain depending on the presence of both. The stresses called into play in the medium will produce a stress between the bodies, but it is questionable whether it would be inversely as the square of the distance. Whether it would be an attraction or repulsion can only be determined by mathematical investigation. The problem is quite determinate, though probably a very difficult one, and would be of mathematical interest quite apart from its physical importance. Since apparently the phenomena of gravitation have no direct interaction with those of light and electricity, whilst the mind rejects the possibility of two different media occupying the same space, we seem driven to look for it in an independent structure of the same medium. Such a structure is already to our hands, with its effects waiting to be determined. It may well be that it may prove to be the cause we are seeking.

The rapid survey I have attempted to make is no doubt a medley of suppositions and inferences combined with some sound deductions. This is the necessary consequence of a prospecting survey in a region whose surface has been merely scratched by pioneers. My object has been to show that this theory of an ether, based on a primitive perfect fluid, is one which shows very promising signs of being able to explain the various physical phenomena of our material universe. Probably, nay certainly, the explanations suggested are not all the true ones. Some will have to be given up, others modified with further knowledge. We cannot proceed to particularise in our secondary hypotheses until we know more about the properties of such media as we have been considering. Every special problem solved in vortex motion puts us in a position to form clearer ideas of what can and what cannot happen. The whole question of vortex aggregates and their interactions is practically untouched, and a rich field is open for mathematical investigation in this portion only of the subject. In all cases, whether a fluid ether is an actual fact or not, the results obtained will be of special interest as types of fluid motion. It is at present a subject in which the mathematicians must lead the attack. I shall have attained my object in choosing this subject for my address, if by it I can induce some of our younger mathematicians to take it up and work out its details.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. RAPHAEL MELDOLA, F.R.S., F.I.C., FOR. SEC. C.S., PRESIDENT OF THE SECTION.

The State of Chemical Science in 1851.

In order to estimate the progress of chemical science since the year 1851, when the British Association last met in this town, it will be of interest for us to endeavour to place ourselves in the position of those who took part in the proceedings of Section B on that occasion. Perhaps the best way of performing this retrograde feat will be to confront the fundamental doctrines of modern chemistry with the state of chemical theory at that period, because at any point in the history of a science the theoretical conceptions in vogue—whether these conceptions have survived to the present time or not—may be taken as the abstract summation of the facts, *i.e.* of the real and tangible knowledge existing at the period chosen as the standard of reference.

Without going too far back in time I may remind you that in 1811 the atomic theory of the chemists was grafted on to the kindred science of physics through the enunciation of the law associated with the name of Avogadro di Quaregna. The rationalising of this law had been accomplished in 1845, but the kinetic theory of gases, which had been foreshadowed by D. Bernoulli in 1738, and in later times by Herapath, Joule, and Krönig, lay buried in the archives of the Royal Society until recently unearthed by Lord Rayleigh and given to the world in 1892 under the authorship of Waterston, the legitimate discoverer. The later developments of this theory did not take place till after the last Ipswich meeting, viz. in 1857-62, by Clausius, and by Clerk Maxwell in 1860-67. Thus the kinetic theory of gases of the physicists had not in 1851 acquired the full significance for chemists which it now possesses: the hypothesis of Avogadro was available, analogous conceptions had been advanced by Davy in 1812, and by Ampere in 1814; but no substantial chemical reasons for its adoption were adduced until the year 1846, when Laurent published his work on the law of even numbers of atoms and the nature of the elements in the free state (*Ann. Chim. Phys.* [3], 18, 266).

The so-called "New Chemistry" with which students of the present time are familiar was, in fact, being evolved about the period when the British Association last assembled at Ipswich; but it was not till some years later, and then chiefly through the writings of Laurent and Gerhardt, that the modern views became accepted. It is of interest to note in passing that the nomenclature of organic compounds formed the subject of a report by Dr. Daubeny at that meeting in which he says:—"It has struck me as a matter of surprise that none of the British treatises on chemistry with which I am acquainted should contain any rules to guide us, either in affixing names to substances newly discovered or in divining the nature and relations of bodies from the appellations attached to them. Nor do I find this deficiency supplied in a manner which to me appears satisfactory when I turn to the writings of continental chemists." In a subsequent portion of the report Dr. Daubeny adds:—"No name ought, for the sake of convenience, to exceed in length six or seven syllables." I am afraid the requirements of modern organic chemistry have not enabled us to comply with this condition.

Among other physical discoveries which have exerted an important influence on chemical theory the law of Dulong and Petit, indicating the relationship between specific heat and atomic weight, had been announced in 1819, had been subsequently extended to compounds by Neumann, and still later had been placed upon a sure basis by the classical researches of Regnault in 1839. But here, again, it was not till after 1851 that Cannizzaro (1858) gave this law the importance which it now possesses in connection with the determination of atomic weights. Thermo-chemistry as a distinct branch of our science may also be considered to have arisen since 1851, although the foundations were laid before this period by the work of Favre and Silbermann, Andrews, Graham, and especially Hess, whose important generalisation was announced in 1840, and whose claim to just recognition in the history of physical chemistry has been ably advocated in recent times by Ostwald. But the elaboration of thermo-chemical facts and views in the light of the dynamical theory of heat was first commenced in 1853 by Julius Thomsen, and has since been carried on concurrently with the work of Berthelot in the same field which the latter investigator entered in 1865. Electro-chemistry in 1851 was in an equally rudimentary condition Davy had published his electro-chemical theory in 1807, and in 1812 Berzelius had put forward those views on electric affinity which became the basis of his dualistic system of formulation. In 1833 Faraday announced his famous law of electro-chemical equivalence, which gave a fatal blow to the conception of Berzelius, and which later (1839-40) was made use of by Daniell in order to show the untenability of the dualistic system. By 1851 the views of Berzelius had been abandoned, and, so far as chemical theory is concerned, the whole subject may be considered to have been in abeyance at that time. It is of interest to note, however, that in that year Williamson advanced on quite distinct grounds his now well-known theory of atomic interchange between molecules, which theory in a more extended form was developed independently from the physical side and applied to electrolytes by Clausius in 1857 The modern theory of electrolysis associated with the names of Arrhenius, van 't Hoff, and Ostwald is of comparatively recent growth. It appears that Hittorf in 1878 was the first to point out the relationship between electrolytic conductivity and chemical activity, this same author as far back as 1856 having combated the prevailing view that the electric current during electrolysis does the work of overcoming the affinities of the ions. Arrhenius formulated his theory of electrolytic dissociation in

1887, Planck having almost simultaneously arrived at similar views on other grounds.

Closely connected with electrolysis is the question of the constitution of solutions, and here again a convergence of work from several distinct fields has led to the creation of a new branch of physical chemistry which may be considered a modern growth. The relationship between the strength of a solution and its freezing point had been discovered by Blagden towards the end of the last century, but in 1851 chemists had no notion that this observation would have any influence on the future development of their science. Another decade elapsed before the law was rediscovered by Rüdorff (1861), and ten years later was further elaborated by de Coppet. Raoult published his first work on the freezing point of solutions in 1882, and two years later the relationship between osmotic pressure and the lowering of freezing point was established by H. de Vries, who first approached the subject as a physiologist, through observations on the cell contents of living plants. As the work done in connection with osmotic pressure has had such an important influence on the "dissociation" theory of solutions, it will be of interest to note that at the last Ipswich meeting Thomas Graham made a communication on liquid diffusion, in which he "gave a view of some of the unpublished results, to ascertain whether solutions of saline bodies had a power of diffusion among liquids, especially water." In 1877 Pfeffer, who, like de Vries, entered the field from the botanical-physiological side, succeeded in effecting the measurement of osmotic pressure. Ten years later van 't Hoff formulated the modern dissociation theory of solution by applying to dissolved substances the laws of Boyle, Gay-Lussac, and Avogadro, the law of osmotic pressure, and Raoult's law connecting the depression of freezing point with molecular weight, thus laying the foundation of a doctrine which, whether destined to survive in its present form or not, has certainly exerted a great influence on contemporary chemical thought.

Consider, further, the state of knowledge in 1851 concerning such leading principles as dissociation or thermolysis, mass action, and chemical equilibrium. Abnormal vapour densities had been observed by Avogadro in 1811, and by Ampère in 1814. Grove had dissociated water vapour by heat in 1847, but the first great advance was made ten years later by Sainte-Claire Deville, from whose work has emanated our existing knowledge of this subject. I may add that the application of this principle to explain the cases of abnormal vapour density was made in 1858 by Kopp, Kekulé, and Cannizzaro almost simultaneously; but, strangely enough, this explanation was not accepted by Deville himself. The subsequent stages are subjects of modern history. The current views on mass action were foreshadowed, as is well known, by Berthollet in his "Statique Chimique," published in 1803, but no great advance had been made when the British Association last met here. The subject first began to assume a quantitative aspect through the researches of Bunsen and Debus in 1853, and was much advanced by Gladstone in 1865 and by Harcourt and Esson a year later. Guldberg and Waage published their classical work on this subject in 1867.

Equally striking will appear the advances made since 1851 if we consider that the whole subject of spectrum analysis, which brings our science into relationship with astronomy, has been called into existence since that date. The celebrated work of Bunsen and Kirchhoff was not published till 1859. Neither can I refrain from reminding you that the coal-tar colour industry, with which I have been to a small extent connected, was started into activity by Perkin's discovery of mauve in 1856; the reaction of this industry on the development of organic chemistry is now too well known to require further mention. In that great domain also which brings chemistry into relationship with biology the progress has been so great that it is not going beyond the limits of accuracy to state that a new science has been created. Pasteur began his studies on fermentation in 1857, and out of that work has arisen the science of bacteriology, with its multifarious and far-reaching consequences. As this chapter of chemical history forms the subject of one of the evening discourses at the present meeting, it is unnecessary to dwell further upon it now. One other generalisation may be chronicled among the great developments achieved since 1851. I refer to the periodic law connecting the atomic weights of the chemical elements with their physical and chemical properties. Attempts to establish numerical relationships in the case of isolated groups of elements had been made by Döbereiner in 1817, by Gmelin in 1826, and again by Döbereiner in 1829. The triad system of grouping was further developed by Dumas in 1851. I am informed by Dr. Gladstone that at the last Ipswich meeting Dumas' speculations in this direction excited much interest. All the later steps of importance have, however, been made since that time, viz. by de Chancourtois in 1862, the "law of octaves" by Newlands in 1864, the periodic law by Mendeléeff, and almost contemporaneously by Lothar Meyer in 1869.

I have been tempted into giving this necessarily fragmentary and possibly tedious historical sketch because it is approaching half a century since the British Association visited this town, and the opportunity seemed favourable for going through that process which in commercial affairs is called "taking stock." The result speaks for itself. Our students of the present time who are nourished intellectually by these doctrines should be made to realise how rapid has been their development. The pioneers of our science, on whose shoulders we stand—and many of whom are happily still among us—will derive satisfaction from the retrospect, and will admit that their labours have borne goodly fruit. It is not, however, simply for the purpose of recording this enormous progress that I have ventured to assume the office of stock-taker. The year 1851 may be regarded as occurring towards the close of one epoch and the dawn of a new era in chemical history. Consider broadly the state of organic chemistry at that time. There is no occasion for going into detail, even if time admitted, because our literature has recently been enriched by the concise and excellent historical works of Schorlemmer and of Ernst von Meyer. It will suffice to mention that the work and writings of Liebig, Berzelius, Wöhler, Dumas, Gay-Lussac, Bunsen, and others had given us the leading ideas of isomerism, substitution, compound radicles, and types. Wurtz and Hofmann had just discovered the organic ammonias; Williamson that same year made known his celebrated work on the ethers; and Gerhardt discovered the acid anhydrides a year later. The newer theory of type was undergoing development by Gerhardt and his followers; the mature results were published in the fourth volume of the "Traité de Chimie" in 1856. In this country the theory was much advanced by the writings of Odling and Williamson.

SUBSEQUENT DEVELOPMENT OF CHEMISTRY ALONG TWO LINES.

The new era which was dawning upon us in 1851 was that of structural or constitutional chemistry, based on the doctrine of the valency of the atoms. It is well known that this conception was broached by Frankland in 1852, as the result of his investigations on the organo-metallic compounds. But it was not till 1858 that Kekulé, who had previously done much to develop the theory of types, and Couper, almost simultaneously, recognised the quadrivalent character of carbon. To attempt to give anything approaching an adequate notion of the subsequent influence of this idea on the progress of organic chemistry would be tantamount to reviewing the present condition of that subject. I imagine that no conception more prolific of results has ever been introduced into any department of science. If we glance back along the stream it will be seen that shortly after the last meeting here the course of discovery began to concentrate itself into two channels. In one we now find the results of the confluent labours of those who have regarded our science from its physical side. In the other channel is flowing the tide of discovery arising from the valency doctrine and its extension to the structure of chemical molecules. The two channels are at present fairly parallel and not far apart; an occasional explorer endeavours now and again to make a cross-cut so as to put the streams into communication. The currents in both are running very rapidly, and the worker who has embarked on one or the other finds himself hurried along at such a pace that there is hardly breathing time to step ashore and see what his neighbours are doing. It speaks well for the fertility of the conception of valency that the current in this channel is flowing with unabated vigour, although its catchment area—to pursue the metaphor—is by no means so extensive as that of the neighbouring stream.

The modern tendency to specialisation, which is a necessity arising from the large number of workers and the rapid multiplication of results, is apparently in the two directions indicated. We have one class of workers dealing with the physics of matter in relation to general chemical properties, and another class of investigators concerning themselves with the special properties of individual compounds and classes of compounds—with atomic idiosyncrasies. The workers of one class are differentiating while their colleagues are integrating. It would be nothing less than unscientific to institute a comparison between the relative

merits of the two methods; both are necessary for the development of our science. All methods of attacking the unknown are equally welcomed. In some cases physical methods are available, in other cases purely chemical methods have alone been found of use. There is no antagonism, but co-operation. If the results of the two methods are sometimes at variance it is simply because we have not known how to interpret them. The physical chemist has adopted the results of the application of chemical methods of determining "constitution," and is endeavouring to furnish us with new weapons for attacking this same problem. The chemist who is seeking to unravel the architecture of molecules is dependent at the outset upon physical methods of determining the relative weights of his molecules. The worker who is bringing about new atomic groupings is furnishing material for the further development of generalisations from which new methods applicable to the problem of chemical structure may again be evolved. The physical chemist sometimes from the broadness of his view is apt to overlook or to minimise the importance of chemical individuality. On the other hand the chemist who is studying the numberless potentialities of combination resident in the atoms, and who has grasped to the full extent their marvellous individualities, is equally liable to forget that there are connecting relationships as well as specific differences in the properties of elements and compounds. These are but the mental traits—the unconscious bias engendered by the necessary specialisation of work to which I have referred, and which is observable in every department of scientific labour.

THE PRESENT STATE OF STRUCTURAL CHEMISTRY.

The success attending the application of the doctrine of valency to the compounds of carbon has helped its extension to all compounds formed by other elements, and the student of the present day is taught to use structural formulæ as the A B C of his science. It is, I think, generally recognised among chemists that this doctrine in its present state is empirical, but it does not appear to me that this point is sufficiently insisted upon in chemical teaching. I do not mean to assert that for the last thirty years chemists have been pursuing a phantom; neither do I think that we should be justified in applying to this doctrine the words applied to its forerunner, the "types" of Gerhardt, by Lothar Meyer, who says that these "have rendered great service in the development of the science, but they can only be regarded as a part of the scaffolding which was removed when the erection of the system of organic chemistry had made sufficient progress to be able to dispense with it" ("Modern Theories of Chemistry," p. 194.) It appears to me, on the contrary, that there is a physical reality underlying the conception of valency, if for no other reason because of the conformability of this property of the atoms to the periodic law. But the doctrine as it stands is empirical in so far that it is only representative and not explanatory. Frankland and Kekulé have given us a great truth, but its very success is now making it more and more obvious that it is a truth which is pressing for further development from the physical side. If we are asked why CO exists, and why CH_2 and CCl_2 do not, together with innumerable similar questions which the inquisitive mind will raise, we get no light from this doctrine. If any over-sanguine disciple goes so far as to assert that all the possible compounds of the elements indicated by their valency are capable of existence, and will sooner or later be prepared, he will, I imagine, find himself rapidly travelling away from the region of fact.

There is something to be reckoned with besides valency. The one great desideratum of modern chemistry is unquestionably a physical or mechanical interpretation of the combining capacities of the atoms. Attempts at the construction of such theories have been made, and thus far only in a tentative way, and these views cannot be said to have yet come within the domain of practical chemical politics. I have in mind, among other suggestions, the dynamical theory of van 't Hoff published in 1881 ("Ansichten über die organische Chemie"), the theory of electric charges on the atoms broached by Johnstone Stoney in 1874, and so ably advocated by the late Prof. v. Helmholtz in his Faraday lecture in 1881, and the electric polar theory of Victor Meyer and Riecke, published in 1888 ("Einige Bemerkungen über den Kohlenstoffatom und die Valenz," *Ber.*, 21, pp. 946, 1620).

Pending the rationalisation of the doctrine of valency its promulgation must continue in its present form. Its services in the construction of rational formulæ, especially within the limits of isomerism, have been incalculable. It is the ladder by which we have climbed to the present brilliant achievements in chemical synthesis, and we are not in a position to perform the ungracious task of kicking it away. In recalling attention to its weaknesses I am only putting myself in the position of the physician who diagnoses his patient's case with the ulterior object of getting him strengthened. There can be no doubt that renewed vitality has been given to the doctrine by the conceptions of tautomerism and desmotropy, formulated by Conrad Laar in 1885, and by Paul Jacobson in 1887. The importance of these ideas is becoming more evident with the advancement of chemical discovery. Any attempt to break down the rigidly statical conception of our structural formulæ appears to me to be a step in the right direction. Then, again, I will remind you of the prolific development of the doctrine in the hands of Le Bel and van 't Hoff by the introduction of the stereochemical hypothesis in 1874—unquestionably the greatest advance in structural chemistry since the recognition of the quadrivalent character of the carbon atom. If evidence be required that there is a physical reality underlying the conception of valency, we need only point to the close accordance of this notion of the asymmetric carbon atom with the facts of so-called "physical isomerism" and the splendid results that have followed from its introduction into our science, especially in the field of carbohydrates through the investigations of Emil Fischer and his pupils. In other directions the stereochemical hypothesis has proved to be a most suggestive guide. It was applied by Prof. v. Baeyer in 1885 (*Ber.*, 18, 2277) to explain the conditions of stability or instability of certain atomic groupings, such as the explosiveness of polyacetylene compounds and the stability of penta- and hexa-cyclic systems. Again, in 1888 this eminent chemist showed its fertility in a series of brilliant researches upon benzene derivatives (*Ann.*, 137, 158, and subsequent papers). Nor can I omit to mention the great impetus given in this field by the classical work of Wislicenus, who in 1887 applied the hypothesis to unsaturated compounds and to cyclic systems with remarkable success ("Ueber die räumliche Anordnung der Atome in organischen Molekulen," &c.). Quite recently Victor Meyer and J. Sudborough have shown that the ability of certain derivatives of benzoic and naphthoic acids to form ethers is governed by stereochemical considerations (*Ber.*, 27, 510, 1580, 3146, and 28, 182, 1254). But I must avoid the temptation to enlarge upon this theme because the whole subject has been recently brought together by C. A. Bischoff in his "Handbuch der Stereochemie" (Frankfurt, 1893–94), a work to which all who are interested in the subject will naturally turn for reference.

While the present advanced state of structural chemistry may thus be looked upon as the outcome of the conceptions of Frankland and Kekulé, it may be well to bear in mind that the idea of structure is not *necessarily* bound up with the hypothesis of valency in its present form. Indeed, some advance had been made in representing "constitution," especially by Kolbe, before the formal introduction of this hypothesis. The two ideas have grown up together, but the experimental evidence that in any molecule the atoms are grouped together in a particular way is really independent of any theory of valency. It is only after this evidence has been acquired, either by analysis or synthesis, that we proceed to apply the hypothesis in building up the structural formula. It is of course legitimate to assume the truth of the hypothesis, and to endeavour by its use to convert an empirical into a rational formula: but this method generally gives us a choice of formulæ from which the true one can only be selected by further experimental investigation. Even within the narrower limits of isomerism it is by no means certain that all the modifications of a compound indicated by hypothesis are actually capable of existence. There is, for example, evidence that some of the "position isomerides" among the derivatives of mono- and poly-cyclic compounds are too unstable to exist; a fact which in itself is sufficient to indicate the necessity for a revision and extension of our notions of valency. Thus, by way of illustration, there is nothing in the hypothesis to indicate why orthoquinones of the benzene series should not be capable of existence; yet it is a fact that in spite of all efforts such compounds have never been obtained. The conditions essential for the existence of these compounds appear to be that the hydrogen of the benzene ring should be replaced by acid substituents such as oxygen, hydroxyl, chlorine, or bromine. Under these circumstances, as Zincke has shown (*Ber.*, 20, 1776), tetrachlor and tetrabrom-orthobenzoquinone are stable compounds. So also the interesting researches of Nietzki have proved that in such a compound as rhodizonic acid (*Ibid.*, 19, 308, and 23, 3136) ortho-

quinone oxygen atoms are present. But there is nothing in the doctrine of valency which leads us to suspect that these orthoquinone derivatives can exist while their parent compound resists all attempts at isolation. I am aware that it is dangerous to argue from negative evidence, and it would be rash to assert that these orthoquinones will never be obtained. But even in the present state of knowledge it may be distinctly affirmed that the methods which readily furnish an orthoquinone of naphthalene completely fail in the case of benzene, and it is just on such points as this that the inadequacy of the hypothesis becomes apparent. In other words, the doctrine fails in the fundamental requirement of a scientific theory; in its present form it gives us no power of prevision: it hints at possibilities of atomic groupings, but it does not tell us *à priori* which of these groupings are likely to be stable and which unstable. I am not without hope that the next great advance in the required direction may yet come from the stereochemical extension of the hypothesis, although the attempts which have hitherto been made to supply its deficiencies cannot but be regarded as more or less tentative.

THE NEW THEORY OF ABSTRACT TYPES.

I will venture, in the next place, to direct attention to a modern development of structural chemistry which will help to illustrate still further some of the points raised. For many years we have been in the habit of abstracting from our structural formulæ certain ideal complexes of atoms which we consider to represent the nucleus or type from which the compound of known constitution is derived. In other words, the hypothesis of valency which was developed originally from Gerhardt's types is now leading us back to another theory of types based upon a more intimate knowledge of atomic grouping within the molecule. In some cases these types have been shown to be capable of existence: in others they are still ideal. Used in this way the doctrine of valency is most suggestive, but at the same time its lack of prevision is constantly forcing itself upon the attention of chemical investigators. The parent compound has sometimes been known before its derivatives, as in the case of ammonia, which was known long before the organic amines and amides. In other instances the derivatives were obtained before the type was isolated, as in the case of the hydrazines, which were characterised by Emil Fischer in 1875, and the hydrazo-compounds, which have been known since 1863, while hydrazine itself was first obtained by Curtius in 1887. Phenylazimide was discovered by Griess in 1864, and many representatives of this group have been since prepared; but the parent compound, hydrazoic acid, was only isolated by Curtius in 1890. Derivatives of triazole and tetrazole were obtained by Bladin in 1885; the types were isolated by this chemist and by Andreocci in 1892. Pyrazole derivatives were prepared by Knorr in 1883; pyrazole itself was not isolated till 1889, by Buchner. Alkyl nitramides were discovered by Franchimont and Klobbie many years before the typical compound, nitramide, $NO_2.NH_2$, which was isolated last year by Thiele and Lachman (*Ber.*, 27, 1909). Examples might be multiplied to a formidable extent, but enough have been given to illustrate the principle of the erection of types, which were at first imaginary, but which have since become real. The utility of the hypothesis is undeniable in these cases, and we are justified in pushing it to its extreme limits. But no chemist, even if endowed with prophetic instinct, could have certainly foretold six years ago that the type of Griess' "triazobenzene" would be capable of free existence, and still less that when obtained it would prove to be a strong acid. The fact, established by Curtius, that the group $\begin{matrix} N \\ N \end{matrix}\!\!>\!N\text{-}$ functions in chemical molecules like the atom of chlorine is certainly among the most striking of recent discoveries. Only last year the list of nitrogen compounds was enriched by the addition of $CO(N_3)_2$, the nitrogen analogue of phosgene (Curtius, *Ber.*, 27, 2684).

These illustrations, drawn from the compounds of nitrogen, will serve to bring out the wonderful development which our knowledge of the chemistry of this element has undergone within the last few years. I might be tempted here into a digression on the general bearing of the very striking fact that an element comparatively inert in the free state should be so remarkably active in combination, but I must keep to the main topic, as by means of these compounds it is possible to illustrate still further both the strength and the weakness of our modern conceptions of chemical structure. Consider some of the undiscovered compounds which are foreshadowed by the process of ideal abstraction of types. The azoxy-compounds contain the complex $-N-N-$ with O bonded to both N (i.e. $-N-N-$ over $\diagdown O \diagup$) or $-N=N-$ with $\ddot{}\ O$. The types would be $HN-NH$ over $\diagdown O \diagup$ or $HN=NH$ with $\ddot{}\ O$. The first of these formulæ represents the unknown dihydro-nitrous oxide. The azo-compounds are derivatives of the hypothetical diimide $HN:NH$. An attempt to prepare this compound from azodicarbonic acid (Thiele, *Ann.*, 271, 130) resulted in the formation of hydrazine. The diethyl-derivative may have been obtained by Harries (*Ber.*, 27, 2276), but this is doubtful. It is at present inexplicable why compounds in which the group $\cdot N:N\cdot$ is in combination with aromatic radicles should be so remarkably stable, while the parent compound appears to be incapable of existence. The addition of two atoms of hydrogen converts this type again into a stable compound. There is nothing in the structural formulæ to indicate these facts. The amidines are stable compounds, and the so-called "anhydro-bases," or imidazoles, are remarkably stable; the parent compound $HC\begin{smallmatrix} \diagup NH \\ \diagdown NH_2 \end{smallmatrix}$, has not been obtained, while its amido-derivative, $H_2N.C\begin{smallmatrix} \diagup\!\!\diagup NH \\ \diagdown NH_2 \end{smallmatrix}$, is the well-known substance guanidine. The isodiazo-compounds recently discovered by Schraube and Schmidt and by Bamberger (*Ibid.*, 27, 514, 679, &c.) are possibly derivatives of the hypothetical substance $O:N.NH_2$, which might be named nitrosamide. Why this compound should not exist as well as nitramide is another question raised by the principle of abstract types. The carbizines were formerly regarded as derivatives of the compounds $CO\begin{smallmatrix} \diagup NH \\ \cdot \\ \diagdown NH \end{smallmatrix}$ and $CS\begin{smallmatrix} \diagup NH \\ \cdot \\ NH_2 \end{smallmatrix}$ (Fischer, *Ann.*, 212, 326; Freund and Goldsmith, *Ber.*, 21, 2456). Although this structure has now been disproved the possible existence of the types has been suggested. Carbizine and thiocarbizine differ from urea and thiocarbamide only by two atoms of hydrogen. These types have not been isolated: if they are incapable of existence the current views of molecular structure give no suggestion of a reason. The diazoamides are derivatives of the hypothetical $H_2N.NH.NH_2$ or $HN:N.NH_2$, compounds which Curtius speaks of as the propane and propylene of the nitrogen series. The latter complex was at one time thought to exist in diazohippuramide (*Ber.*, 24, 3342. This has since been shown to be hippurazide, *i.e.* a derivative of N_3H, *Ber.*, 27, 779), and a biacidyl derivative of the former type has also been obtained (*Ibid.*, 3344). Both these types await isolation if they are capable of existence. I may add that several attempts to convert diazoamides into dihydro-derivatives by mild alkaline reduction have led me to doubt whether this nitrogen chain can exist in combination with hydrocarbon radicles. The bisdiazoamides of H. v. Pechmann and Frobenius (*Ber.*, 27, 898) are derivatives of the 5-atom chain $H_2N.NH.NH.NH.NH_2$ or $HN:N.NH.N:NH$, a type which hardly seems likely to be of sufficient stability to exist. The tetrazones of Emil Fischer have for their type the 4-atom chain $H_2N.N:N.NH_2$ or $H_2N.NH.NH.NH_2$, of which the free existence is equally problematical, although a derivative containing the chain $-N:N.NH.NH-$ has been obtained by Curtius (*Ibid.*, 26, 1263). Hydrazoic acid may be regarded as a derivative of triimide, $HN\begin{smallmatrix} \diagup NH \\ \diagdown NH \end{smallmatrix}$, but this type appears to be also incapable of isolation (Curtius, *Ber.*, 26, 407). The hydrazidines or formazyls of Pinner (*Ber.*, 17, 182) and of H. v. Pechmann (*Ibid.*, 25, 3175), have for their parent compound the hypothetical substance $H_2N.N:CH.N:NH$. In 1888 Limpricht described certain azo-compounds (*Ibid.*, 21, 3422) which, if possessing the structure assigned by that author, must be regarded as derivatives of diamidotetrimide:

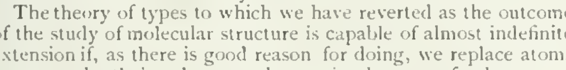
$$\begin{matrix} HN-NH \\ | \qquad | \\ HN-NH \end{matrix} \qquad\qquad \begin{matrix} H_2N.N-N \\ | \qquad | \\ H_2N.N-N \end{matrix}$$

Both these types are at present imaginary; whether it is possible for cyclic nitrogen systems to exist we have no means of knowing—all that can be said is that they have never yet been obtained. It is possible, as I pointed out in 1890 at the Leeds meeting of

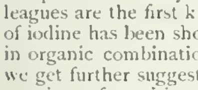
the British Association, that mixed diazoamides may be derivatives of such a 4-atom ring.

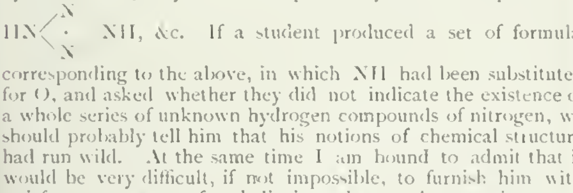
Any chemist who has followed the later developments of the chemistry of nitrogen could supply numerous other instances of undiscovered types. A chapter on the unknown compounds of this element would furnish quite an exciting addition to many of those books which are turned out at the present time in such profusion to meet the requirements of this or that examining body. I have selected my examples from these compounds simply because I can claim some of them as personal acquaintances. It would be easy to make use of carbon compounds for the same purpose, but it is unnecessary to multiply details. It has frequently happened in the history of science that a well-considered statement of the shortcomings of a theory has led to its much-desired extension. This is my hope in venturing to point out one of the chief deficiencies in the structural chemistry of the present time. I am afraid that I have handled the case badly, but I am bound to confess that I am influenced by the same feelings as those which prevent us from judging an old and well-tried friend too severely.

The theory of types to which we have reverted as the outcome of the study of molecular structure is capable of almost indefinite extension if, as there is good reason for doing, we replace atoms or groups by their valency analogues in the way of other atoms or groups of atoms. The facts that in cyclic systems N can replace CH (benzene and pyridine), that O, S, and NH are analogues in furfurane, thiophene, and pyrrole, are among the most familiar examples. The remarkable iodo- and iodoso-compounds recently discovered by Victor Meyer and his colleagues are the first known instances in which the trivalent atom of iodine has been shown to be the valency analogue of nitrogen in organic combination. Pushing this principle to the extreme we get further suggestions for new groupings, but, as before, no certainty of prevision. Thus, if nitrogen formed the oxide N_2O_2 the series might be written:



Of course these formulæ are more or less conjectural, being based on valency only. But since nitrous oxide is the analogue of hydrazoic acid, they hint at the possibility of such compounds as

 &c. If a student produced a set of formulæ corresponding to the above, in which NH had been substituted for O, and asked whether they did not indicate the existence of a whole series of unknown hydrogen compounds of nitrogen, we should probably tell him that his notions of chemical structure had run wild. At the same time I am bound to admit that it would be very difficult, if not impossible, to furnish him with satisfactory reasons for believing that such groupings are improbable. Compare again the series:



The first is urea; the second, third, fourth, fifth (methylene diamine), and sixth are unknown; the seventh is the remarkably interesting diazomethane discovered last year by H. v. Pechmann (*Ber.*, 27, 1888). The last compound, dinitromethane, is known in the form of its salts, but appears to be incapable of existence in the free state. There is nothing expressed or implied in the existing theory of chemical structure to explain why dinitromethane is unstable while trinitromethane is stable, and mono- and tetranitromethane so stable as to admit of being distilled without decomposition. Chemists will form their own views as to the possibility or impossibility of such a series as this being completed. Whether there would be a concordance of opinion I will not venture to say; but any chemist who expressed either belief or disbelief with regard to any special member would, I imagine, have great difficulty in giving a scientific reason for the faith which is in him. At the most, he would have only the very unsafe guide of analogy to fall back upon. Perhaps by the time the British Association holds its next meeting at Ipswich it will have become possible to prove that one particular configuration of certain atoms is possible and another configuration impossible. Then will have been achieved that great advance for which we are waiting the reunion of the two streams into which our science began to diverge shortly after the last Ipswich meeting.

The present position of structural chemistry may be summed up in the statement that we have gained an enormous insight into the anatomy of molecules, while our knowledge of their physiology is as yet in a rudimentary condition. In the course of the foregoing remarks I have endeavoured to indicate the direction in which our theoretical conceptions are most urgently pressing for extension. It is, perhaps, as yet premature to pronounce an opinion as to whether the next development is to be looked for from the stereochemical side; but it is not going too far to express once again the hope that the geometrical representation of valency will give us a deeper insight into the conditions which determine the stability of atomic configurations. The speculations of A. v. Baeyer, Wislicenus, Victor Meyer, Wunderlich, Bischoff, and others have certainly turned the attention of chemists towards a quarter from which a new light may eventually dawn.

THE PROGRESS OF SYNTHETICAL CHEMISTRY.

If, in my earnest desire to see the foundations of structural chemistry made more secure, I may have unwittingly given rise to the impression that I am depreciating its services as a scientific weapon, let me at once hasten to make amends by directing attention to the greatest of its triumphs, the synthesis of natural products, *i.e.* of compounds which are known to be produced by the vital processes of animals and plants.

Having been unable to find any recent list of the natural compounds which have been synthesised, I have compiled a set of tables which will, I hope, see the light at no very distant period. According to this census we have now realised about 180 such syntheses. The products of bacteria have been included in the list because these compounds are the results of vital activity in the same sense that alcohol is a product of the vital activity of the yeast plant. On the other hand the various uro-compounds resulting from the transformation in the animal economy of definite chemical substances administered for experimental purposes have been excluded, because I am confining my attention to natural products. Of course the importance of tracing the action of the living organism on compounds of known constitution from the physiological point of view cannot be overestimated. Such experiments will, without doubt, in time shed much light on the working of the vital laboratory.

The history of chemical synthesis has been so thoroughly dealt with from time to time that I should not have ventured to obtrude any further notice of this subject upon your patience were it not for a certain point which appeared to me of sufficient interest to merit reconsideration. It is generally stated that the formation of urea from ammonium cyanate by Wohler in 1828 was the first synthesis of an organic compound. There can be no doubt that this discovery, which attracted much attention at the time, gave a serious blow to the current conceptions of organic chemistry, because urea was so obviously a product of the living animal. It will be found, however, that about the same time Henry Hennell, of Apothecaries' Hall, had really effected the synthesis of alcohol—that is to say, had synthesised this compound in the same sense that Wohler had synthesised urea. The history is soon told. In 1826 Hennell (through Brande) communicated a paper to the Royal Society which appears in the *Philosophical Transactions* for that year.[1] In studying the compounds produced by the action of sulphuric acid on alcohol, and known as "oil of wine," he obtained sulphovinic acid, which had long been known, and gave fairly good analyses of this acid and of some of its salts, while expressing in the same paper very clear notions as to its chemical nature. Having satisfied himself that sulphovinic acid is a product of the action in question, he then proceeded to examine some sulphuric acid which had absorbed eighty times its volume of olefiant gas, and which had been placed at his disposal for this purpose by Michael Faraday. From this he also isolated sulphovinic acid. In another paper, communicated to the Royal Society in 1828,[2] he proves quantitatively that when sulphovinic acid is distilled with sulphuric acid and water the whole of the alcohol and sul-

[1] "On the Mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the resulting compound," *Phil. Trans.*, 1826, p. 240.

[2] "On the Mutual Action of Sulphuric Acid and Alcohol, and on the Nature of the Process by which Ether is formed," *Phil. Trans.*, 1828, p. 365.

phuric acid which united to form the sulphovinic acid are recovered. In the same paper he shows that he had very clear views as to the process of etherification. Hennell's work appears to have been somewhat dimmed by the brilliancy of his contemporaries who were labouring in the same field; but it is not too much to claim for him, after the lapse of nearly seventy years, the position of one of the pioneers of chemical synthesis. Of course in his time the synthesis was not complete, because he did not start from inorganic materials. The olefiant gas used by Faraday had been obtained from coal-gas or oil-gas. Moreover, in 1826-1828 alcohol was not generally regarded as a product of vital activity, and this is, no doubt, the reason why the discovery failed to produce the same excitement as the formation of urea. But the synthesis of alcohol from ethylene had, nevertheless, been accomplished, and this hydrocarbon occupied at that time precisely the same position as ammonium cyanate. The latter salt had not then been synthesised from inorganic materials, and the formation of urea, as Schorlemmer points out ("The Rise and Development of Organic Chemistry," p. 195), was also not a complete synthesis. The reputation of Wöhler, the illustrious friend and colleague of the more illustrious Liebig, will lose not a fraction of its brilliancy by the raising of this historical question. Science recognises no distinction of nationality, and the future historian of synthetical chemistry will not begrudge the small niche in the temple of fame to which Hennell is entitled.

Like many other great discoveries in science, the artificial formation of natural products began, as in the case of alcohol and urea, with observations arising from experiments not primarily directed to this end. It was not till the theory of chemical structure had risen to the rank of a scientific guide that the more complicated syntheses were rendered possible by more exact methods. We justly credit structural chemistry with these triumphant achievements. In arriving at such results any defects in the theory of structure are put out of consideration because—and this point must never be lost sight of—all doubt as to the possibility of this or that atomic grouping being stable is set aside at the outset by the actual occurrence of the compound in nature. The investigator starts with the best of all assurances. From the time of Wöhler and Hennell the course of discovery in this field has gone steadily on. The announcement of a new synthesis has ceased to produce that excitement which it did in the early days when the so-called "organic" compounds were regarded as products of a special vital force. The interest among the uninitiated now rises in proportion to the technical value of the compound. The present list of 180 odd synthetical products comprises, among the latest discoveries, gentisin, the colouring-matter of the gentian root (*Gentiana lutea*), which has been prepared by Kostanecki and Tambor, and caffeine, synthesised by Emil Fischer and Lorenz Ach, starting from dimethylurea and malonic acid.

I have allowed myself no time for those prophetic flights of the imagination which writers on this subject generally indulge in. When we know more about the structure of highly complex molecules, such as starch and albumin, we shall probably be able to synthesise these compounds. It seems to me more important just at present to come to an understanding as to what is meant by an organic synthesis. There appears to be an impression among many chemists that a synthesis is only effected when a compound is built up from simpler molecules. If the simpler molecules can be formed directly from their elements, then the synthesis is considered to be complete. Thus urea is a complete synthetical product, because we can make hydrogen cyanide from its elements; from this we can prepare a cyanate, and finally urea. In dictionaries and text-books we find synthetical processes generally separated from modes of formation, and the latter in their turn kept distinct from methods of preparation. The distinction between formation and preparation is obviously a good one, because the latter has a practical significance for the investigator. But the experience gained in drawing up the tables of synthesised compounds, to which I have referred, has resulted in the conclusion that the terms "synthesis" and "mode of formation" have been either unnecessarily confused or kept distinct without sufficient reason, and that it is impossible now to draw a hard-and-fast line between them. Some recent writers, such, for example, as Dr. Karl Elbs, in his admirable work on this subject ("Die synthetischen Darstellungsmethoden der Kohlenstoffverbindungen," Leipzig, 1889), have expanded the meaning of the word synthesis so as to comprise generally the building up of organic molecules by the combination of carbon with carbon, without reference to the circumstance whether the compound occurs as a natural product or not. But although this definition is sufficiently wide to cover the whole field of the production of carbon compounds from less complex molecules, it is in some respects too restricted, because it excludes such well-known cases as the formation of hydrogen cyanide from its elements, or of urea from ammonium cyanate. I should not consider the discussion of a mere question of terminology of sufficient importance to occupy the attention of this Section were it not for a matter of principle, and that a principle of the very greatest importance, which I believe to be associated with a clear conception of chemical synthesis. The great interest of all work in this field arises from our being able, by laboratory processes, to obtain compounds which are also manufactured in nature's laboratory—the living organism. It is in this direction that our science encroaches upon biology through physiology. Now, if we confine the notion of synthesis to the building up of molecules from simpler molecules or from atoms, we exclude one of nature's methods of producing many of these very compounds which we claim to have synthesised. There can be no manner of doubt that a large proportion, if not a majority, of the natural products which have been prepared artificially are not synthesised by the animal or plant in the sense of building up at all. They are the results of the breaking down—of the degradation—of complex molecules into simpler ones. I urge, therefore, that if in the laboratory we can arrive at one of these products by decomposing a more complex molecule by means of suitable reagents, we have a perfect right to call this a synthesis, provided always that the more complex molecule, which gives us our compound, can be in its turn synthesised, by no matter how many steps, from its constituent atoms. Thus oxalic acid has been directly synthesised from carbon dioxide by Kolbe and Drechsel by passing this gas over potassium or sodium amalgam heated to 360°. Whether the plant makes oxalic acid directly out of carbon dioxide we cannot at present state; if it does it certainly does not employ Kolbe and Drechsel's process. On the other hand this acid may, for all that is known, exist in the plant as a product of degradation. Many more complex acids, such as citric and tartaric, break down into oxalic acid when fused with potash. Both citric and tartaric acids can now be completely synthesised; therefore the formation of oxalic acid from these by potash fusion is a true synthesis.

The illustration given will make clear the point which I am urging. The distinction between a synthesis and a mode of formation vanishes when we can obtain a compound by the breaking down of a more complex molecule in all those cases where the latter can be completely built up. If we do not expand the meaning of synthesis so as to comprise such cases we are simply shutting the door in nature's face. It must be borne in mind that the actual yield of the compound furnished by the laboratory process does not come into consideration, because it may be generally asserted that in most cases the artifical processes are not the same as those which go on in the animal or plant. The information of real value to the physiologist which these syntheses give is the suggestion that such or such a compound may possibly result from the degradation of this or that antecedent compound, and not from a process of building up from simpler molecules.

The Bearing of Chemical Synthesis on Vital Chemistry.

With these views—the outcome of structural chemistry—the chemist and physiologist may join hands and move fearlessly onwards towards the great mystery of vital chemistry. In considering the results of organic synthesis two questions always arise as it were spontaneously: How does nature produce these complicated molecules without the use of strong reagents and at ordinary temperatures? What bearing have our laboratory achievements on the mechanism of vitality? The light shed upon these questions by experimental investigation has as yet flickered only in fitful gleams. We are but dwellers in the outer gates, waiting for the guide who is to show us the bearing of modern research on the great problem which confronts alike the physicist, the chemist, and the biologist. The chemical processes that go on in the living organism are complex to an extent that is difficult to realise. Of the various compounds of animal or vegetable origin that have been produced synthetically some are of the nature of waste products, resulting from metabolic degradation; others are the result of zymolytic action within the organism; and others, again, are secondary products arising from the action of associated bacteria, the relationship between the bac-

teria and their host being as yet imperfectly understood. The answer to the question how nature produces complicated organic molecules will be much facilitated when the physiologist, by experiment and observation, shall have made possible a sound classification of these synthetical products based on their mode of origination in the organism.

The enlargement of the definition of organic synthesis which I have advocated has been rendered necessary by the consideration of certain questions which have arisen in connection with the present condition of chemical discovery in this field. What evidence is there that any one of the 180 compounds which have been prepared artificially is produced in the organism by a direct process of building up? Is not the opposite view quite as probable? May they not, from the simplest to the most complex, be products of the degradation of still more complex molecules? I venture to suggest—not without some temerity lest our colleagues of Sections I and K should treat me as an intruder—that this view should be given a fair trial. I am aware that the opposite view, especially as regards plant assimilation, has long been held, and especially since 1870, when v. Baeyer advanced his celebrated theory of the formic aldehyde origin of carbohydrates. It is but natural to consider that the formation of a complex molecule is the result of a building-up process. It must be remembered, however, that in the living organism there is always present a compound or mixture, or whatever we like to call it, of a highly complex proteid nature, which, although at present indefinite from the purely chemical point of view, is the essence of the vitality. Of course I refer to what biologists have called protoplasm. Moreover, it is perhaps necessary to state what is really nothing more than a truism, viz. that protoplasm is present in and forms a part of the organism from the very beginning of its existence—from the germ to the adult, and onwards to the end of life. Any special chemical properties pertaining to protoplasm are inseparable from the animal or plant until that period arrives which Kekulé has hinted at when we shall be able to "build up the formative elements of living organisms" in the laboratory (NATURE, vol. xviii. p. 212). But here I am afraid I am allowing the imagination to take a flight which I told you a few minutes ago that time would not admit of.

The view that requires pushing forward into a more prominent position than it has hitherto occupied is that all the chemical transformations in the organism—at any rate all the primary changes—are made possible only by the antecedent combination of the substances concerned with living protoplasmic materials. The carbon dioxide, water, &c., which the plant absorbs must have formed a compound or compounds with the protoplasmic material of the chloroplasts before starch, or sugar, or cellulose can be prepared. There is, on this view, no such process as *the direct combination* of dead molecules to build up a complex substance. Everything must pass through the vital mill. The protoplasmic molecule is vastly more complex than any of the compounds which we have hitherto succeeded in synthesising. It might take up and form new and unstable compounds with carbon dioxide or formic aldehyde, or sugar, or anything else, and our present methods of investigation would fail to reveal the process. If this previous combination and, so to speak, vitalisation of dead matter actually occurs, the appearance of starch as the first visible product of assimilation, as taught by Sachs, or the formation of a 12-carbon-atom sugar as the first carbohydrate, as shown by the recent researches of Horace Brown and G. H. Morris, is no longer matter for wonderment. The chemical equations given in physiological works are too purely chemical; the physiologists have, I am afraid, credited the chemists with too much knowledge—it would appear as though their intimate familiarity with vital processes had led them to undervalue the importance of their prime agent. In giving expression to these thoughts I cannot but feel that I am treating you to the strange spectacle of a chemist pleading from the physiologists for a little more vitality in the chemical functions of living organisms. The future development of vital chemistry rests, however, with the chemist and physiologists conjointly; the isolation, identification, and analysis of the products of vital activity, which has hitherto been the task of the chemist, is only the preliminary work of physiological chemistry leading up to chemical physiology.

PROTOPLASMIC THEORY OF VITAL SYNTHESIS.

The supposition that chemical synthesis in the organism is the result of the combination of highly complex molecules with simpler molecules, and that the unstable compounds thus formed then undergo decomposition with the formation of new products, may be provisionally called the protoplasmic theory of vital synthesis. From this standpoint many of the prevailing doctrines will have to be inverted, and the formation of the more complex molecules will be considered to precede the synthesis of the less complex. It may be urged that this view simply throws back the process of vital synthesis one stage and leaves the question of the origin of the most complex molecules still unexplained. I grant this at once; but in doing so I am simply acknowledging that we have not yet solved the enigma of life. We are in precisely the same position as is the biologist with respect to abiogenesis, or the so-called "spontaneous generation." To avoid possible misconception let me here state that the protoplasmic theory in no way necessitates the assumption of a special "vital force." All that is claimed is a peculiar, and at present to us mysterious, power of forming high-grade chemical combinations with appropriate molecules. It is not altogether absurd to suppose that this power is a special property of nitrogen in certain forms of combination. The theory is but an extension of the views of Kuhne, Hoppe-Seyler, and others respecting the mode of action of enzymes. Neither is the view of the degradational origin of synthetical products in any way new.[1] I merely have thought it desirable to push it to its extreme limit in order that chemists may realise that there is a special chemistry of protoplasmic action, while the physiologists may exercise more caution in representing vital chemical transformations by equations which are in many cases purely hypothetical, or based on laboratory experiments which do not run parallel with the natural process. The chemical transformations which go on in the living organism are thus referred back to a peculiarity of protoplasmic matter, the explanation of which is bound up with the inner mechanism of the process of assimilation. If, as the protoplasmic theory implies, there must be combination of living protoplasm with appropriate compounds before synthesis is possible, then the problem resolves itself into a determination of the conditions which render such combination possible—*i.e.* the conditions of assimilation. It may be that here also light will come from the stereochemical hypothesis. The first step was taken when Pasteur found that organised ferments had the power of discriminating between physical isomerides; a similar selective power has been shown to reside in enzymes by the researches of Emil Fischer and his coadjutors. Fischer has quite recently expressed the view that the synthesis of sugars in the plant is preceded by the formation of a compound of carbon dioxide, or of formic aldehyde, with the protoplasmic material of the chloroplast, and similar views have been enunciated by Stohmann. The question has further been raised by van 't Hoff, as well as by Fischer, whether a stereochemical relationship between the living and dead compounds entering into combination is not an absolutely essential condition of all assimilation. The settlement of this question cannot but lead us onwards one stage towards the solution of the mystery that still surrounds the chemistry of the living organism.

RECENT DISCOVERIES OF GASEOUS ELEMENTS.

The past year has been such an eventful one in the way of startling discoveries that I must ask indulgence for trespassing a little further upon the time of the Section. It was only last year at the Oxford meeting of the British Association that Lord Rayleigh and Prof. Ramsay announced the discovery of a gaseous constituent of the atmosphere which had up to that time escaped detection. The complete justification of that announcement is now before the world in the paper recently published in the *Philosophical Transactions* of the Royal Society. The history of this brilliant piece of work is too recent to require much recapitulation. I need only remind you how, as the result of many years' patient determinations of the density of the gases oxygen and nitrogen, Lord Rayleigh established the fact that atmospheric nitrogen was heavier than nitrogen from chemical sources, and was then led to suspect the existence of a heavier gas in the atmosphere. He set to work to isolate this substance, and succeeded in doing so by the method of Cavendish. In the meantime Prof. Ramsay, quite independently, isolated the gas by removing the nitrogen by means of red-hot magnesium, and the

[1] See, *e.g.*, Vines' "Lectures on the Physiology of Plants," pp. 145, 218, 227, 233, and 234. Practically all the great classes of synthetical products are regarded as the results of the destructive metabolism of protoplasm. A special plea for protoplasmic action has also been urged, from the biological side, by W. T. Thiselton-Dyer, *Journ. Chem. Soc.*, 1893; *Trans.* pp. 680-681

two investigators then combining their labours, followed up the subject, and have given us a memoir which will go down to posterity among the greatest achievements of an age renowned for its scientific activity.

The case in favour of argon being an element seems to be now settled by the discovery that the molecule of the gas is monatomic, as well as by the distinctness of its electric spark spectrum. The suggestion put forward soon after the discovery was announced, that the gas was an oxide of nitrogen, must have been made in complete ignorance of the methods by which it was prepared. The possibility of its being N_3 has been considered by the discoverers and rejected on very good grounds. Moreover, Peratoner and Oddo have been recently making some experiments in the laboratory of the University of Palermo with the object of examining the products of the electrolysis of hydrazoic acid and its salts. They obtained only ordinary nitrogen, not argon, and have come to the conclusion that the anhydride $N_3.N_3$ is incapable of existence, and that no allotropic form of nitrogen is given off. It has been urged that the physical evidence in support of the monatomic nature of the argon molecule, viz. the ratio of the specific heats, is capable of another interpretation—that argon is in fact an element of such extraordinary energy that its atoms cannot be separated, but are bound together as a rigid system which transmits the vibrational energy of a sound-wave as motion of translation only. If this be the state of affairs we must look to the physicists for more light. So far as chemistry is concerned, this conception introduces an entirely new set of ideas, and raises the question of the monatomic character of the mercury molecule which is in the same category with respect to the physical evidence. It seems unreasonable to invoke a special power of atomic linkage to explain the monatomic character of argon, and to refuse such a power in the case of other monatomic molecules, like mercury or cadmium. The chemical inertness of argon has been referred also to this same power of self-combination of its atoms. If this explanation be adopted it carries with it the admission that those elements of which the atoms composing the molecule are the more easily dissociated should be the more chemically active. The reverse appears to be the case if we bear in mind Victor Meyer's researches on the dissociation of the halogens, which prove that under the influence of heat the least active element, iodine, is the most easily dissociated. On the whole, the attempts to make out that argon is polyatomic by such forced hypotheses cannot at present be considered to have been successful, and the contention of the discoverers that its molecule is monatomic must be accepted as established.

In searching for a natural source of combined argon Prof. Ramsay was led to examine the gases contained in certain uranium and other minerals, and by steps which are now well known he has been able to isolate helium, a gas which was discovered by means of the spectroscope in the solar chromosphere during the eclipse of 1868 by Profs. Norman Lockyer and E. Frankland. In his address to the British Association in 1872 (*Reports*, 1872, p. lxxiv. the late Dr. W. B. Carpenter said:—

"But when Frankland and Lockyer, seeing in the spectrum of the yellow solar prominences a certain bright line not identifiable with that of any known terrestrial flame, attribute this to a hypothetical new substance which they propose to call helium, it is obvious that their assumption rests on a far less secure foundation, until it shall have received that verification which, in the case of Mr. Crookes' researches on thallium, was afforded by the actual discovery of the new metal, whose presence had been indicated to him by a line in the spectrum not attributable to any substance then known."

It must be as gratifying to Profs. Lockyer and Frankland as it is to the chemical world at large to know that helium may now be removed from the category of solar myths and enrolled among the elements of terrestrial matter. The sources, mode of isolation, and properties of this gas have been described in the paper recently published by Prof. Ramsay and his colleagues. Not the least interesting fact is the occurrence of helium and argon in meteoric iron from Virginia, as announced by Prof. Ramsay in July (NATURE, vol. lii. p. 224). Like argon, helium is monatomic and chemically inert so far as the present evidence goes. The conditions under which this element exists in cleveite, uraninite, and the other minerals have yet to be determined.

Taking a general survey of the results thus far obtained, it seems that two representatives of a new group of monatomic elements characterised by chemical inertness have been brought to light. Their inertness obviously interposes great difficulties in the way of their further study from the chemical side: the future development of our knowledge of these elements may be looked for from the physicist and spectroscopist. Prof. Ramsay has not yet succeeded in effecting a combination between argon or helium and any of the other chemical elements. M. Moissan finds that fluorine is without action on argon. M. Berthelot claims to have brought about a combination of argon with carbon disulphide and mercury, and with "the elements of benzene, . . . with the help of mercury," under the influence of the silent electric discharge. Some experiments which I made last spring with Mr. R. J. Strutt with argon and moist acetylene submitted to the electric discharge, both silent and disruptive, gave very little hope of a combination between argon and carbon being possible by this means. The coincidence of the helium yellow line with the D_3 line of the solar chromosphere has been challenged, but the recent accurate measurements of the wave-length of the chromospheric line by Prof. G. E. Hale, and of the line of terrestrial helium by Mr. Crookes, leave no doubt as to their identity. Both the solar and terrestrial lines have now been shown to be double. The isolation of helium has not only furnished another link proving community of matter, and, by inference, of origin between the earth and sun, but an extension of the work by Prof. Norman Lockyer, M. Deslandres, and Mr. Crookes, has resulted in the most interesting discovery that a large number of the lines in the chromospheric spectrum, as well as in certain stellar spectra, which had up to the present time found no counterparts in the spectra of terrestrial elements, can now be accounted for by the spectra of gases contained with helium in these rare minerals. The question now confronts us, Are these gases members of the same monatomic inert group as argon and helium? Whether, and by what mechanism, a monatomic gas can give a complicated spectrum is a physical question of supreme interest to chemists, and I hope that a discussion of this subject with our colleagues of Section A will be held during the present meeting. That mercury is capable under different conditions of giving a series of highly complex spectra can be seen from the memoir by J. M. Eder and E. Valenta, presented to the Imperial Academy of Sciences of Vienna in July 1894. With respect to the position of argon and helium in the periodic system of chemical elements, it is, as Prof. Ramsay points out, premature to speculate until we are quite sure that these gases are homogeneous. It is possible that they may be mixtures of monatomic gases, and in fact the spectroscope has already given an indication that they contain some constituent in common. The question whether these gases are mixtures or not presses for an immediate answer. I will venture to suggest that an attack should be made by the method of diffusion. If argon or helium were allowed to diffuse fractionally through a long porous plug into an exhausted vessel there might be some separation into gases of different densities, and showing modifications in their spectra, on the assumption that we are dealing with mixtures composed of molecules of different weights.

NOTES.

THE *Times* of Tuesday last contained a letter, signed by Profs. M. Foster, E. Ray Lankester, and G. B. Howes (Hon. Secretaries to the Provisional Committee), with reference to the General Committee now being formed for the purpose of establishing a memorial of the late Prof. Huxley. The letter states that H.R.H. the Prince of Wales has been pleased to become the Honorary President of the Committee. No very active steps can be taken until after the autumn recess, when the General Committee will hold its first meeting, probably in October. The Honorary Secretaries will after that report the progress that has been made both in this country and abroad, and a list of the complete Committee and a statement of the subscriptions received will be published. Appended to the letter is a list of an enormous number of names of persons who have already signified their desire to serve on the Committee.

A MEMORIAL tablet in honour of Prof. Helmholtz has been affixed to the house, No. 8 Hadtzstrasse, at Potsdam, where he was born, and it is stated that it is intended to erect a joint

monument to the memory of Werner Siemens and Helmholtz in front of the Technische Hochschule at Charlottenburg.

Prof. Retsius and Dr. Bergh, of Copenhagen, have been elected Correspondants of the Paris Academy.

The Berliner Akademie der Wissenschaften has, we understand, recently elected the following gentlemen as corresponding members:—Prof. W. V. Gümbel (Münich), Prof. A. von Zittell (Münich), Prof. A. Schrauf (Vienna), Prof. A. Cossa (Turin), Prof. A. Agassiz (Cambridge, Mass.), and Prof. E. Mascart (Paris).

The quinquennial International Metric Congress, which is at present being held in Paris, under the presidency of Dr. Marey, was opened on the 4th inst. by M. Hanotaux, who delivered a brief address. On the 6th inst. the second session of the Congress took place, and M. Hirsch, of the Neuchâtel Observatory, was elected Secretary. The Secretary presented the report of the Committee on the work already done, and the present state of the International Bureau of Weights and Measures, and a series of metric standards which have been under consideration since the Congress of 1889 was sanctioned.

The Swiss Naturforschende Gesellschaft has been holding its annual congress at Zermatt. The proceedings began on September 8, and concluded on the 11th. September 8 was devoted to the meetings of committees; the Sections met on September 10, and on the 9th and 11th inst. the general meetings took place.

The death is announced of Dr. Sven Lovén, the distinguished Swedish naturalist. He was born, says the *Times*, at Stockholm in 1809, and received his education at the University of Lund, where he took the degree of Doctor of Philosophy. After attending lectures in Berlin in 1830-31, he devoted himself to the study of the maritime fauna of the coasts of Scandinavia. He also explored the Baltic and the North Seas, and conducted the first scientific expedition to Spitzbergen in 1837. He was the author of numerous scientific memoirs, all published by the Royal Swedish Academy of Sciences. Dr. Lovén was elected a member of the Academy of Stockholm in 1840, and Professor and Conservator of the Royal Museum of Natural History of that city in 1841. He was a member of the academies of Berlin and Munich, a corresponding member of the Institute of France, and in 1885 was elected a foreign member of the Royal Society of London.

The death is recorded, at the age of eighty-one years, of Mr. James Carter, of Cambridge. For very many years Mr. Carter practised as a medical man, but found time to engage in the study of scientific and antiquarian subjects, and was especially interested in palæontology. He contributed many papers to the *Geological Magazine* and the *Quarterly Journal* of the Geological Society, and served for many years on the Councils of the Geological and Palæontological Societies.

The *Kew Bulletin* has heard with regret of the death from dysentery in May last of Mr. F. H. Smiles, who had been attached to the Royal Survey Department of Siam. Mr. Smiles, who had already done some good botanical work, returned to Siam in December last with the intention of making further botanical collections, and it was confidently anticipated that he would have added considerably to the knowledge of the rich flora of Upper Siam.

The death is announced of Mr. R. H. Tweddell, the well-known engineer; of Mr. E. F. C. Davis, president of the American Society of Mechanical Engineers; and of Mr. H. C. Hart, one of the first class technical officers of the engineer-in-chief's office, Post Office Telegraphs.

The centenary of Jenner's first experiments in vaccination is to be celebrated next May by the Russian National Health Society. To commemorate the event the Society proposes (1) to offer four prizes for the best works upon vaccination; (2) to collect and publish materials for a history of the practice of vaccination in Russia, and a short history of the same in Western Europe; (3) to publish a Russian translation of Jenner's works, accompanied by his biography and portrait; (4) to organise an exhibition of objects connected with vaccination; (5) to hold a commemorative meeting on the day of the centenary.

The annual joint meeting of the Swiss Geographical Societies will be held this year at St. Gall, on September 22 and 23. At this meeting a paper will be read by Dr. Hans Meyer on the "Snow Mountains of Equatorial Africa."

An exhibition of agricultural machinery, similar to that held in May of the present year, is being arranged under the auspices of the Imperial and Royal Agricultural Society of Vienna, to take place in that city in May 1896. The exhibits will comprise not only agricultural machines as generally understood, but appliances used in all branches of industry connected with agriculture, such as breweries, and distilleries, and yeast, sugar, vinegar, and starch factories.

We learn from the *Nation*, New York, that only one MS. was received in competition for the prize of 400 dollars given by Dr. Gould's *Astronomical Journal* "for the most thorough discussion of the theory of the rotation of the earth with reference to the recently discovered variations of latitude." The paper was sent by and the prize awarded to Prof. Newcomb. The other prize, of 200 dollars, was given to Mr. Paul S. Yendell, for the best series of determinations of maxima and minima of variable stars.

Science states that the Berliner Akademie der Wissenschaften has recently put aside over £1000 for the promotion of scientific work and research. Of this amount an appropriation of £100 has been made to Prof. Fuchs, of Berlin, to be devoted to the continuation of the publication of Dirichlet's works; £100 to Prof. Weierstrass, of Berlin, for the publication of his collected works; £75 to Prof. Gerhardt for the publication of the mathematical correspondence of Leibnitz, and £100 to Dr. Schauinsland for researches on the Fauna of the Pacific islands.

The Göttingen Gesellschaft der Wissenschaften will, on February 1, 1897, award a prize of 500 marks for an anatomical research and description of the cavities of the body of the new-born child and their contents compared with those of the adult.

The Academy of Sciences of Cracow proposes, as the subject for the Copernicus prizes, theories concerning the physical condition of the globe. Essays must be written in the Polish language, and reach the Academy before the end of 1898.

The Orient Steam Navigation Company, Limited, announce their intention of sending one of their steamships to Vadsö, Varanger Fiord, Lapland, in August next, to enable observations to be made of the total eclipse of the sun on August 9, 1896. It is arranged for the vessel to leave London on July 21, to arrive at Vadsö on August 3, and to return from the latter place on the 10th, reaching London on August 17. Particulars as to the cost, &c., of the trip may be seen in our advertisement columns, or obtained from Messrs. Anderson, Anderson, and Co., 5 Fenchurch Avenue, E.C., or 16 Cockspur Street, S.W.

Severe thunderstorms again occurred in the southern and eastern parts of England early on Saturday morning, 7th instant, accompanied with heavy falls of hail and rain, and causing considerable damage. The disturbance was occasioned by the

development of shallow depressions over the Bay of Biscay and the English Channel, and by the intense heat over the continent, the maximum shade temperature in some parts of France being considerably above 90°, while in the east of England a temperature of 85° was recorded. Rainfall exceeded an inch in London and other places, and amounted to 1·78 inches in Hampshire. During the height of the storm the lightning flashes averaged about twenty-five to the minute.

THE Shetland County Council, says the *Glasgow Herald*, has resolved to apply to the Secretary for Scotland for an order under the Wild Birds Protection Act of 1894, prohibiting the taking of the eggs of certain wild birds. The schedule proposed includes such birds as the white-tailed or sea eagle, great skua, Richardson's skua, Allan whimbrel, ember goose, &c. All these birds have become extremely rare, and it is stated that there has been recently a trade carrying on in their eggs for the American market, to the threatened extinction of the birds.

WE are asked to announce that with the September number the *American Journal of Psychology* will enter upon its seventh volume. The preceding volumes have been edited by President G. Stanley Hall (Clark University). For the future the editorial responsibility of the *Journal* will be shared by President Hall, Prof. E. C. Sanford (Clark University), and Prof. E. B. Titchener (Cornell University). A co-operative board has been formed, which includes the names of Prof. F. Angell, Prof. H. Beaunis, Prof. J. Delboeuf, Dr. A. Kirschmann, Prof. O. Kuelpe, Dr. A. Waller, F.R.S., and Prof. H. K. Wolfe. The *Journal* will be devoted exclusively to the interests of experimental psychology (psychophysiology, psychophysics, physiological psychology, &c.). Each number will contain, as heretofore, original articles, reviews and abstracts of current psychological books and monographs, and notes upon topics of immediate psychological importance. Contributions may be addressed to either of the three editors.

Science states that the Board of Scientific Directors of the New York Botanic Garden has recently resolved to authorise a topographical survey of the 250 acres of land in Bronx Park which have been set aside for the uses of the garden. All the trees in the park are to be labelled, and new varieties of seeds desirable for cultivation are to be secured.

THE Allahabad *Pioneer Mail* says that an experiment is now in progress in several of the larger gaols of the Punjab, which may have important results in the future. It has been one of the ordinary precautions in time of cholera epidemics to boil the drinking water supplied to the prisoners. To ascertain whether it might not be advisable always to boil the drinking water, the Lieutenant-Governor has ordered that a certain number of the prisoners should be given boiled, and an equal number unboiled, water, the results being reported at the end of the year. If these are as expected, the reduction in the fever death-rate should be followed by a similar reduction in the mortality from dysentery and diarrhoea.

WE learn from *Engineering* that an important undertaking has been inaugurated at Seattle, in the State of Washington, U.S.A. This city is situated on Elliott Bay, a thoroughly sheltered harbour, which communicates with the Pacific by the Straits of San Juan de Fuca. About two miles from the coast and behind the town is a fresh-water lake of considerable size, the water level of which is about 16 feet above high water in the bay. A ship canal between the lake and the sea has long been suggested, and the work has at last been definitely commenced. The bottom of the channel will be 80 feet, and the greatest depth of cutting will be 308 feet. Almost the whole of the work will, however, be carried out through comparatively high land, the amount of excavation required being estimated at 36,000,000 cubic yards. The material is mostly glacial drift, and it is proposed to use hydraulic nozzles to facilitate the work of excavation, the spoil being washed down by a jet of water issuing at high pressure from a nozzle, as in some of the Californian gold workings. A lock 400 feet long will be constructed at the sea entrance to the canal. The material excavated will be used for raising the level of low-lying ground along the sea front of the city.

M. ZACHAREWIEZ, Professor of Agriculture at Vaucluse, has found by experiment with different-coloured glasses that fruit is finest and earliest when grown under clear glass. Orange glass produces an increase of vegetation, but at the cost of the amount of fruit, of the size and of its forwardness. Violet glass causes the number of fruit to increase at the expense of the quality. Red, blue, and green glass are hurtful to all kinds of vegetation.

THE possibility of successfully boring for water in extensive areas of crystalline rocks has been demonstrated, we learn from the September number of *Natural Science*, at several places in Sweden. The experiments were suggested by certain conclusions of Nordenskiöld, based on the downward limit of surface variations of temperature and other physical considerations. He considered that vertical jointing of the rocks would not extend below 30 or 40 metres, and that at that depth extensive horizontal fissures must be formed. This has now been found to be the case, and from these horizontal fissures abundant water of great purity has been obtained. While these results are of practical importance (particularly with regard to the water-supply of small rocky islands), it also opens up a number of interesting general questions as to the flow and pressure of water in crystalline rocks.

IN our issue for August 15, we printed an abstract of a paper on "The Voyage of the *Antarctic* to Victoria Land," read by Mr. C. E. Borchgrevink at the recent International Geographical Congress, and now have to acknowledge the receipt of the journal and notes of the commander of the whaler *Antarctic*, in which Mr. Borchgrevink made his somewhat unpropitious voyage as a sailor before the mast, which the Secretary of the Royal Geographical Society of Australasia (Victorian Branch) has been good enough to send us. The pamphlet, which contains some highly interesting matter, is accompanied by a lithographed map, by Captain Leonard Kristensen, of the track taken by his vessel, and forms part of the *Transactions* of the above-named Society.

Natural Science for September contains extracts from the address delivered by the Rev. Canon A. M. Norman, F.R.S., as President of the recently held Museums Association at Newcastle, and deals with the progress of biology in that northern town. An article on "The Geology of Ipswich and its Neighbourhood," by Mr. Clement Reid, appears at an opportune moment, and will doubtless be consulted by many geologists visiting the British Association. Other contributions to the number are:—"Some Recent Insect Literature," "The Nucleolus," "The Rôle of Sex," and "The Alleged Miocene Man in Burma." The last-named article has reference to a paper by Dr. Noetling, published towards the close of last year, "On the Occurrence of Chipped (?) Flints in the Upper Miocene of Burma." The writer, Mr. R. D. Oldham, says in conclusion, "till more complete evidence has been produced it is impossible to accept the existence of man in either Miocene or Pliocene times as one of the established facts of geology."

WE are glad to note the reappearance of the *Bollettino Mensuale* of the reorganised Italian Meteorological Society. The bulletin is issued in a more convenient, small folio form, but in other respects it is similar to the former publication. The current number contains two important articles by Prof. L. Di Marchi, on the causes of the glacial epoch, and the dynamical

conditions of thunderstorms, and an investigation of the effects of the earthquake at Florence on May 18 last, by C. Bassani.

AN examination of the gases liberated from certain of the sulphurous waters of the Pyrenees reveals, in the hands of M. Ch. Bouchard, the interesting fact that the formerly assumed nitrogen (from which the Spanish physicians have named these waters *azoades*) consists in part of free argon and helium. The collected gas was in each case, after treatment with potash and phosphoric anhydride, introduced into a Plücker tube containing magnesium wire. Under the action of the silent discharge the nitrogen rapidly disappeared by combination with magnesium, leaving a residue exhibiting the characteristic rays of both argon and helium for the gas derived from the waters of la Raillère, helium from the springs of Bois, and helium together with probably an unknown gas from the waters of lowest temperature at Bois.

THE use of magnesium wire and the silent discharge is due to MM. L. Troost and L. Ouvrard, who show that the magnesium vapour produced very rapidly combines with nitrogen under the conditions obtaining in the tubes. Further, the continued action of a powerful silent discharge, for some hours after the spectroscopic evidence proves the absence of nitrogen, results in a gradual diminution in intensity of the helium and argon rays. Finally a complete vacuum is produced, hence it appears that magnesium combines with argon and helium under these circumstances. Platinum appears to behave like magnesium towards argon in Plücker tubes with the silent discharge.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*, ♀) from India, presented by Mrs. Ball; an Emu (*Dromaeus novae-hollandiae*) from Australia, presented by Mr. C. W. Williams; a Raven (*Corvus corax*), British, presented by Mr. W. Weeker; a Royal Python (*Python reginis*) from Dahomey, West Africa, presented by Mr. C. H. Harley-Moseley; a Common Chameleon (*Chamaeleon vulgaris*) from North Africa, presented by Mr. C. Sampson; a —— Snake (*Phrynonax eutropis*), a —— Snake (*Phrynonax fasciatus*) from Trinidad, presented by Mr. R. R. Mole; a White-tailed Sea Eagle (*Haliaetus albicilla*) from Scotland, two Diamond Snakes (*Morelia spilotes*) from Australia, deposited; eight Amherst Pheasants (*Thaumalea amherstiae*), six Ring-necked Pheasants (*Phasianus torquatus*), two Japanese Pheasants (*Phasianus versicolor*), a Temminck's Tragopan (*Ceriornis temmincki*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE PROPER MOTION OF THE SUN. In the September number of the *Bulletin Astronomique* M. Tisserand gives an interesting account of a method of determining the proper motion of the sun from stellar proper motions. Denoting by m and m' the values of the annual proper motions of the stars, c the space described by the sun in one year, this space being measured with the same unit as the distance (ρ) of the sun, and A and D the Right Ascension and Declination of the apex of the sun's way, the formulae for reduction become

$$m \cos \delta = \frac{c}{\rho} \cos \mathrm{D} \sin (\alpha - \mathrm{A})$$

$$\frac{m'}{\cos \delta} = -\frac{c}{\rho} \sin \mathrm{D} + \frac{c}{\rho} \cos \mathrm{D} \tan \delta \,.\, \cos (\alpha - \mathrm{A}).$$

In the second equation the second term changes its sign with $\tan \delta$, ρ changes its value from star to star. Assuming that the mean of the values of this term will be small or zero, and that Σ represents the arithmetical mean, we have—

$$\Sigma\left(\frac{m'}{\cos \delta}\right) = -c \sin \mathrm{D}\, \Sigma\left(\frac{1}{\rho}\right).$$

Now, because sin D is positive, the mean values of the left-hand side of the equation ought to be negative. If there were no proper motion to the sun, they should be zero.

Using the catalogue of 1054 stellar proper motions, motions of M. Stumpe (*Astr. Nach.*, Nos. 2999-3000, year 1890), only those stars have been employed the declinations of which are comprised between $-30°$ and $+30°$, and the proper motions less than $0''\cdot64$.

The mean values for the sum above were then tabulated for every hour of Right Ascension. These were found to be all negative, as they ought to be, and they did not differ very much from one another. For 585 stars the mean value was $-0''\cdot151$.

M. Tisserand further investigated the values obtained from another catalogue of 2641 stellar proper motions, by M. Bossert, in exactly the same way. Here the mean values were still found all to be negative, and not very different from one another. From 1537 proper motions the value obtained was $-0''\cdot131$.

By taking only the proper motions of stars comprised between declinations $\pm 15°$, the value obtained does not differ materially from that given above. In the interval then of a century, for each hour of right ascension, the declinations of all the stars have diminished (in the mean) by quantities comprised between $10''$ and $20''$; and he says, "il nous semble que cela donne une preuve materielle frappante du mouvement du Soleil."

THE ROTATION OF VENUS.—A difficult problem in observational astronomy is the determination of the period of the rotation of Venus. M. Schiaparelli, whose powers of observations have been often put to the test, still thinks that the planet accomplishes one rotation in the same time that it takes to travel round the sun, or, in other words, the same hemisphere is always turned towards the sun. M. Leo Brunner, however, who has made during three months a great number of drawings, which appear to corroborate his statement, seems to be of quite a different opinion, for he says: "J'ai le plaisir de vous annoncer que je viens de découvrir la vraie période de rotation de Venus, qui ne diffère que de quelques minutes de celle de notre terre. Cette découverte est hors doute, car j'ai pu voir arriver et passer des taches plusieurs jours avec la plus grande distinction. Nul doute à cet egard." It must not be forgotten, however, that the observation of Venus is one attended by great difficulty. Even Brunner's drawings and those of Schiaparelli made of the planet at the same time are very different. There seems to be no doubt that the observations are all verging on the limit of visibility, and that the 224 days or the 24-hour period are just as probable as ever.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following appointments have been recently made by the governing bodies of the undermentioned colleges:—At St. John's, Mr. R. H. Adie, a Lecturer in Natural Science; at Magdalene, Mr. G. T. Manley, Lecturer in Mathematics; at Trinity, Mr. G. T. Walker, Lecturer in Mathematics, and Messrs. W. C. D. Whetham and J. W. Capstick, Lecturers in Nataral Science; at Emmanuel, Mr. A. Eicholz, Lecturer in Natural Science; at Sidney Sussex, Mr. R. H. D. Mayall, Lecturer in Mathematics; at Selwyn, Mr. L. A. Borradaile, Lecturer in Natural Science.

ACCORDING to *Science*, Prof. Bonnet, Professor of Anatomy in the University of Giessen, has received a call to Greifswald; and Dr. M. Miyoshi has been appointed Professor of Botany in the University of Tokyo.

MR. CHAS. BERRY, horticultural lecturer to the East Suffolk County Council Technical Instruction Committee, has been appointed Instructor in Horticulture by the Devonshire County Council, and will enter upon his duties at the end of September.

THE prospectus of Day and Evening Classes at the Battersea Polytechnic Institute for the session 1895-6, has reached us, and contains full information respecting the numerous classes held at this well-appointed institution. Several new classes are to be formed, and special provision is made for the needs of students who are desirous of entering for the examination of London University, from the matriculation to the final B.Sc.

THE fourth annual report (1894-5) of the Department of Agriculture, Yorkshire College, Leeds, has been published, and shows clearly that a great deal of useful work has been carried on during the past twelve months, and has, on the whole, met with very satisfactory success. With one exception (that of the classes for elementary teachers) each branch has exhibited much growth. The lectures given to farmers and others were

well attended, and the work of the lecturers was much assisted by the travelling libraries sent out by the Victoria University in connection with the various courses. A new departure was made by the institution of short lectures on poultry-keeping. At the close of the session examinations were held, at which 188 candidates from 26 centres presented themselves, and of this number 145 passed, 58 attaining distinction. The prospectus of the Courses in Agriculture, Session 1895-6, is now ready, and may be had on application to the Registrar.

THE Agricultural Department of the University College of North Wales, Bangor, has just issued its prospectus for the approaching session, in which all information respecting classes, &c., is given. Arrangements have been made by which farms in the neighbourhood of the college may be made use of by the professors and their students for practical instruction. The prospectus can be obtained from the Secretary.

THE *Technical World* says: "One of the most interesting experiments undertaken by the Durham College of Science is the provision of a series of agricultural stations, of which there are now about sixty in Northumberland, Cumberland, and Durham. At these stations practical instruction is given by means of experiment and demonstration in the science of agriculture. Manures are supplied to the stations from the college, where they are analysed and blended as may be required for the particular experiment, and the resultant crops are afterwards tested under the direction of the Professor of Agriculture. These experiments give valuable opportunities to students to observe the varying results obtained under the different conditions of soil and climate in the various districts of the North, and also provide useful data for agriculturists therein."

A NEW technical school was opened at Runcorn on August 31, by Sir John T. Brunner, M.P. The school was erected at a cost of £4200, and contains eleven class-rooms and a lecture-hall.

IN view of the forthcoming opening of the Medical Schools, the current issues of our contemporaries, the *Lancet* and *British Medical Journal*, are devoted almost exclusively to particulars likely to be of service to medical students. The *Chemical News* for September 6 is likewise a "student's number," and contains much information respecting the various schools of chemistry.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 2.—M. Fizeau in the chair.—The work of 1895 at Mont Blanc Observatory, by M. J. Janssen. Determinations of the intensity of gravity have been made with very delicate instruments at Grands-Mulets (3050 m.) and at Chamounix by M. Bigourdan. It is hoped to carry out a similar determination on the summit of Mont Blanc next year. All the parts of the 33 cm. parallactic telescope have been conveyed to the site at the head of the glacier where it is to be erected.—On the presence of argon and of helium in certain mineral waters, by M. Ch. Bouchard. (See Notes, p. 487).—On the combination of magnesium with argon and with helium, by MM. L. Troost and L. Ouvrard. (See Notes, p. 487).—On a continuous group of transformations with twenty-eight parameters which occurs in the theory of deformation of surfaces, by M. Paul Staeckel.—Researches on the combinations of mercuric cyanide with bromides, by M. Raoul Varet. Thermochemical data are given for a number of compounds of the general type $2HgCy_2.MBr_2.xH_2O$. It is shown that in solution these substances yield but a slight isopurpurate reaction, and slightly redden litmus. With heat the effect is increased. The substances possess a similar constitution as the chlorocyanides, the cyanogen remaining mostly in combination with the mercury. Rather a greater proportion of the cyanogen passes over to the second metal than in the case of the chlorocyanides. A slight evolution of heat occurs in the change, a result contrary to what would be expected from the character of the iodo-compounds.—On the formation of hydrogen selenide, by M. H. Pelabon. Liquid selenium absorbs hydrogen selenide. Carrying out experiments on the formation of hydrogen selenide in relation to temperature with the smallest excess of selenium in order to avoid this source of error, it is found that the formula of Gibbs and Duhem,

$$\log \frac{p_1}{p_2} = \frac{M}{T} + N \log T + S,$$

accurately represents the experimental results (p_1 and p_2 represent the partial pressures of H and SeH_2, T is the abs. temp. of experiment, log means Napierian log, M, N, and S are constants). The ratio $\rho = \frac{p_2}{p_1 + p_2}$ has a maximum value at a temperature $t = \frac{M}{N} - 273$. With values of the constants calculated from the experimental results, $t = 575°$, the experimental maximum agrees with this result. The molecular heat of formation calculated by Duhem's formula with the found values for the above constants is − 17380 Cal., Fabre found − 18000 Cal. The difference is not great, and may be readily accounted for when it is remembered that (1) in this formula hydrogen and hydrogen selenide have been assumed to be perfect gases; (2) the formula has been applied beyond the limits of temperature of the experiments from which M and N are determined.—Action of carbonic acid, water, and alkalis on cyanuric acid and its dissolved sodium and potassium salts, by M. Paul Lemoult. A heat of neutralisation paper in which the decomposition of cyanuric acid slowly occurring in presence of bases is shown to agree with the equation $C_3N_3O_3H_3$ diss. + $3H_2O$ + Aq = $3CO_2$ diss. + $3NH_3$ diss. + 200 Cal.—The eclipsoscope, an apparatus for viewing the chromosphere and solar protuberances, by M. Ch. V. Zenger.—M. Ch. V. Zenger sends another note relative to the possibility of predicting great seismic and atmospheric disturbances during the passage of periodic swarms of shooting-stars when great activity of the solar surface is observed at the same time.

BOOKS, PAMPHLET, and SERIALS RECEIVED.

BOOKS.—The Herschels and Modern Astronomy: A. M. Clerke (Cassell).—The Growth of the Brain: Prof. H. H. Donaldson (Scott).—Peasant Rents (Economic Classics): R. Jones, 1831 (Macmillan).—Cubature des Terrasses et Mouvement des Terres: G. Dariès (Paris, Gauthier-Villars).—Quantitative Chemical Analysis: Clowes and Coleman, 3rd edition (Churchill).—Notes on the Nebular Theory in relation to Stellar, Solar, Planetary, Cometary, and Geological Phenomena: W. F. Stanley (K. Paul).—On the Structure of Greek Tribal Society: H. E. Seebohm (Macmillan).—Observations and Researches made at the Hong Kong Observatory in the Year 1894: Dr. W. Doberck (Hong Kong).

PAMPHLET.—The Movements of the Kosi River: F. A. Shillingford (Calcutta).

SERIALS.—Science Progress, September (Scientific Press).—Proceedings of the Physical Society of London, September (Taylor and Francis).—Himmel und Erde, September (Berlin).—Journal of the Asiatic Society of Bengal, Vol. lxiv Part 2, No. 2 (Calcutta).—Journal of the Franklin Institute, September (Philadelphia).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, Fourth Series, Vol. 9, Nos. 3, 4, 5, (Manchester).—American Journal of Science, September (New Haven).

CONTENTS.

THURSDAY, SEPTEMBER 19, 1895.

THE BRITISH ASSOCIATION.

WEDNESDAY.

THE British Association meeting at Ipswich has now practically come to an end. The stream of strangers which set towards the town a week ago shows signs of retiring, and, in the course of a day or two, the ancient and interesting county town of Suffolk will have returned to its normal condition. The meeting has been a very pleasant one for all, and the delightful weather of the past week has naturally attracted a large attendance at each of the many enjoyable excursions to places of interest in the surrounding country. The Association has often met in places far richer in educational and scientific institutions than Ipswich, but it has rarely met in a centre within easy reach of picturesque scenery offering more facilities for geological observation, or possessing a greater abundance of objects of interest to students of antiquities. This, combined with the fact that papers of extreme value have been communicated to each of the Sections, will make the meeting memorable to all who have attended it. As we shall follow our usual custom of giving reports of the work done in the Sections, it is unnecessary here to do more than refer to one or two of the papers and discussions which have excited general interest.

The subject of scientific research was brought up in Section A by Sir Douglas Galton's description of the Reichanstalt, Charlottenburg. After giving a full account of the construction, endowment, and management of that institution, which has for its object "the development of pure scientific research and the promotion of new applications of science for industrial purposes," it was pointed out that, in this country, there is no Government department which approximates to it. Recognising our deficiency in this respect, the suggestion was made that a committee of inquiry take the matter up, with the idea of formulating some definite proposal for the establishment of a central institution where standardising and research could be carried on without interruption. If the ideas with reference to such an institution should take tangible shape, as we sincerely hope they will, the Ipswich meeting will be remarkable in the annals of the Association as one from which a new departure in national enterprise began.

The joint meeting of Sections A and B, on Friday, was marked by two important communications on argon and helium. By methods which command the admiration of every one who can appreciate scientific inquiry, Lord Rayleigh showed how he had measured the refraction and viscosity of the two new gases. The refractive index of argon turns out to be 0·961, while that of helium appears to be as low as 0·146; both being compared with dry air. With the viscosity of dry air as the standard of comparison, those of argon and helium were respectively 1·21 and 0·96. Another interesting matter referred to by Lord Rayleigh in the course of his communication was the nature of the gas from the mineral spring at Bath. Some months ago, before the discovery of terrestrial helium, Lord Rayleigh and Prof. Ramsay examined samples of that gas for argon, but without finding the new element. The results were such, however, that an examination of the gas for helium was lately undertaken, and Lord Rayleigh was able to say that he had proved spectroscopically that helium really exists in the Bath gas. The question as to the nature of helium itself was elucidated by Prof. Runge in his contribution to the discussion of "the evidence to be gathered as to the simple or compound character of a gas from the constitution of its spectrum." It may be remembered that a short time ago, Prof. Runge contributed to these columns an article on the analysis of spectra by investigation of the periodic distribution of wave-lengths. He took the spectrum of lithium as a typical example of a spectrum which could be resolved into two spectra, the lines in each of which were connected by a simple formula. Taking his own observations of the spectrum of helium, Prof. Runge showed that helium is not an element but consists of two, and not more than two, elements. The conclusion is arrived at because the helium spectrum can be resolved into two sets of lines each apparently distinct from the other.

Of all the Sections, those of Geography and Anthropology have attracted the largest attendance, owing doubtless to the fact that the subjects dealt with could be easily followed, and are of general interest. But, besides the more or less popular papers of a resurrectionary character, a large number of distinctly new subjects have been brought up and discussed. The difficulty has been to find time for the long lists published in each day's *Journal*, and this difficulty is increased by the apparent inability of some of the readers of papers to express their conclusions in concise language. On account of the lack of this quality, the time for discussions has in several cases been very limited, and thus the first aim of a meeting of scientific men has been defeated.

At a meeting of the General Council, the question of Antarctic exploration was brought forward by the Royal Geographical Society, with a view to co-operation, and to the undertaking being unanimously advocated by the scientific societies of Great Britain and Ireland. The Council expressed their sympathy with, and approval of, the effort which was being made to organise an expedition for the exploration of the Antarctic Sea, but did not consider that any further action could usefully be taken by them at present.

As to the official affairs of the Association, Prof. Schäfer has been elected General Secretary in the place of Sir Douglas Galton, the present President. Sir W. H. Flower has been elected to represent the Association at the International Congress of Zoology at Leyden.

The retiring members of the Council were Prof. Lankester, Prof. Liveing, Mr. Preece, Prof. Reinold, and Prof. J. J. Thomson; and the new members elected to serve on the Council were Prof. Vernon Harcourt, Prof. Poulton, Prof. W. N. Shaw, Mr. Thiselton-Dyer, and Prof. J. M. Thomson.

The General Committee resolved on Monday that Sir Joseph Lister be appointed President-elect for the meeting at Liverpool next year. Prof. Herdman, Mr. J. C. Thompson, and Mr. W. E. Willink were appointed local secretaries for that meeting, and Mr. R. Bushell local treasurer. The Vice-Presidents-elect nominated for the meeting were the Lord Mayor of Liverpool (1896), the Earl of Sefton, the Lord-Lieutenant of the County of Lancaster, the Earl of Derby, Sir W. B. Forwood, Sir H. E. Roscoe, Mr. W. Rathbone, and Mr. W. Crookes. An invitation to hold the meeting in 1897 in Toronto, supported by cordial letters from British Columbia, from the University of Toronto, and Colleges of Manitoba, was accepted.

The following is a synopsis of the grants of money appropriated to scientific purposes by the General Committee this morning. The names of the members entitled to call on the General Treasurer for the respective grants are prefixed:—

	£	s.	d.
Mathematics and Physics.			
Prof. Carey Foster—Electrical Standards (and unexpended balance in hand)	£5	0	0
Mr. G. J. Symons—Photographs of Meteorological Phenomena...	15	0	0
Lord Rayleigh—Mathematical Tables (unexpended balance)			
*Mr. G. J. Symons—Seismological Observations .	80	0	0
Dr. E. Atkinson—Abstracts of Physical Papers ..	100	0	0
*Rev. R. Harley—Calculation of Certain Integrals (renewed)	15	0	0
Prof. S. P. Thompson—Uniformity of Size of Pages of Transactions, &c. (renewed)	5	0	0
Sir G. G. Stokes—Solar Radiation	30	0	0
Chemistry.			
Sir H. E. Roscoe—Wave-length Tables of the Spectra of the Elements	10	0	0
Dr. T. E. Thorpe—Action of Light upon Dyed Colours	5	0	0
Prof. J. E. Reynolds—Electrolytic Quantitative Analysis (renewed)	10	0	0
Prof. R. Warrington—The Carbohydrates of Barley Straw	50	0	0
Prof. R. Meldola—Report of the Discussion on the Relation of Agriculture to Science ...	5	0	0
Geology.			
*Prof. E. Hull—Erratic Blocks	10	0	0
*Prof. T. Wiltshire—Palæozoic Phyllopoda ..	5	0	0
Mr. J. Horne—Shell-bearing Deposits at Clava, &c.	10	0	0
Dr. R. H. Traquair—Eurypterids of the Pentland Hills	5	0	0
Prof. T. G. Bonney—Investigation of a Coral Reef by Boring and Sounding (renewed)	10	0	0
*Prof. A. H. Green—Examination of the Locality where the Cetiosaurus in the Oxford Museum was found (£20 renewed)	25	0	0
Sir John Evans—Palæolithic Deposits at Hoxne ..	25	0	0
Sir W. H. Flower—Fauna of Singapore Caves .	40	0	0
T. F. Jamieson—Age and Relation of Rocks near Moreseat, Aberdeen	10	0	0
Zoology.			
Dr. P. L. Sclater—Table at the Zoological Station, Naples	100	0	0
*Mr. G. C. Bourne—Table at the Biological Laboratory, Plymouth (£5 renewed) ...	15	0	0
*Prof. W. A. Herdman—Zoology, Botany, and Geology of the Irish Sea (partly renewed) .	50	0	0
*Dr. P. L. Sclater—Zoology of the Sandwich Islands	100	0	0
Dr. P. L. Sclater—African Lake Fauna	100	0	0
Prof. W. A. Herdman—Oysters under normal and abnormal environment	40	0	0
Geography.			
*Mr. E. G. Ravenstein—Climatology of Tropical Africa	10	0	0
Mechanical Science.			
Prof. A. B. W. Kennedy—Calibration and comparison of measuring instruments (£25 renewed)	30	0	0
Mr. W. H. Preece—Introduction of the B.A. Small Screw Gauge	10	0	0
Anthropology.			
*Prof. E. B. Tylor—North-Western Tribes of Canada (£76 15s. renewed)	100	0	0
*Dr. R. Munro—Lake Village at Glastonbury (£5 renewed)	30	0	0
*Sir J. Evans—Exploration of a Kitchen-midden at Hastings (unexpended balance)			
*Mr. E. W. Brabrook—Ethnographical Survey (£20 renewed)	40	0	0
*Sir Douglas Galton—Mental and Physical Condition of Children	10	0	0
Physiology.			
Prof. J. G. McKendrick—Physiological Applications of the Phonograph	25	0	0
Corresponding Societies.			
*Prof. R. Meldola—For preparing Report	30	0	0
	£1160	0	0

* Reappointed.

PRESIDENTS' ADDRESSES (*continued*).

SECTION C.

GEOLOGY.

Underground in Suffolk and its Borders.

Opening Address by W. Whitaker, B.A., F.R.S., F.G.S.

When the British Association revisits a town it is not unusual for the Sectional Presidents to refer to the addresses of their local predecessors, and to allude to the advance of their science since the former meeting. I have at all events tried to follow this course, with the sad result of having to chronicle a falling back rather than an advance in our methods of procedure; for at the meeting of 1851 all the Sectional Presidents had the wisdom not to give an address, and of all the inventions of later years I look upon the presidential address as perhaps the worst.

Had I the courage of my opinion I should not now trouble you; but an official life of over thirty-eight years has led me to do what I am told to do, and to suppress my own ideas of what is right. After all it is the fault of the Sections themselves that they should suffer the evil of addresses. They could disestablish the institution without difficulty.

On these occasions it is not usual to allude to the personal losses our science has had in the past year; but there are times when the lack of a familiar presence can hardly be passed over, and since we last met we have lost one of our most constant friends, who had served us long and well, and had been our Secretary for a far longer time than any other holder of that office. When we were at Oxford last summer none of us could have thought that it was our last meeting with William Topley.

I do not now mean to say anything on the origin or on the classification of the various divisions of the Crag and of the Drift that occur so plentifully around us, and form the staple interest of East Anglian geology. These subjects, which are the more interesting from being controversial, I leave to my brother-hammerers, and without claiming the credit of magnanimity in so doing, having said what I had to say on them in sundry Geological Survey Memoirs. The object of this address is to carry you below the surface, and to point out how much our knowledge of the geology of the county in which we meet has been advanced by workers in another field, by engineers and others in their search for water. As far as possible allusion will be made only to work in Suffolk; but we must occasionally invade the neighbouring counties.

This kind of evidence has chiefly accumulated since the meeting of the Association at Ipswich, in 1851; for of the 476 Suffolk wells of which an account, with some geologic information, has been published, only sixty-eight were noticed before that year, all but two of these being in a single paper. The notes on all these wells are now to be found in twelve Geological Survey Memoirs that refer to the county. Number alone, however, is not the only point, and many of the later records are marked by a precision and a detail rarely approached in the older ones. It should be stated that in the above and in the following numbers strict accuracy is not professed, nor is it material. A slight error in the number of the wells, one way or the other, would make practically no difference to the general conclusions.

Now let us see how these records affect our knowledge of the various geologic formations, beginning with the newest and working downward.

The Drift.

Under this head, as a matter of convenience for the present purpose, we will include everything above the Chillesford Clay. There is no need for refinement of classification, and the thin beds that come in between that Clay and the Drift in some parts do not affect the evidence we have to deal with.

As a matter of fact it is only from wells that we can tell the thickness of the Drift over most of the great plateau that this formation chiefly forms; open sections through a great thickness of Drift, to its base, are rare, except on the coast.

There is often some doubt in classifying the beds, the division between Drift and Crag being sometimes hard to make in sections of wells and borings; but from an examination of the records of these Suffolk sections that pass through any part of the Drift Series (as defined above) we find that no less than 173 show a thickness of 50 feet and upward, whilst of these 34 prove no less than 100 feet of Drift, many reaching to much more. Of the two that are said to show a thickness of over 200 feet and the one other said to be more than 300 feet deep in Drift, we

can hardly feel certain: but such amounts have been recorded with certainty as occurring in the neighbouring county of Essex.

These great thicknesses (chiefly consisting of Boulder Clay) show the importance of the Drift, and the impossibility of mapping the formations beneath with any approach to accuracy, on the supposition that the Drift is stripped off, as is the case in the ordinary geologic map. The records also show the varying thickness of the Drift, and how difficult it often is therefore to estimate the thickness at a given spot. Sometimes the sections seem to point to the existence of channels filled with Drift, such as are found also in Essex and in Norfolk; and it may be noted that in the northern inland part of the former county, one of these channels has been traced, though of course not continuously, for some eleven miles along the valley of the Cam, and at one place to the depth of 340 feet (or nearly 140 below sea-level), the bottom of the Drift moreover not having been reached even then. A channel of this sort seems to occur close to us, in the midst of the town of Ipswich, where, by St. Peter's, one boring has pierced 70 feet of Drift, and another 127, in ground but little above the sea-level.

As the Drift sands and gravels, that in many places occur below the Boulder Clay, often yield a fair amount of water, the proof of their occurrence and of the thickness of the overlying clay is of some practical good.

The Crag.

On this geologic division we have a less amount of information, as would be expected from the fact that it is not nearly so widespread as the Drift, and this information is confined to the Upper, or Red, Crag, the Lower, or Coralline, Crag occurring only over a very small area, and no evidence of its underground extension being given by wells.

What we learn of the Red Crag, however, is of interest, several wells having proved that it is far thicker underground than would have been supposed from what is seen where its base crops out. One characteristic, indeed, of this sandy deposit, in the many parts where it can be seen from top to bottom, is its thinness, as in such places it rarely reaches a thickness of 40 feet. But, on the other hand, wells at Hoxne seem to prove more than 60 feet of Crag, whilst at Saxmundham the formation is 100 feet thick, and at Leiston and Southwold over 140. Further north, just within the border of Suffolk, there is, at Beccles, a thickness of 80 feet of sand, or, with the overlying Chillesford Clay, a total of 95. Our underground information has, then, trebled the known thickness of the Upper Crag of Suffolk.

It has also shown that at some depth underground the colour-name is a misnomer, the shelly sands being light-coloured and not red. This is the case too with some other deposits, which owe their reddish-brown colour at the surface to peroxide of iron. Presumably the iron-salt is in a lower state of oxidation until it comes within reach of surface-actions. This seems to point to the risk of taking colour as the mark of a geologic formation.

Eocene Tertiaries.

Below the Crag there is a great gap in the geologic series, and we come to some of the lower of the Tertiary formations, about which little had been published, as regards Suffolk, before the work of the Geological Survey in the county. It seems as if the special interest in the more local Crag had led observers to neglect these beds, which had been amply noticed in other parts.

We have records of more than forty wells in Suffolk that are partly in these deposits, and of these thirty-six reach down to the Chalk, twenty giving good sections from the London Clay to the Chalk. The thickness of the Lower London Tertiaries (between those formations) thus proved varies from 30 to 79½ feet, the higher figure being much greater than anything shown at the outcrop. The greatest recorded thickness is at Leiston, where, moreover, the top 26 feet of the 79½ may belong to the uppermost and most local of the three divisions of the series, the Oldhaven Beds, of very rare occurrence in the county. The next greatest thickness is at Southwold, where the whole has been classed as Reading Beds (the persistent division), though here and elsewhere it is possible that the underlying Thanet Beds are thinly represented. It is noteworthy that at both these places, where the Lower London Tertiaries are thick, they are also at a great depth, beginning at 252½ and 218 feet respectively, which looks as if, like the Crag, they thickened in their underground course away from the outcrop.

The important evidence given by these wells, however, is not as regards thickness; it is to show the underground extent of the older Tertiary beds, beneath the great sheet of Crag and Drift that prevents them from coming to the surface north-eastward from the neighbourhood of Woodbridge. It is clear that over this large tract we can know nothing of the beds beneath the Crag otherwise than from wells and borings: and, until these were made, our older geologic maps cut off the older Tertiary beds far south of the parts to which we now know that they reach, though hidden from our sight. No one, for instance, would have imagined many years ago that at Southwold the Chalk would not be touched till a boring had reached the depth of 323 feet, or some 280 below sea-level, nor that at Leiston those figures would have been about 297 and 240.

It is from calculations based on the levels of the junction of the Chalk and the Tertiary beds in many wells that the line engraved on the Geological Survey map as the probable boundary of the latter beds under the Crag and Drift has been drawn. From what has gone before, however, as to the great irregularity in the thickness of the Drift, it is clear that this line must be taken only as approximate, and open to correction as further evidence is got; albeit the junction of the Chalk and the Tertiary beds is found to be here, as elsewhere, fairly even, along an inclined plane that sinks towards the coast.

Cretaceous Beds.

Though the *Chalk* is reached by very many wells, yet we get less information about it, by reason of its great thickness. Moreover, the great amount of overlying beds in many cases is a bar to deep exploration.

Of our Suffolk wells there are forty which go through 100 feet or more of Chalk. Of these twenty go through 200 feet or more, half of these to 300 or more, and again half of the ten to 400 or more, a very exact piece of geometric progression, or more strictly, retrogression. Although two wells pass through the great thickness of more than 800 feet of Chalk, yet neither of them gives us the full thickness of the formation; for the 816 feet at Landguard Fort do not reach to the base, whilst the 843 (or 817) feet at Combs, near Stowmarket, do not begin at the top.

As in no case yet recorded has the Chalk been pierced from top to bottom in Suffolk (a defect that will be supplied during this meeting by the description of the Stutton boring), that is to say, no boring has gone from the overlying older Tertiary beds to the underlying Gault, we must now, therefore, cross the border of the county to get full information as to the thickness of the Chalk; and we have not far to go, for the well-known Harwich boring passes through the whole of the Chalk, proving a thickness of 890 feet. It is almost certain, indeed, that this should be given as a few feet more, for the 22 feet next beneath, which have been described as Gault mixed with Greensand, is probably in part the green clayey glauconitic base of the Chalk Marl. We may fairly add to this number 5 feet (as also in the case of the Combs boring), and may say that, in round numbers, the Chalk reaches a thickness of about 900 feet in the south-eastern part of Suffolk. Toward the northern border of the county it is probably more, as the deep boring at Norwich passes through nearly 1160 feet of Chalk, and that without beginning at the top of the formation.

Of our recorded Suffolk wells only three reach the base of the Chalk, at Mildenhall, Culford and Combs; consequently we have little knowledge of the divisions of the Chalk. These divisions, indeed, are of comparatively late invention, having been evolved since the publication of many of the deep sections that have been referred to.

If the Upper Chalk at Harwich goes as far down as the flints, then we must allow it to be 690 feet thick, leaving little more than 200 for the Middle and Lower Chalk together. At Landguard Fort, from the same point of view, the Upper Chalk would certainly be 500 feet thick, and one cannot say how much more.

At Combs, on the other hand, flints have been recorded as present only in the top 27 feet of the Chalk; but whilst this may have been owing in part to the boring having passed between fairly scattered nodules, and in part, perhaps, to insufficient care in observation, at Harwich it is possible that some flints may have been carried down in the process of boring.

What evidence we have tends to show, however, that the Upper Chalk forms a good deal more than half, and perhaps about two thirds, of the formation, the Middle and Lower Chalk

being rather thin. This agrees with what is found in other parts where the Chalk is thick, extra thickness being chiefly due to the highest division. The glauconitic marly bed at the base seems to be well developed and to be underlain by the Gault clay; so that we have no good evidence of the occurrence of Upper Greensand. This division may be thinly represented at Mildenhall, but it is difficult to classify some of the beds passed through in the old boring there.

As far as the *Gault* is concerned, little, of course, is known; yet that little points to this formation being unusually thin, presumably only 73 feet from top to bottom at Culford, and probably not more than between 50 and 60 at and near Harwich. In the north-western part of the neighbouring county of Norfolk it is well known to be still less, the clay thinning out northward along the outcrop, until at last there is nothing but a few feet of Red Chalk between the carstone of the Lower Greensand and the Chalk. The Gault being of much greater thickness around and under other parts of the London Basin, this thinning in Norfolk and Suffolk is noteworthy. The absence of the more inconstant Upper Greensand is to be expected in most places, and calls for no remark: it may, however, be noted that geologists are coming to the conclusion that these two divisions are really parts of one formation, and one result of this geologic wedding is for the inconstancy of one partner to be greatly compensated by the constancy of the other.

The *Lower Greensand* has been found in one deep boring only, at Culford, in the western part of the county, where it is represented by $32\frac{1}{2}$ feet of somewhat exceptional beds. This slight thickness prepares us for underground thinning, and in the far east of the county the formation is presumably absent, there being no trace of it at Harwich or at Stutton.

With the Cretaceous beds we pass from the regular orderly succession of geological formations; indeed, it may be said that when we reach the base of the Gault we pass out of the region of facts into the realm of speculation.

We have come, then, to perhaps the most interesting problem in the geology of the Eastern Counties, to the consideration of the question, What rocks underlie the Cretaceous beds at great depths? In dealing with this I must ask your patience for frequent excursions outside our special district, and sometimes to lead far away from it.

Beyond the outcrop of the lower beds of the Cretaceous Series in Cambridgeshire and Norfolk, we find of course a powerful development of the great Jurassic Series; but the only two recorded deep borings in and near Suffolk that have pierced through the Cretaceous base, at Culford on the north-west and at Harwich on the south-east, show not a trace of anything Jurassic: they pass suddenly from Cretaceous into far older rocks. And here a paper that is to be brought before you must be anticipated, to a slight extent, by adding that the trial-boring at Stutton shows just the same thing, the Gault resting directly on a much older rock, which cannot be classed as of Secondary age.

There is no need now to discuss the literature of the old rocks underground in south-eastern England, that has often been done. We may take the knowledge of what has been shown by the various deep borings as common property, and may use it freely, without troubling to state the source of each piece of information, and I will not therefore burden this address with references. I had indeed thought of supplementing a former account by noticing the later literature of the subject; but decided to spare you from the infliction, and myself from the trouble of inflicting: though it may be convenient to add, in the form of an Appendix, a list of the chief papers on the subject that have been published since the question was discussed at length in 1889, in an official memoir on the geology of London, and to supply some omissions in that work. Nor do I propose to enter into any special criticism of papers on the subject that have appeared of late years; this is hardly the occasion for controversy, which may well be put off to a more convenient season. Some general remarks, however, I shall have to make after putting the facts before you.

There are ten deep borings reaching to old rocks in the London Basin, of which accounts have been published. We know that in four of these (Meux's, Streatham, Richmond and Dover) Jurassic beds separate those rocks from the Cretaceous beds, whilst there are six in which these last rest direct on old rocks (Ware, Cheshunt, Kentish Town, Crossness, Culford, and Harwich). Stutton of course makes a seventh. The Jurassic r[illegible] any in the northern borings, either in London or still further southward, and in one case only (Dover) is there any considerable thickness of these: in the other three they are from $38\frac{1}{2}$ to $87\frac{1}{2}$ feet thick. As far as regards Suffolk and its borders we may therefore disregard them, except in the far west, near their outcrop, and we may pass on to consider the older rocks that have been found.

So far the occurrence, next beneath the Cretaceous or Jurassic beds, of Silurian, Devonian, and Carboniferous rocks has been proved, whilst in some cases we are still doubtful as to the age of the old rocks found. In five cases distinctive fossils have been found (Ware, Cheshunt, Meux's, Dover, and Harwich), but in five others they have not (Kentish Town, Crossness, Richmond, Streatham, and Culford), and it is in the latter group too that the character of the beds leaves their age in doubt. So far another must be added to these, as no fossil has yet been found in the old rocks at Stutton.

Of the above ten deep borings in the London Basin (using that term in the widest sense, as including the Chalk tract that everywhere surrounds the Tertiary beds) we owe nine to endeavours to get water from deep-seated rocks, and in addition to these nine we have several other deep borings, which though not carried through to the base of the Secondary rocks, yet give us much information concerning those beds (at Holkham, Norwich, Combs, Winkfield, London, Loughton, Chatham, and Dover). In one case only, that of Dover, has the work been done for the purpose of exploration, but now, after a few years' interval, a second trial has been made at Stutton.

Now both of these borings were started for a much more definite object than merely to prove the depth to older rocks, or the thickness of the Cretaceous and Jurassic Series. There is one particular division of those older rocks that has a distinct fascination for others than geologists. We, happily, are content to find anything and to increase our knowledge in any direction, but naturally those who are not geologists, as well as many who are, like to find something of immediate practical value. As already shown, we owe much knowledge of the underground extension of formations to explorations for water; it has now become the turn of geologists to help those who would like to find that much less general, though nearly as needful and certainly more valuable thing, *coal*.

The first place to suggest itself to those geologists who had worked at this question, as a good site for trial, was the neighbourhood of Dover, and for various good reasons. The trial has been made, and successfully, several hundred feet of Coal Measures having been found, without reaching their base, but with several beds of workable coal.

Beyond that neighbourhood, however, geologists are not in such accord, and generally speaking, fairly good reasons can be given both for and against the selection of many tracts for trial, except in and near London, where no geologists would recommend it, from the evidence in our hands.

Let us then shortly review the evidence that we have on the underground extension of the older rocks in south-eastern England, with a view of considering the question of the possibility of finding Coal Measures in any of the folds into which those rocks have probably, nay almost certainly, been thrown.

The area within which the borings that reach older rocks in the London Basin is enclosed is an irregular pentagon, from near Dover, on the south-east, to Richmond on the west, thence to Ware, thence to Culford on the north, thence to Harwich, and thence southward to Dover, the greatest distance between any borings being from Dover to Culford, about eighty-six miles. It is therefore over a large tract, extending of course beyond the boundaries sketched above, that we have good reason to infer that older rocks are within reasonable distance of the surface, nowhere probably as much as 1000 feet, and mostly a good deal less.

We must now consider some evidence outside the tract hitherto dealt with. Southward of the central and eastern parts of the London Basin we have evidence that the Lower Cretaceous beds thicken greatly, from what is seen over their broad outcrop between the North and South Downs. We know also, from the Dover and Chatham borings, that the Upper and Middle Jurassic beds come in to the south-east, whilst the Sub-Wealden Exploration, near Battle, proves that those divisions thicken greatly southward, the latter not having been bottomed at the depth of over 1900 feet, at that trial-boring.

Westward, however, near Burford in Oxfordshire, and some miles northward of the nearest part of the London Basin, Carboniferous rocks have been found at the depth of about 1180

feet, these being separated from the thick Jurassic beds (including therein the Liassic and Rhætic) by perhaps 420 of Trias. They consist of Coal Measures, which were pierced to the depth of about 230 feet.

In and near Northampton, north-eastward of the last site, and still further from the northern edge of the London Basin, the like occurs; but the beds found are older than the Coal Measures, and the Trias is thin, not reaching indeed to 90 feet in thickness, and being absent in one case. At one place, too, the Carboniferous beds have been pierced through, with a thickness of only 222 feet, when Old Red Sandstone was found, and in another place still older rock seems to have been found next beneath the Trias. The depth to the rocks older than the Trias, where they were reached, was 677, 738, and 790 feet, or respectively 395, 460, and 316 below sea-level. Some of these figures must be taken as somewhat approximate, though they are near enough to the truth for practical purposes.

A boring at Bletchley, to the south, reached granitic rocks at the depths of 378½ and 401 feet: but these rocks seem to be only boulders in a Jurassic clay: their occurrence, however, is suggestive of the presence of older rocks at the surface no great way off, in Middle Jurassic times.

Much further northward, at Scarle, south-west of Lincoln, the older rocks have been reached at the depth of about 1500 feet, all but 141 of which are Trias, and they begin with the Permian (which crops out some eighteen miles westward), the Carboniferous occurring after another 400 feet, and having been pierced to 130.

We have then evidence that over a large part of south-eastern England, reaching northward and westward of the London Basin, though the older rocks are hidden by a thick mantle of Jurassic, Cretaceous, and Tertiary beds, yet they seem to be rarely at a depth that would be called very great by the coal-miner. They are distinctly within workable depths wherever they have been reached.

There is no area of old rocks at the surface in our island, south of the Forth, in which Coal Measures are not a constituent formation. Truly, further north, in the great tract of Central and Northern Scotland there are no Carboniferous rocks; but we can hardly say that none ever occurred, at all events in the more southern parts. We know, though, that on the west and north Jurassic and Triassic beds rest on formations older than the Carboniferous.

It is not, however, to this more northern and distant tract that we should look for analogy to our underground plain of old rocks; rather should we look to more southern parts, to Wales and to central and northern England, where Coal Measures are of frequent occurrence. On the principle of reasoning from the known to the unknown, I cannot see why we should expect anything but a like occurrence of Coal Measures, in detached basins, in our vast underground tract of old rocks.

What, then, is the evident conclusion from what we know and from what we may reasonably infer? Surely that trials should be made to see if such hidden coal-basins can be found.

One trial has been made, and it has succeeded; the Dover boring has proved the presence of coal underground in Eastern Kent, along the line between the coal-fields of South Wales and of Bristol on the west, and those of Northern France and of Belgium on the east.

The long gap between the distant outcrops of the Coal Measures near Bristol and Calais has been lessened very slightly by the working of coal under the Triassic and Jurassic beds near the former place, but much more by our brethren across the narrow sea, the extent of the Coal Measures, beneath the Jurassic and Cretaceous beds, having not only been proved by the French and the Belgians along their borders, but the coal having been largely worked. At last, we too have still further decreased the gap, by the Dover boring, a work that I trust is to be followed by other work along the same line.

But is this the only line along which we are to search? Are we to conclude that the only coal-fields under our great tract of Cretaceous beds (where these are either at the surface or covered by Tertiary beds) are in Kent, Surrey, and other counties to the west? Have we no coal-fields but those of Bristol and of South Wales? The bounds of our midland and northern coal-fields have been extended by exploration beneath the New Red Series; are we to stop here and to assume that there can be no further underground extension of the Coal Measures south-eastward? This seems hardly a wise course, and is certainly a very unenterprising one. It seems to me rather that the right thing to be done is to try to find out the real state of things, by means of borings.

There are, of course, objectors in this as in other matters. Some may say that it is silly to try in Suffolk, and that Essex gives a better chance of success. Others, again, may prefer Norfolk. And yet others may argue that there is no chance of finding Coal Measures in any of those three counties. But I must confess my inability to understand this line of reasoning; the fact is that the data we have are few and far between, and that we want more. It is really of little use to bandy words, and I do not now mean to take up the matter in detail. We cannot get at the truth except by actual work; justification by faith will not hold in this case, still less justification by unfaith.

Let us hark back a little and call to mind what has happened in the past. I remember the time when certain geologists disbelieved in the possibility of the occurrence of Coal Measures anywhere in south-eastern England, it being argued that the formation thinned out before it could get so far eastward. Then this view was somewhat varied, and it was inferred, from certain observed facts, that even if Coal Measures did reach underground into these benighted parts, they would be without workable coal, and so practically useless.

Now for some years nothing occurred to upset the prophets of evil, that is to say, no fact came to light. There were not wanting inferences to the contrary, but it remained practically a matter of opinion. One day, however, the needful fact came, and the first boring made specially to test the question (at Dover) disproved both the above negative theories by finding Coal Measures with workable coal. Let us hope that a like result may happen in East Anglia, and that the pessimists may again be in the wrong.

We should not, however, fall into the opposite error, that of optimism. We must not expect an immediate success like that at Dover. We are here much further from any known coal-field. Advertisements of various wares sometimes tell us that "one trial will suffice," but it is not so in this case. We should not be content until many borings have been made, and we should not be despondent if, after sites have been selected to the best of our judgment, we begin with a set of borings that are unsuccessful in finding coal.

At the time of writing I cannot say that the Stutton boring is a success or a failure as far as coal is concerned, but I am quite ready to accept the latter without being discouraged. Whatever it is you may know during our meeting; it is certainly a success in the matter of reaching the old rocks at a depth of less than 1000 feet. We should remember that every boring is almost certain to give us some knowledge that may help in future work.

There is a further point, however, to be taken into account. A boring that may at first seem to be a failure, from striking beds older than the Coal Measures, may some day turn out otherwise. The coal-field along the borders of France and Belgium is sometimes affected by powerful and peculiar disturbances, by faults of comparatively gentle inclination (far removed from the usual more or less vertical displacements) which have thrown Coal Measures beneath older beds in large tracts. This is no mere theory, though advanced as such at first by some continental geologists, who have had the great satisfaction of seeing their theory adopted by practical men, and proved to be true, much coal being worked below the older beds that have been pushed above the Coal Measures by the over-thrust faults.

Our trial-work, of course, does not yet lead us to consider such disturbances as those alluded to. We have at first to assume a normal succession of formations, and not to carry on explorations in beds that can be proved to be older than the Coal Measures; but the time may come when it will be otherwise.

Another matter to which attention has been drawn by our foreign friends is an apparent general persistence of disturbances along certain lines, or in other words, the recurrence of disturbances in newer beds in those parts where earlier movements had affected older beds; so that, reasoning backward, where we see marked signs of disturbance for long distances in beds at or near the surface, there we may expect to find pre-existing disturbances of the older beds beneath. This, however, is a somewhat controversial question, and much remains to be done on it; but should it be proved as a general rule it may have much effect on our underground coal.

Finally, the question of the possibility of finding and of work-

[illegible] parts of south-eastern England is not merely of [illegible]; it is of national importance. The time must come when the coal-fields that we have worked for years will be more or less exhausted, and we ought certainly to look out ahead for others, so as to be ready for the lessening yield of those that have served us so well. It is on our coal that our national prosperity largely, one may say chiefly, depends, and, as far as we can see, will depend. Let us not neglect any of the bounteous gifts of nature, but let us show rather that we are ready to search for the treasures that may be hidden under our feet, and the finding of which will result in the continued welfare of our native land.

APPENDIX.—*[illegible] the Chief Papers on the Old Rocks Underground in South-Eastern England since 1889, when the [illegible] subject was treated of in the Memoir on the Geology of London, &c.*

Bertrand, Prof. M. Sur le Raccordement des Bassins houillers du Nord de la France et du Sud de l'Angleterre. *Annales des Mines* [illegible]. *Fed. Inst. Min. Eng.*, vol. v. (1893).

Brady, F. Dover Coal Boring. Observations on the Correlation of the Franco-Belgian, Dover and Somerset Coal-fields [illegible] 1892. Second Issue, with Additions, 1893. Notice by E. Lorieux in *Annales des Mines*, 1892.

Dawkins, Prof. W. B. The Discovery of Coal near Dover. NATURE, vol. [illegible], pp. 418, 410; *Iron and Coal Trades Gazette*; [illegible], vol. lvii. pp. 470–478. The Search for Coal in the South of England, *Proc. Roy. Inst.* (nine pages); NATURE, vol. xlii. pp. 319–322. The Discovery of Coal Measures near Dover, *Trans. Manchester Geol. Soc.*, vol. xx. pp. 502–517 (1890).

The Further Discovery of Coal at Dover and its Bearing on the Coal Question. *Trans. Manchester Geol. Soc.*, vol. xxi. pp. 456–474 (1892).

On the South-Eastern Coal-field at Dover, *Trans. Manchester Geol. Soc.*, [illegible], pp. 488–510; The Probable Range of the Coal Measures in Southern England, *Trans. Fed. Inst. Min. Eng.*, [illegible] (thirteen pages and plate) (1894).

Harrison, W. J. On the Search for Coal in the South-East of England; with Special Reference to the Probability of the Existence of a Coal-field beneath Essex (twenty-eight pages and [illegible]. 8vo. Birmingham (1894).

Irving, Rev. Dr. A. The Question of Workable Coal Measures [illegible] Essex. *Herts and Essex Observer*, July 14, 1894.

Marr, J. E. On the Underground Geology of London. [illegible], No. 335, pp. 251–254; No. 337, pp. 11–15 (1892, 1893).

Rücker, Prof. A. W., and Prof. T. E. Thorpe. Magnetic Survey of the British Isles. *Phil. Trans.*, vol. clxxxi. (see pp. 280, [illegible] 14) (1891); A popular account by Prof. Rucker [illegible] Underground Mountains, *Good Words*, January [illegible] 1895.

Topley, W. Coal in Kent. *Trans. Fed. Inst. Min. Eng.*, vol. [illegible] pp. 370–387 (1892).

Whitaker, W. Coal in the South-East of England. *Journ.* [illegible], pp. 543–557; Suggestions on Sites for [illegible] in the South-East of England, *Geol. Mag.*, dec. iii. vol. [illegible] pp. 514–516 (1890).

Whitaker, W., and A. J. Jukes-Browne. On Deep Borings at [illegible] Winfield, with Notes on those at Ware and [illegible], *Quart. Journ. Geol. Soc.*, vol. l. pp. 488–514 (1894).

The Eastern Counties' Coal Boring and Development Syndicate [illegible] Reports by T. A. Holmes, J. J. Taylor, and W. Whitaker (fifteen pages, 8vo, Ipswich), (1893). Partly [illegible] in *Essex Naturalist*.

Omitted from Notice in 1889.

[illegible] Is there Coal under London? *Science for All*, [illegible] 324–328.

[illegible] Sur l'Extension en Angleterre du Bassin houiller [illegible] *Ann. Soc. Géol. Belg.* t. x. *Bulletin*, pp. xcii.–[illegible].

[illegible] On the Probability of Finding Coal in the South-East of England, pp. 11–22 (8vo. Reigate). (1886).

Topley, W. On the Correspondence between some Areas of Apparent Elevation and the Thickening of Subjacent Beds. *Quart. Journ. Geol. Soc.*, vol. xxx. see pp. 186, 190–195. (1874). See also Memoir "The Geology of the Weald," pp. 241, 242, pl. vi. (1875).

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY WILLIAM A. HERDMAN, D.Sc., F.R.S., F.L.S., F.R.S.E., PROFESSOR OF NATURAL HISTORY IN UNIVERSITY COLLEGE.

THIS year, for the first time in the history of the British Association, Section D meets without including in the range of its subject-matter the Science of Botany. Zoology now remains as the sole occupant of Section D that "Fourth Committee of Sciences," as it was at first called, more than sixty years ago, when our subject was one of that group of biological sciences, the others being Botany, Physiology, and Anatomy. These allied sciences have successively left us. Like a prolific mother our Section has given rise one after another to the now independent Sections of Anthropology, Physiology, and Botany. Our subject-matter has been greatly restricted in scope, but it is still very wide—this year, when Section I, devoted to the more special physiology of the medical physiologist, does not meet, perhaps a little wider than it may be in other years, since we are on this occasion credited with the subject "Animal Physiology"—surely *always* an integral part of Zoology! It is to be hoped that this Section will always retain that general and comparative physiology which is inseparable from the study of animal form and structure. The late Wayntlete Professor of Physiology at Oxford, in his Newcastle address to this Section, said "that every appreciable difference in structure corresponds to a difference of function" (Burdon-Sanderson, "British Association Report" for 1889), and his successor, the present Waynflete Professor, has shown us "how pointless is structure apart from function, and how baseless and unstable is function apart from structure" (Gotch, "Presidential Address to Liverpool Biological Society," vol. ix., 1894) the "argument for the simultaneous examination of both" in that science of Zoology which we profess is, to my mind, irresistible.

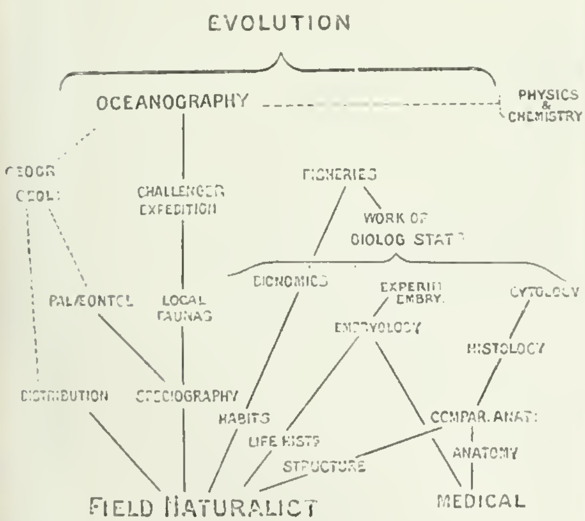
We include also in our subject-matter, besides the adult structure and the embryonic development of animals, their distribution both in space and time, the history and structure of extinct forms, speciography and classification, the study of the habits of animals and all that mass of lore and philosophy which has gathered around inquiries into instinct, breeding, and heredity. I trust that the discussion of matters connected with Evolution will always, to a large extent, remain with this Section D, which has witnessed in the past the addresses, papers, discussions, and triumphs of Darwin, Huxley, and Wallace.

When the British Association last met in Ipswich, in 1851, Section D, under the presidency of Prof. Henslow, still included Zoology, Botany, and Physiology, and a glance through the volumes of reports for that and neighbouring years recalls to us that our subject has undergone great and striking developments in the forty-four years that have elapsed. Zoology was still *pre-Darwinian* though Charles Darwin was then in the thick of his epoch-making work—both what he calls his "plain barnacle work" and his "theoretic species work" (see "Life and Letters," vol. i. p. 380). Although the cell-theory had been launched a decade before, zoologists were not yet greatly concerned with those minute structural details which have since built up the science of Histology. The heroes of our science were then chiefly those glorious field naturalists, observers, and systematists who founded and established on a firm basis British Marine Zoology. Edward Forbes, Joshua Alder, Albany Hancock, were then in active work. George Johnston was at his zoophytes, Bowerbank at sponges, Busk at polyzoa. Forbes' short brilliant career was nearly run. He probably did more than any of his contemporaries to advance marine zoology. In the previous year, at the Edinburgh meeting of the Association, he and his friend MacAndrew had read their classic reports ("British Association Report" for 1850, p. 192 *et seq.*), "On the Investigation of British Marine Zoology by Means of the Dredge," and "On South European Marine Invertebrata," which mark the high-water level reached at that date, and for some time afterwards, in the exploration of our coasts and the explanation of the distribution of our marine animals. At the Belfast meeting, which followed Ipswich, Forbes exhibited his great map of the distribution of marine life in "Homoiozoic Belts." In November 1854 he was dead, six months after his appointment to the goal of his ambition, the professorship at Edinburgh, where, had he lived, there can be no doubt he would,

with his brilliant ability and unique personality, have founded a great school of Marine Zoology.

To return to the early fifties, Huxley—whose recent loss to science, to philosophy, to culture, we, in common with the civilised world, now deplore—at that time just returned from the memorable voyage of the *Rattlesnake*, was opening out his newly acquired treasures of comparative anatomy with papers on Siphonophora and on Sagitta, and one on the structure of Ascidians, in which he urged—fourteen years before Kowalevsky established it on embryological evidence in 1866—that their relations were with Amphioxus, as we now believe, rather than with the Polyzoa or the Lamellibranchiata, as had formerly been supposed. Bates was then on the Amazons, Wallace was just going out to the Malay Archipelago, Wyville Thomson, Hincks, and Carpenter, the successors of Forbes, Johnston, and Alder, were beginning their life-work. Abroad that great teacher and investigator, Johannes Müller, was training amongst his pupils the most eminent zoologists, anatomists, and physiologists of the succeeding quarter century. In this country, as we have seen, Huxley was just beginning to publish that splendid series of researches into the structure of nearly all groups in the animal kingdom, to which comparative anatomy owes so much.

In fact, the few years before and after the last Ipswich meeting witnessed the activity of some of the greatest of our British zoologists—the time was pregnant with work which has since advanced, and in some respects revolutionised our subject. It was then still usual for the naturalist to have a competent knowledge of the whole range of the natural sciences. Edward Forbes, for example, was a botanist and a geologist, as well as a zoologist. He occupied the chair of Botany at King's College, London, and the presidential chair of the Geological Section of the British Association at Liverpool in 1854. That excessive specialisation, from which most of us suffer in the present day, had not yet arisen; and in the comprehensive, but perhaps not very detailed, survey of his subject taken by one of the field naturalists of that time, we find the beginnings of different lines of work, which have since developed into some half-dozen distinct departments of zoology, are now often studied independently, and are in some real danger of losing touch with one another (see diagram).



The spendid anatomical and "morphological" researches of Huxley and Johannes Müller have been continued by the more minute histological or cellular work rendered possible by improvements of the microtome and the microscope, until at last in these latter years we investigate not merely the cellular anatomy of the body, but *the anatomy of the cell*—if indeed we are permitted to talk of "cell" at all, and are not rather constrained to express our results in terms of "cytomicrosomes," "somacules," or "idiosomes," and to regard our morphological unit, the cell, as a symbiotic community containing two colonies of totally dissimilar organisms (see Watasé in "Wood's Holl Biological Lectures," 1893). To such cytological investigations may well be applied Lord Macaulay's aphorism, "A point which yesterday was invisible is its goal to-day, and will be its starting-point to-morrow."

Somewhat similar advances in methods have led us from the life-histories studied of old to the new and fascinating science of embryology. The elder Milne-Edwards and Van Beneden knew that in their life-histories Ascidians produced tadpole-like young. Kowalevsky (1866) showed that in their embryonic stages these Ascidian tadpoles have the beginnings of their chief systems of organs formed in essentially the same manner and from the same embryonic layers as in the case of the frog's tadpole or any other typical young vertebrate; and now we are not content with less than tracing what is called the "cell-lineage" of such Ascidian embryos, so as to show the ancestry and descendants, the traditions, peculiarities of, and influences at work upon each of the embryonic cells—or areas of protoplasm—throughout many complicated stages. And there is now opening up from this a great new field of experimental and "mechanical" embryology, in which we seek the clue to the explanation of particular processes and changes by determining under what conditions they take place, and how they are affected by altered conditions. We are brought face to face with such curious problems as, Why does a frog's egg, in the two-celled stage, of which one-half has been destroyed, develop into half an embryo when it is kept with one (the black) surface uppermost, and into—not half an embryo, but—a whole embryo of *half the usual size* if kept with the other (the white) surface upwards. Apparently, according to the conditions of the experiment, we may get half embryos or whole embryos of half size from one of the first two cells of the frog's egg.[1]

One of the most characteristic studies of the older field naturalists, the observation of habits, has now become, under the influence of Darwinism, the "Bionomics" of the present day, the study of the relations between habit and structure and environment—a most fascinating and promising field of investigation, which may be confidently expected to tell us much in the future in regard to the competition between species, and the useful or indifferent nature of specific characters.

Other distinct lines of zoological investigation, upon which I shall not dwell, are geographical distribution and palæontology—subjects in which the zoologist comes into contact with, and may be of some service to his fellow-workers in geology. And there still remains the central avenue of the wide zoological domain—that of speciography and systematic zoology—which has been cultivated by the great classifiers and monographers from Linnæus to Hæckel, and has culminated in our times in the magnificent series of fifty quarto volumes, setting forth the scientific results of the *Challenger* Expedition; a voyage of discovery comparable only in its important and wide-reaching results with the voyages of Columbus, Gama and Magellan at the end of the fifteenth century. It is now so long since the *Challenger* investigations commenced that few I suppose outside the range of professional zoologists are aware that although the expedition took place in 1872 to 1876, the work resulting therefrom has been going on actively until now—for nearly a quarter of a century in all—and in a sense, and a very real one, will never cease, for the *Challenger* has left an indelible mark upon science, and will remain through the ages exercising its powerful, guiding influence, like the work of Aristotle, Newton, and Darwin.

Most of the authors of the special memoirs on the sea and its various kinds of inhabitants, have interpreted in a liberal spirit the instructions they received to examine and describe the collections entrusted to them, and have given us very valuable summaries of the condition of our knowledge of the animals in question, while some of the reports are little less than complete monographs of the groups. I desire to pay a tribute of respect to my former teacher and scientific chief, Sir Wyville Thomson, to whose initiative, along with Dr. W. B. Carpenter, we owe the first inception of our now celebrated deep-sea dredging expeditions, and to whose scientific enthusiasm, combined with administrative skill, is due in great part the successful accomplishment of the *Lightning*, the *Porcupine*, and the *Challenger* Expeditions. Wyville Thomson lived long enough to superintend the first examination of the collections brought home, their division into groups, and the allotment of these to specialists for description. He enlisted the services of his many scientific friends at home and abroad, he arranged the general plan of the work, decided upon the form of publication, and died in

[1] See Morgan, "Anat. Anzeig.," 1895, x. Bd. p. [illegible], and recent papers by Roux Hertwig, Born, and O. Schultze.

1882. After seeing the first ten or twelve zoological reports through the press.

Within the last few months have been issued the two concluding volumes of this noble series, dealing with a summary of the results, conceived and written in a masterly manner by the eminent editor of the reports, Dr. John Murray. An event of such first-rate importance in zoology as the completion of this great work ought not to pass unnoticed at this zoological gathering. I desire to express my appreciation and admiration of Dr. Murray's work, and I do not doubt that the Section will permit me to convey to Dr. Murray the congratulations of the zoologists present, and their thanks for his splendid services to science. Murray, in these "Summary" volumes, has given definiteness of scope and purpose, and a tremendous impulse, to that branch of science—mainly zoological—which is coming to be called

OCEANOGRAPHY.

Oceanography is the meeting ground of most of the sciences. It deals with botany and zoology, "including animal physiology"; chemistry, physics, mechanics, meteorology, and geology all contribute, and the subject is of course intimately connected with geography, and has an incalculable influence upon mankind, his distribution, characteristics, commerce, and economics. Thus oceanography, one of the latest developments of marine zoology, extends into the domain of, and ought to find a place in, every one of the Sections of the British Association.

Along with the intense specialisation of certain lines of zoology in the last quarter of the nineteenth century, it is important to notice that there are also lines of investigation which require an extended knowledge of, or at least make use of the results obtained from, various distinct subjects. One of these is oceanography, another is bionomics, which I have referred to above, a third is the philosophy of zoology, or all those studies which bear upon the theory of evolution, and a fourth is the investigation of practical fishery problems—which is chiefly an application of marine zoology. Of these four subjects—which while analytic enough in the detailed investigation of any particular problem, are synthetic in drawing together and making use of the various divergent branches of zoology and the neighbouring sciences—oceanography, bionomics, and the fisheries' investigation, are most closely related, and I desire to devote the remainder of this address to the consideration of some points in connection with their present position

Dr. Murray, in a few only too brief paragraphs at the end of his detailed summary of the results of the *Challenger* Expedition, which I have alluded to above, states some of the views, highly suggestive and original, at which he has himself arrived from his unique experience. Some of his conclusions are very valuable contributions to knowledge, which will no doubt be adopted by marine zoologists. Others, I venture to think, are less sound and well founded, and will scarcely stand the test of time and further experience. But for all such statements, or even suggestions, we should be thankful. They do much to stimulate further research, they serve, if they can neither be refuted nor established, as working hypotheses; and even if they have to be eventually abandoned, we should bear in mind what Darwin has said as to the difference in their influence on science between erroneous facts and erroneous theories. "False facts are highly injurious to the progress of science, for they often endure long; but false views, if supported by some evidence, do little harm, for every one takes a salutary pleasure in proving their falseness; and when this is done, one path towards error is closed, and the road to truth is often at the same time opened" (Darwin, "The Descent of Man," second edit. 1882, p. 606).

With all respect for Murray's work, and fully conscious of my own temerity in venturing to differ from one who has had such unrivalled experience of the sea and its problems, I am constrained to express my disagreement with some of his conclusions. And I am encouraged to do so by the belief that Murray will recognise that the best compliment which zoologists can pay to his work is to give it careful, detailed consideration, and discuss it critically. He will, I am sure, join me in the hope that, [illegible] or [illegible] prove the false ones, we may be able, by their elimination, to close a "path towards error," and possibly open the road to truth."

One of the points upon which Murray lays considerable stress, and to the consideration of which he devotes a prominent position in his "General Observations on the Distribution of Marine Organisms," is the presence of what he has called a "mud-line" around coasts at a depth of about one hundred fathoms. It is the point "at which minute particles of organic and detrital matters in the form of mud begin to settle on the bottom of the ocean." He regards it as the great feeding ground, and a place where the fauna is most abundant, and from which there have hived off, so to speak, the successive swarms or migrations which have peopled other regions—the deep waters, the open sea, the shallow waters and the estuaries, fresh waters, and land. Murray thus gives to his mud-line both a present and an historic importance which can scarcely be surpassed in the economy of life on this globe. I take it that the historic and the present importance stand or fall together—that the evidence as to the origin of faunas in the past is derived from their distribution at the present day, and I am inclined to think that Murray's opinion as to the distribution of animals in regard to the mud-line is not entirely in accord with the experience of specialists, and is not based upon reliable statistics. Murray's own statement is ("*Challenger* Expedition, Summary," vol. ii. p. 1433):—"A depth is reached along the continental shores facing the great oceans immediately below which the conditions become nearly uniform in all parts of the world, and where the fauna likewise presents a great uniformity. This depth is usually not far above nor far below the 100-fathom line, and is marked out by what I have elsewhere designated as the *Mud-line*. . . . Here is situated the great feeding ground in the ocean . . ." and he then goes on (p. 1434) to enumerate the Crustaceans, such as species of *Calanus*, *Euchæta*, *Pasiphæa*, *Crangon*, *Calocaris*, *Pandalus*, *Hippolyte*, many amphipods, isopods, and immense numbers of schizopods, which swarm, with fishes and cephalopods, immediately over this mud deposit. Now I venture to think that the experience of some of those who have studied the marine zoology of our own coasts does not bear out this statement. In the first place, our experience in the Irish Sea is that mud may be found at almost any depth, but is very varied in its nature and in its source. There may even be mud laid down between tide marks in an estuary where a very considerable current runs. A deposit of mud may be due to the presence of an eddy or a sheltered corner in which the finer particles suspended in the water are able to sink, or it may be due to the wearing away of a limestone beach, or to quantities of alluvium brought down by a stream from the land, or to the presence of a submerged bed of boulder clay, or even, in some places, to the sewage and refuse from coast towns. Finally, there is the deep-water mud, a very stiff blue-grey substance which sets, when dried, into a firm clay, and this is, I take it, the mud of which Dr. Murray writes. But in none of these cases, and certainly not in the last mentioned, is there in my experience or in that of several other naturalists I have consulted, any rich fauna associated with the mud. In fact, I would regard mud as supporting a comparatively poor fauna as compared with other shallow water deposits.

For practical purposes, round our own British coasts, it is still convenient to make use of the zones of depth marked out by Forbes. The first of these is the "Littoral zone," the space between tide marks, characterised by the abundance of sea-weeds, belonging to the genera *Lichina*, *Fucus*, *Enteromorpha*, *Polysiphonia*, and others, and by large numbers of individuals belonging to common species of *Balanus*, *Mytilus*, *Littorina*, *Purpura*, and *Patella* amongst animals. The second zone is the "Laminarian," which extends from low-water mark to a depth of a few fathoms, characterised by the abundant growth of large sea-weeds belonging to the genera *Laminaria*, *Alaria*, and *Himanthalia*, and by the presence of the beautiful red sea-weeds (Florideæ). There is abundance of vegetable food, and animals of all groups swarm in this zone, the numbers both of species and of individuals being very great. The genera *Helcion*, *Trochus*, and *Lacuna* are characteristic molluscan forms in our seas. Next comes Forbes' "Coralline" zone, badly so named, extending from about ten to forty or fifty fathoms or so. Here we are beyond the range of the ordinary sea-weeds, but the calcareous, coral-like Nullipores are present in places in such abundance as to make up deposits covering the floor of the sea for miles. Hydroid zoophytes and polyzoa are also abundant, and it is in this zone that we find the shell-beds lying off our coasts, produced by great accumulations of species of *Pecten*, *Ostrea*, *Pectunculus*, *Fusus*, and *Buccinum*, and forming rich feeding grounds for many of our larger fishes. All groups of marine animals are well represented in this zone, and *Antedon*, *Ophiothrix*, *Ophioglypha*, *Ebalia*, *Inachus*, and *Eurynome*, may be mentioned as characteristic genera. Lastly, there is what may be appropriately called the zone of deep mud (although Forbes

did not call it so), extending from some fifty fathoms down to (in our seas) one hundred or so. The upper limit of this zone is Murray's mud-line. We come upon it in the deep fjord-like sea-lochs on the west of Scotland, and in the Irish Sea to the west of the Isle of Man.

Now of these four zones, my experience is that the last—that of the deep mud—has by far the poorest fauna both in species and in individuals. The mud has a *peculiar* fauna and one of great interest to the zoologist, but it is not a *rich* fauna. It contains some rare and remarkable animals not found elsewhere, such as *Calocaris macandreae*, *Panthalis oerstedi*, *Lipobranchius jeffreysi*, *Brissopsis lyrifera*, *Amphiura chiajii*, *Isocardia cor*, and *Sagartia herdmani*; and a few striking novelties have been described from it of late years, but we have no reason to believe that the number of these is great compared with the number of animals obtained from shallower waters.

Dr. Murray not only insists upon the abundance of animals on the mud, and its importance as the great feeding ground and place of origin of life in the ocean, but he also (p. 1432) draws conclusions as to the relative numbers of animals taken by a single haul of the trawl in deep and shallow waters which can scarcely be received, I think, by marine zoologists without a protest. His statement runs (p. 1432): "It is interesting to compare single hauls made in the deep sea and in shallow water with respect to the number of different species obtained. For instance, at station 146 in the Southern Ocean, at a depth of 1375 fathoms the 200 specimens captured belonged to 59 genera and 78 species." That was with a 10-foot trawl dragged for at most two miles during at most two hours. Murray then goes on to say: "In depths less than 50 fathoms, on the other hand, I cannot find in all my experiments any record of such a variety of organisms in any single haul even when using much larger trawls and dragging over much greater distances." He quotes the statistics of the Scottish Fishery Board's trawlings in the North Sea, with a 25-foot trawl, to show that the average catch is 7·3 species of invertebrata and 8·3 species of fish, the greatest number of both together recorded in one haul being 29 species. Murray's own trawlings in the West of Scotland gave a much greater number of species, sometimes as many as 50, "still not such a great variety of animals as was procured in many instances by the *Challenger's* small trawl in great depths."

Now, in the first place, it is curious that Murray's own table on p. 1437, in which he shows that the "terrigenous" deposits lying along the shore-lines yield many more animals, both specimens and species, per haul, than do the "pelagic" deposits[1] at greater depths, such as red clays and globigerina oozes, seems directly opposed to the conclusion quoted above. In the second place, I am afraid that Dr. Murray has misunderstood the statistics of the Scottish Fishery Board when he quotes them as showing that only 7·3 or so species of invertebrates are brought up, on the average, in the trawl net. I happen to know from Mr. Thomas Scott, F.L.S., the naturalist who has compiled the statistics in question, and also from my own observations when on board the *Garland* on one of her ordinary trawling expeditions, that the invertebrata noted down on the station sheet are merely a few of the more conspicuous or in other ways noteworthy animals. No attempt is made—nor could possibly be made in the time—by the one naturalist who has to attend to tow-nets, water-bottle, the kinds, condition, food, &c., of the fish caught and other matters—to give anything like a complete or even approximate list of the species, still less the number of individuals, brought up in the trawl. I submit, therefore, that it is entirely misleading to compare those Scottish Fishery Board statistics, which were not meant for such a purpose, but only to give a rough idea of the fauna associated with the fish upon certain grounds, with the carefully elaborated results, worked out at leisure by many specialists in their laboratories, of a haul of the *Challenger's* trawl. Of Dr. Murray's own trawlings in the West of Scotland I cannot, of course, speak so positively; but I shall be surprised to learn that the results of each haul were as carefully preserved and as fully worked out by specialists as were the *Challenger* collections.

[1] One of the earliest of the *Challenger* oceanographic results, the classification of the submarine deposits into "terrigenous" and "pelagic," seems inadequate to represent fully the facts in regard to sea-bottoms, so I am proposing elsewhere ("Report of Irish Sea Committee") the following amended classification:—(1) Terrigenous (Murray), where the deposit is formed chiefly of mineral particles derived from the waste of the land; (2) Neritic, where the deposit is chiefly of organic origin, and is derived from the shells and other hard parts of the animals and plants living on the bottom; (3) Planktonic (Murray's "pelagic"), where the greater part of the deposit is formed of the remains of free-swimming animals and plants which lived in the sea over the deposit.

Lastly, on the next Liverpool Marine Biology Committee's dredging expedition in the Irish Sea after the appearance of Dr. Murray's volumes, I set myself to determine the species taken in a haul of the trawl for comparison with the *Challenger* numbers. The haul was taken on June 23, at 7 miles west from Peel, on the north bank, bottom sand and shells, depth 21 fathoms, with a trawl of only 4-foot beam, less than half the size of the *Challenger* one, and it was not down for more than twenty minutes. I noted down the species observed, and I filled two bottles with undetermined stuff which my assistant, Mr. Andrew Scott, and I examined the following day in the laboratory. Our list comes to at least 112 species, belonging to at least 103 genera.[1] I counted 120 duplicate specimens which, added to 112, gives 232 individuals, but there may well have been 100 more. This experience, then, is very different from Murray's, and gives far larger numbers in every respect—specimens, species, and genera—than even the *Challenger* deep-water haul quoted. I append my list of species,[2] and practised marine zoologists will, I think, see at a glance that it is nothing out of the way, that it is a fairly ordinary assemblage of not uncommon animals such as is frequently met with when dredging in the "coralline" zone. I am sure that I have taken better netfuls than this both in the Irish Sea and on the West of Scotland.

In order to get another case on different ground, not of my own choosing, on the first occasion after the publication of Dr. Murray's volumes, when I was out witnessing the trawling observations of the Lancashire Sea Fisheries steamer *John Fell*, I counted, with the help of my assistant, Mr. Andrew Scott, and the men on board, the results of the first haul of the shrimp trawl. It was taken at the mouth of the Mersey estuary, inside the Liverpool bar, on what the naturalist would consider very unfavourable ground, with a bottom of muddy sand, at a depth of 6 fathoms. The shrimp trawl ($1\frac{1}{2}$-inch mesh) was down for one hour, and it brought up over seventeen thousand specimens, referable to at least 39 species,[3] belonging to 34 genera. These numbers have been exceeded on many other hauls taken in the ordinary course of work by the Fisheries steamer in Liverpool Bay—for example, on this occasion the fish numbered 5943, and I have records of hauls on which the fish numbered over 20,000, and the total catch of individual animals must have been nearly 50,000. Can any of Dr. Murray's hauls on the deep mud beat these figures?

The conclusion, then, at which I arrive in regard to the distribution of animals in deep water and in water shallower than 50 fathoms, from my own experience and an examination of the *Challenger* results, is in some respects the reverse of Murray's. I consider that there are more species and more individuals in the shallower waters, that the deep mud as dredged has a poor fauna, that the "Coralline" zone has a much richer one, and that the "Laminarian" zone, where there is vegetable as well as animal food, has probably the richest of all.

In order to come to as correct a conclusion as possible on the matter, I have consulted several other naturalists in regard to the smaller groups of more or less free-swimming Crustacea, such as Copepoda and Ostracoda, which I thought might possibly be in considerable numbers over the mud. I have asked three well-known specialists on such Crustaceans—viz., Prof. G. S. Brady, F.R.S., Mr. Thomas Scott, F.L.S., and Mr. I. C. Thompson, F.L.S.—and they all agree in stating that, although interesting and peculiar, the Copepoda and Ostracoda from the deep mud are not abundant either in species or in individuals.

[1] It is interesting, in connection with Darwin's opinion that an animal's most formidable competitors in the struggle for existence are those of its own kind or closely allied forms, to notice the large proportion of genera to species in such hauls. I have noticed this in many lists, and it certainly suggests that closely related forms are comparatively rarely taken together.

[2] See *Appendix*, p. 501.

[3]

Solea vulgaris	*Mytilus edulis*	*Dactylopus rostratus*
Pleuronectes platessa	*Tellina tenuis*	*Cletodes limicola*
P. limanda	*Mactra stultorum*	*Caligus*, sp.
Gadus morrhua	*Fusus antiquus*	*Flustra foliacea*
G. æglefinus	*Carcinus mænas*	*Aphrodite aculeata*
G. merlangus	*Portunus*, sp.	*Pectinaria belgica*
Clupea spratta	*Eupagurus bernhardus*	*Nereis*, sp.
C. harengus	*Crangon vulgaris*	*Asterias rubens*
Trachinus vipera	*Sacculina*, sp.	*Hydractinia echinata*
Agonus cataphractus	Some Amphipoda	*Sertularia abietina*
Gobius minutus	*Longipedia coronata*	*Hydrallmania falcata*
Raia clavata	*Ectinosoma spinipes*	*Aurelia aurita*
R. maculata	*Sunaristes paguri*	*Cyanea*, sp.

In answer to the question which of the three regions (1) the littoral zone, (2) from low water to 20 fathoms, and (3) from 20 fathoms downwards, is richest in small free-swimming, but bottom-haunting, Crustacea, they all replied the middle region from low water to 20 fathoms, which is the Laminarian zone and the upper edge of the Coralline. Prof. Brady assures me that nearly every other kind of bottom and locality is better than mud for obtaining Ostracoda. Mr. T. Scott considers that Ostracoda are most common in shallow water, from 5 to 20 fathoms. He tells me that, as the result of his experience in Loch Fyne, where a great part of the loch is deep, the richest fauna is always where banks occur, rising up to about 20 fathoms, and having the bottom formed of sand, gravel and shells. The fauna on and over such banks, which are in the Coralline zone, is much richer than on the deeper mud around them. On an ordinary shelving shore on the west coast of Scotland Mr. Scott, who has had great experience in collecting, considers that the richest fauna is usually at about 20 fathoms. My own experience in dredging in Norway is the same. In the centre of the fjords in deep water on the mud there are rare forms, but very few of them, while in shallower water at the sides, above the mud, on gravel, shells, rock, and other bottoms, there is a very abundant fauna.

Probably no group of animals in the sea is of so much importance from the point of view of food as the Copepoda. They form a great part of the food of whales, and of herrings and many other useful fish, both in the adult and in the larval state, as well as of innumerable other animals, large and small. Consequently, I have inquired somewhat carefully into their distribution in the sea, with the assistance of Prof. Brady, Mr. Scott, and Mr. Thompson. These experienced collectors all agree that Copepoda are most abundant, both as to species and individuals, close round the shore, amongst seaweeds, or in shallow water in the Laminarian zone over a weedy bottom. Individuals are sometimes extremely abundant on the surface of the sea amongst the plankton, or in shore pools near high water, where, amongst *Enteromorpha*, they swarm in immense profusion; but, for a gathering rich in individuals, species, and genera, the experienced collector goes to the shallow waters of the Laminarian zone. In regard to the remaining, higher, groups of the Crustacea my friend, Mr. Alfred O. Walker, tells me that he considers them most abundant at depths of 0 to 20 fathoms.

I hope no one will think that these are detailed matters interesting only to the collector, and having no particular bearing upon the great problems of biology. The sea is admittedly the starting point of life on this earth, and the conclusions we come to as to the distribution of life in the different zones must [illegible] modify our views as to the origin of the faunas as to the peopling of the deep sea, the shallow waters, and the land. [illegible] supposes that life started in Pre-Cambrian times on the [illegible], and from there spread upwards into shallower waters, [illegible] to the surface, and, a good deal later, downwards to the [illegible] by means of the cold polar waters. The late Prof. Moseley considered the pelagic, or surface life of the ocean [illegible] the primitive life from which all the others have been derived. Prof. W. K. Brooks ("The Genus Salpa," 1893, p. 156, [illegible]) [illegible] that there was a primitive pelagic fauna, consisting of [illegible] microscopic plants and animals, and "that pelagic life was [illegible] for a long period during which the bottom was uninhabited."

I, on the other hand, for the reasons given fully above, consider that the Laminarian zone close to low-water mark is at [illegible] life, that it probably has been so in the [illegible], [illegible] to express a more definite opinion as to [illegible] Pre-Cambrian times, life in its simplest forms first [illegible] I [illegible] reason why any other zone should be con[illegible] better claim than what is now the Laminarian [illegible]. It is there, at present at any rate, in the [illegible] Laminarian zone, at the point of junction of [illegible], where there is a profusion of food, where the [illegible] stretches or worn away from the land [illegible], where the animals are able to receive the [illegible] light and heat, oxygen and food, without [illegible] to the air, rain, frost, sun, and other [illegible] the littoral zone, it is there that life [illegible] most active, competition [illegible] that the surrounding con[illegible] animal life; and, therefore, it [illegible] that, as the result of over[illegible] downward to the abysses, [illegible] to the shore. Finally, it is in this Laminarian zone, probably, that under the stress of competition between individuals and between allied species evolution of new forms by means of natural selection has been most active. Here, at any rate, we find, along with some of the most primitive of animals, some of the most remarkably modified forms, and some of the most curious cases of minute adaptation to environment. This brings us to the subject of

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which deals with the habits and variations of animals, their modifications, and the relations of these modifications to the surrounding conditions of existence.

It is remarkable that the great impetus given by Darwin's work to biological investigation has been chiefly directed to problems of structure and development, and not so much to bionomics until lately. Variations amongst animals in a state of nature is, however, at last beginning to receive the attention it deserves. Bateson has collected together, and classified in a most useful book of reference, the numerous scattered observations on variation made by many investigators, and has drawn from some of these cases a conclusion in regard to the discontinuity of variation which many field zoologists find it hard to accept.

Weldon and Karl Pearson have recently applied the methods of statistics and mathematics to the study of individual variation. This method of investigation, in Prof. Weldon's hands, may be expected to yield results of great interest in regard to the influence of variations in the young animal upon the chance of survival, and so upon the adult characteristics of the species. But while acknowledging the value of these methods, and admiring the skill and care with which they have been devised and applied, I must emphatically protest against the idea which has been suggested, that only by such mathematical and statistical methods of study can we successfully determine the influence of the environment on species, gauge the utility of specific characters, and throw further light upon the origin of species. For my part, I believe we shall gain a truer insight into those mysteries which still involve variations and species by a study of the characteristic features of individuals, varieties, and species in a living state in relation to their environment and habits. The mode of work of the old field naturalists, supplemented by the apparatus and methods of the modern laboratory, is, I believe, not only one of the most fascinating, but also one of the most profitable fields of investigation for the philosophical zoologist. Such studies must be made in that modern outcome of the growing needs of our science, the Zoological Station, where marine animals can be kept in captivity under natural conditions, so that their habits may be closely observed, and where we can follow out the old precept—first, observation and reflection; then experiment.

The biological stations of the present day represent, then, a happy union of the field work of the older naturalists with the laboratory work of the comparative anatomist, histologist, and embryologist. They are the culmination of the "Aquarium" studies of Kingsley and Gosse, and of the feeling in both scientific men and amateurs, which was expressed by Herbert Spencer when he said: "Whoever at the seaside has not had a microscope and an aquarium has yet to learn what the highest pleasures of the seaside are." Moreover, I feel that the biological station has come to the rescue, at a critical moment, of our laboratory worker who, without its healthy, refreshing influence, is often in these latter days in peril of losing his intellectual life in the weary maze of microtome methods and transcendental cytology. The old Greek myth of the Libyan giant, Antæus, who wrestled with Hercules and regained his strength each time he touched his mother earth, is true at least of the zoologist. I am sure he derives fresh vigour from every direct contact with living nature.

In our tanks and artificial pools we can reproduce the Littoral and the Laminarian zones; we can see the methods of feeding and breeding—the two most powerful factors in influencing an animal. We can study mimicry, and test theories of protective and warning colouration.

The explanations given by these theories of the varied forms and colours of animals were first applied by such leaders in our science as Bates, Wallace, and Darwin, chiefly to insects and birds, but have lately been extended, by the investigations of Giard, Garstang, Clubb, and others, to the case of marine animals. I may mention very briefly one or two examples. Amongst the Nudibranchiate Mollusca—familiar animals around

most parts of our British coasts—we meet with various forms which are edible, and, so far as we know, unprotected by any defensive or offensive apparatus. Such forms are usually shaped or coloured so as to resemble more or less their surroundings, and so become inconspicuous in their natural haunts. *Dendronotus arborescens*, one of the largest and most handsome of our British Nudibranchs, is such a case. The large, branched processes on its back, and its rich purple-brown and yellow markings, tone in so well with the masses of brown and yellow zoophytes and purplish-red seaweeds, amongst which we usually find *Dendronotus*, that it becomes very completely protected from observation; and, as I know from my own experience, the practised eye of the naturalist may fail to detect it lying before him in the tangled forests of a shore-pool.

Other Nudibranchs, however, belonging to the genus *Eolis* for example, are coloured in such a brilliant and seemingly crude manner, that they do not tone in with any natural surroundings, and so are always conspicuous. They are active in their habits, and seem rather to court observation than to shun it. When we remember that such species of *Eolis* are protected by the numerous stinging cells in the cnidophorous sacs placed on the tips of all the dorsal processes, and that they do not seem to be eaten by other animals, we have at once an explanation of their fearless habits and of their conspicuous appearance. The brilliant colours are in this case of a warning nature, for the purpose of rendering the animal provided with the stinging cells noticeable and recognisable. But it must be remembered that in a museum jar, or in a laboratory dish, or as an illustration in a book or on the wall, *Dendronotus* is quite as conspicuous and striking an animal as *Eolis*. In order to interpret correctly the effect of their forms and colours, we must see them alive and at home, and we must experiment upon their edibility or otherwise in the tanks of our biological stations.[1]

Let me give you one more example of a somewhat different kind. The soft, unprotected mollusc, *Lamellaria perspicua*, is not uncommonly found associated (as Giard first pointed out) with colonies of the compound Ascidian *Leptoclinum maculatum*, and in these cases the *Lamellaria* is found to be eating the *Leptoclinum*, and lies in a slight cavity which it has excavated in the Ascidian colony, so as to be about flush with the general surface. The integument of the mollusc is, both in general tint and also in surface markings, very like the Ascidian colony with its scattered ascidiozooids. This is clearly a good case of protective colouring. Presumably the *Lamellaria* escapes the observation of its enemies through being mistaken for a part of the *Leptoclinum* colony; and the *Leptoclinum*, being crowded like a sponge with minute sharp-pointed spicules, is, I suppose, avoided as inedible by carnivorous animals, which might devour such things as the soft unprotected mollusc. But the presence of the spicules evidently does not protect the *Leptoclinum* from *Lamellaria*, so that we have, if the above interpretation is correct, the curious result that the *Lamellaria* profits by a protective characteristic of the *Leptoclinum*, for which it has itself no respect, or, to put it another way, the *Leptoclinum* is protected against enemies to some extent for the benefit of the *Lamellaria*, which preys upon its vitals.

It is, to my mind, no sufficient objection to theories of protective and warning colouration that careful investigation may from time to time reveal cases where a disguise is penetrated, a protection frustrated, an offensive device supposed to confer inedibility apparently ignored. We must bear in mind that the enemies, as well as their prey, are exposed to competition, are subject to natural selection, are undergoing evolution; that the pursuers and the pursued, the eaters and the eaten, have been evolved together; and that it may be of great advantage to be protected from *some*, even if not from all enemies. Just as on land, some animals can browse upon thistles whose "nemo me impune lacessit" spines are supposed to confer immunity from attack, so it is quite in accord with our ideas of evolution by means of natural selection to suppose that some marine animals have evolved an indifference to the noxious sponge or to the bristling Ascidian, which are able, by their defensive characteristics, like the thistle, to repel the majority of invaders.

Although we can keep and study the Littoral and Laminarian animals at ease in our zoological stations, it may perhaps be questioned how far we can reproduce in our experimental and observational tanks the conditions of the "Coralline" and the "Deep-mud" zones. One might suppose that the pressure —which we have no means as yet for supplying[1]—and which at 30 fathoms amounts to nearly 100 lbs. on the square inch, and at 80 fathoms to about 240 lbs., or over 2 cwt. on the square inch, would be an essential factor in the life conditions of the inhabitants of such depths, and yet we have kept half a dozen specimens of *Calocaris macandreæ*, dredged from 70 to 80 fathoms, alive at the Port Erin Biological Station for several weeks; we have had both the red and the yellow forms of *Sarcodictyon catenata*, dredged from 30 to 40 fathoms, in a healthy condition with the polypes freely expanded for an indefinite period; and Mr. Arnold Watson has kept the Polynoid worm, *Panthalis oerstedi*, from the deep mud at over 50 fathoms, alive, healthy, and building its tube under observation, first for a week at the Port Erin Station, and then for many months at Sheffield in a comparatively small tank with no depth of water. Consequently it seems clear that, with ordinary care, almost any marine animals from such depths as are found within the British area may be kept under observation and submitted to experiment in healthy and fairly natural conditions. The Biological Station, with its tanks, is in fact an arrangement whereby we bring a portion of the sea with its rocks and bottom deposits and seaweeds, with its inhabitants and their associates, their food and their enemies, and place it for continuous study on our laboratory table. It enables us to carry on the bionomical investigations to which we look for information as to the methods and progress of evolution; in it lie centred our hopes of a comparative physiology of the invertebrates—a physiology not wholly medical —and finally to the Biological Station we confidently look for help in connection with our coast fisheries. This brings me to the last subject which I shall touch upon, a subject closely related both to Oceanography and Bionomics, and one which depends much for its future advance upon our Biological Stations—that is the subject of

Aquiculture,

or industrial Ichthyology, the scientific treatment of fishery investigations, a subject to which Prof. M'Intosh has first in this country directed the attention of zoologists, and in which he has been guiding us for the last decade by his admirable researches. What chemistry is to the aniline, the alkali, and some other manufactures, marine zoology is to our fishing industries.

Although zoology has never appealed to popular estimation as a directly useful science having industrial applications in the same way that Chemistry and Physics have done, and consequently has never had its claims as a subject of technical education sufficiently recognised; still, as we in this Section are well aware, our subject has many technical applications to the arts and industries. Biological principles dominate medicine and surgery. Bacteriology, brewing, and many allied subjects are based upon the study of microscopic organisms. Economic entomology is making its value felt in agriculture. Along all these and other lines there is a great future opening up before biology, a future of extended usefulness, of popular appreciation, and of value to the nation—and not the least important of these technical applications will, I am convinced, be that of zoology to our fishing industries. When we consider their enormous annual value about eight millions sterling at first hand to the fisherman, and a great deal more than that by the time the products reach the British public, when we remember the very large proportion of our population who make their living directly or indirectly (as boatbuilders, net-makers, &c.) from the fisheries, and the still larger proportion who depend for an important element in their food supply upon these industries; when we think of what we pay other countries France, Holland, Norway —for oysters, mussels, lobsters, &c., which we could rear in this country if our sea-shores and our sea-bottom were properly cultivated; and when we remember that fishery cultivation or aquiculture is applied zoology, we can readily realise the enormous value to the nation which this direct application of our science will one day have -perhaps I ought rather to say, we can *scarcely* realise the extent to which zoology may be made the guiding science of a great national industry. The flourishing shellfish industries of France, the oyster culture at Arcachon and Marennes, and the mussel culture by bouchots in the Bay of Aiguillon, show what can be done as the result of encouragement and wise assistance from Government, with constant

[1] See my experiments on Fishes with Nudibranchs, in *Trans. Biol. Soc.*, Liverpool, vol. iv. p. 151; and NATURE for June 26, 1890.

[1] Following up M. Regnard's experiments, some mechanical arrangement whereby water could be kept circulating and aerated under pressure in closed tanks might be devised, and ought to be tried at some zoological station. I learn from the Director at the Plymouth Station that some of the animals from deep water, such as Polyzoa, do not expand in their tanks.

industry on the part of the people, directed by scientific knowledge. In another direction the successful hatching of large numbers (hundreds of million) of cod and plaice by Captain Dannevig in Norway, and by the Scottish Fishery Board at Dunbar, opens up possibilities of immense practical value in the way of restocking our exhausted bays and fishing banks—depleted by the over-trawling of the last few decades.

The demand for the produce of our seas is very great, and would probably pay well for an increased supply. Our choicer fish and shellfish are becoming rarer, and the market prices are rising. The great majority of our oysters are imported from France, Holland, and America. Even in mussels we are far from being able to meet the demand. In Scotland alone the long line fishermen use nearly a hundred millions of mussels to bait their hooks every time the lines are set, and they have to import annually many tons of these mussels at a cost of from £3 to £3 10s. a ton. . . .

Whether the wholesale introduction of the French method of mussel culture, by means of bouchots, on to our shores would be a financial success is doubtful. Material and labour are dearer here, and beds, scars, or scalps seem, on the whole, better fitted to our local conditions; but as innumerable young mussels all round our coast perish miserably every year for want of suitable objects to attach to, there can be no reasonable doubt that the judicious erection of simple stakes or plain bouchots would serve a useful purpose, at any rate in the collection of seed, even if the further rearing be carried on by means of the bed system.

All such aquicultural processes require, however, in addition to the scientific knowledge, sufficient capital. They cannot be successfully carried out on a small scale. When the zoologist has once shown as a laboratory experiment, in the zoological station, that a particular thing can be done—that this fish can be hatched or that shellfish reared under certain conditions which promise to be an industrial success, then the matter should be carried out by the Government[1] or by capitalists on a sufficiently large scale to remove the risk of results being vitiated by temporary accident or local variation in the conditions. It is contrary, however, to our English traditions for Government to help in such a matter, and if our local Sea Fisheries Committees have not the necessary powers nor the available funds, there remains a splendid opportunity for opulent landowners to erect sea-fish hatcheries on the shores of their estates, and for the rich merchants of our great cities to establish aquiculture in their neighbouring estuaries, and by so doing, instruct the fishing population, resuscitate the declining industries, and cultivate the barren shores—in all reasonable probability to their own ultimate profit.

In addition to the farming of our shores there is a great deal to be done in promoting the fishing industries on the inshore and offshore grounds along our coast, and in connection with such work the first necessity is a thorough scientific exploration of our British seas by means of a completely fitted dredging and trawling expedition. Such exploration can only be done in little bits, spasmodically, by private enterprise. From the time of Edward Forbes it has been the delight of British marine zoologists to explore, by means of dredging from yachts or hired vessels during their holidays, whatever areas of the neighbouring seas were open to them. Some of the greatest names in the roll of our zoologists, and some of the most creditable work in British zoology, will always be associated with dredging expeditions. Forbes, Wyville Thomson, Carpenter, Gwyn Jeffreys, M'Intosh, and Norman—one can scarcely think of them without recalling—

> Hurrah for the dredge, with its iron edge,
> And its mystical triangle,
> And its hided net, with meshes set,
> Odd fishes to entangle![2]

Much good pioneer work in exploration has been done in the past by these and other naturalists, and much is now being done locally by committees or associations—by the Dublin Royal Society on the West of Ireland, by the Marine Biological Association at Plymouth, by the Fishery Board in Scotland, and by the Liverpool Marine Biology Committee in the Irish Sea; but few zoologists or zoological committees have the means, the opportunity, the time to devote, along with their professional duties, to the thorough systematic survey of our whole British sea-area which is really required. Those who have not had experience of it can scarcely realise how much time, energy, and money it requires to keep up a series of dredging expeditions, how many delays, disappointments, expensive accidents and real hardships there are, and how often the naturalist is tempted to leave unprofitable ground, which ought to be carefully worked over, for some more favoured spot where he knows he can count upon good spoil. And yet it is very necessary that the whole ground—good or bad though it may be from the zoological point of view—should be thoroughly surveyed, physically and biologically, in order that we may know the conditions of existence which environ our fishes, on their feeding grounds, their spawning grounds, their "nurseries," or wherever they may be.

The British Government has done a noble piece of work which will redound to its everlasting credit in providing for, and carrying out, the *Challenger* expedition. Now that that great enterprise is completed, and that the whole scientific world is united in appreciation of the results obtained, it would be a glorious consequence, and surely a very wise action in the interests of the national fisheries, for the Government to fit out an expedition, in charge of two or three zoologists and fisheries experts, to spend a couple of years in exploring more systematically than has yet been done, or can otherwise be done, our British coasts from the Laminarian zone down to the deep mud. No one could be better fitted to organise and direct such an expedition than Dr. John Murray.

Such a detailed survey of the bottom and the surface waters, of their conditions and their contents, at all times of the year for a couple of years, would give us the kind of information we require for the solution of some of the more difficult fishery problems—such as the extent and causes of the wanderings of our fishes, which "nurseries" are supplied by particular spawning grounds, the reason of the sudden disappearance of a fish such as the haddock from a locality, and in general the history of our food fishes throughout the year. It is creditable to our Government to have done the pioneer work in exploring the great ocean, but surely it would be at least equally creditable to them—and perhaps more directly and immediately profitable, if they look for some such return from scientific work—to explore our own seas and our own sea-fisheries.

There is still another subject connected with the fisheries which the the biologist can do much to elucidate—I mean the diseases of edible animals and the effect upon man of the various diseased conditions. It is well known that the consumption of mussels taken from stagnant or impure water is sometimes followed by severe symptoms of irritant poisoning which may result in rapid death. This "musselling" is due to the presence of an organic alkaloid or ptomaine, in the liver of the mollusc, formed doubtless by a micro-organism in the impure water. It is clearly of the greatest importance to determine accurately under what conditions the mussel can become infected by the micro-organism, in what stage it is injurious to man, and whether, as is supposed, steeping in pure water with or without the addition of carbonate of soda will render poisonous mussels fit for food.

During this last year there has been an outcry, almost amounting to a scare, and seriously affecting the market,[1] as to the supposed connection between oysters taken from contaminated water and typhoid fever. This, like the musselling, is clearly a case for scientific investigation, and, with my colleague, Prof. Boyce, I have commenced a series of experiments and observations, partly at the Port Erin Biological Station, where we have oysters laid down on different parts of the shore under very different conditions, as well as in dishes and tanks, and partly at University College, Liverpool.

Our object is to determine the effect of various conditions of water and bottom upon the life and health of the oyster, the effect of the addition of various impurities to the water, the conditions under which the oyster becomes infected with the typhoid bacillus, and the resulting effect upon the oyster, the period during which the oyster remains infectious, and lastly, whether any simple practicable measures can be taken (1) to determine whether an oyster is infected with typhoid, and (2) to render such an oyster innocuous to man. As Prof. Boyce and I propose to lay a paper upon this subject before the Section, I shall not occupy further time now by a statement of our methods and results.

I have probably already sufficiently indicated to you the extent and importance of the applications of our science to

[1] We [illegible] Fisheries Central Board or Government Department of Fisheries [illegible] experts, and that not merely for the [illegible] but still more, in order that [illegible] aquicultural experiments [illegible].

[2] The Dredging Song [illegible] Edward Forbes, p. 47).

[1] I am told that between December and March the oyster trade decreased 70 per cent.

practical questions connected with our fishing industries. But if the zoologist has great opportunities for usefulness, he ought always to bear in mind that he has also grave responsibilities in connection with fisheries investigations. Much depends upon the results of his work. Private enterprise, public opinion, local regulations, and even imperial legislation, may all be affected by his decisions. He ought not lightly to come to conclusions upon weighty matters. I am convinced that of all the varied lines of research in modern zoology, none contains problems more interesting and intricate than those of bionomics, oceanography, and the fisheries, and of these three series the problems connected with our fisheries are certainly not the least interesting, not the least intricate, and not the least important in their bearing upon the welfare of mankind.

APPENDIX.

List of Species taken in one haul, on June 23, 1895 (see p. 497).

SPONGES:

Reniera, sp.
Halichondria, sp.
Cliona celata
Suberites domuncula
Chalina oculata

COELENTERATA:

Dicoryne conferta
Halecium halecinum
Sertularia abietina
Coppinia arcta
Hydrallmania falcata
Campanularia verticillata
Lafoea dumosa
Antennularia ramosa
Alcyonium digitatum
Virgularia mirabilis
Sarcodictyon catenata
Sagartia, sp.
Adamsia palliata

ECHINODERMATA:

Cucumaria, sp.
Thyone fusus
Asterias rubens
Solaster papposus
Stichaster roseus
Porania pulvillus
Palmipes placenta
Ophiocoma nigra
Ophiothrix fragilis
Amphiura chiajii
Ophioglypha ciliata
O. albida
Echinus sphæra
Spatangus purpureus
Echinocardium cordatum
Brissopsis lyrifera
Echinocyamus pusillus

VERMES:

Nemertes neesii
Chætopterus, sp.
Spirorbis, sp.
Serpula, sp.
Sabella, sp.
Owenia filiformis
Aphrodite aculeata
Polynoe, sp.

CRUSTACEA:

Scalpellum vulgare
Balanus, sp.
Cyclopicera nigripes
Acontiophorus elongatus
Artotrogus magniceps
Dyspontius striatus
Zaus goodsiri
Laophonte thoracica
Stenhelia reflexa
Lichomolgus forficula
Anonyx, sp.
Galathea intermedia
Munida bamffica
Crangon spinosus
Stenorhynchus rostratus
Inachus dorsettensis
Hyas coarctatus
Xantho tuberculatus
Portunus pusillus
Eupagurus bernhardus
E. prideauxii
E. cuanensis
Eurynome aspera
Ebalia tuberosa

POLYZOA:

Pedicellina cernua
Tubulipora, sp.
Crisia cornuta
Cellepora pumicosa, and three or four undetermined species of Lepralids
Flustra securifrons
Scrupocellaria reptans
Cellularia fistulosa

MOLLUSCA:

Anomia ephippium
Ostrea edulis
Pecten maximus
P. opercularis
P. tigrinus
P. pusio
Mytilus modiolus
Nucula nucleus
Cardium echinatum
Lissocardium norvegicum
Cyprina islandica
Solen pellucidus
Venus gallina
Lyonsia norvegica
Scrobicularia prismatica
Astarte sulcata
Modiolaria marmorata
Saxicava rugosa
Chiton, sp.
Dentalium entale
Emarginula fissura
Velutina lævigata
Turritella terebra
Natica alderi
Fusus antiquus
Aporrhais pespelicani
Oscanius membranaceus
Doris, sp.
Eolis coronata
Tritonia plebeia

TUNICATA:

Ascidiella virginea
Styelopsis grossularia
Eugyra glutinans
Botryllus, sp.
B., sp.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY L. F. VERNON-HARCOURT, M.A., M.INST.C.E.

The Relation of Engineering to Science.

THE selection of a subject for an inaugural address, necessitated by the honour conferred upon me of presiding over this Section, has been rendered peculiarly difficult, both on account of the numerous able addresses delivered in past years by my eminent predecessors in this office, and also by the circumstance that the branches of engineering to which most of my professional life has been devoted have not as intimate a connection with mechanical science as some others. Moreover, whilst former Presidents of Section G have frequently dealt, in their addresses, with the progress of those special branches of engineering in which they have had most practical experience, such a course, in the present instance, would have exposed me to the danger of merely repeating information and reiterating opinions already recorded in the *Proceedings* of the Institution of Civil Engineers, and in other publications, with reference to maritime and hydraulic engineering. It has, accordingly, appeared to me that the exceptional occasion of addressing a gathering of scientific persons, and of engineers who testify their interest in science by attending these meetings, would be best utilised by considering the relation that engineering in general, and maritime and hydraulic engineering in particular, bear to pure science, and the means by which progress in engineering science might be best promoted, and its scope and utility increased.

In addition to the oft-quoted definition of civil engineering as "the art of directing the great sources of power in nature for the use and convenience of man," Thomas Tredgold also defined it, in 1828, as "that practical application of the most important principles of natural philosophy which has, in a considerable degree, realised the anticipations of Bacon and changed the aspect and state of affairs in the whole world." If the influence of engineering could be thus described in 1828, when railways and steamships were in their infancy and the electric telegraph and the various modern applications of electricity and magnetism had not come into existence, how far more true is it at the present day, when the various branches of engineering have attained such a marvellous development! Tredgold also realised, at that early date, that the resources of the engineer must be further directed so as to cope with the injurious forces of nature, such as floods, storms, and unsanitary conditions, and thus protect men from harm as well as promote their well-being. Moreover, he foresaw the great capabilities of development possessed by engineering, and its dependence on science: for he stated that "the real extent to which civil engineering may be applied is limited only by the progress of science; its scope and utility will be increased with every discovery in philosophy, and its resources with every invention in mechanical or chemical art, since its bounds are unlimited, and equally so must be the researches of its professors." If the full significance of these statements may be accepted as correct, engineers might fairly claim to have a right to say, "As engineers we are necessarily men of science, and no branch of science is outside our province." It might, however, be said that no engineer, with his absorbing professional avocations, would have the time to acquire even the rudiments of the principal branches of science, with their ever-increasing developments, to the study of each of which the life-work of many earnest searchers into the secrets of nature is wholly devoted. Nevertheless, a few branches of science, such as physiology, biology, and botany, appear to be beyond the scope of practical engineering; whilst a moderate acquaintance with some others might suffice for the needs of the engineer, except in certain special branches, supplemented, as it can readily be, by the advice of a specialist in complicated cases.

Among the branches of science necessary for the engineer, two may be regarded as of the highest importance, namely, mathematics and physics, upon which the science of engineering mainly depends; and without an adequate knowledge of these, no person should be able at the present day to enter the profession of a civil engineer. Other sciences of considerable, though of comparatively minor, importance to engineers in general, are chemistry, geology, and meteorology; but each of these assumes an enhanced value in special branches of engineering.

Mathematics in Relation to Engineering.—The pre-eminent importance of mathematics in relation to engineering may be regarded as fully established; and a President of the Institution of Civil Engineers would not now tell a pupil, at their first interview, that he had done very well without mathematics, a remark made to me by a justly celebrated engineer over thirty years ago.

Surveying, which is the handmaid of civil engineering, depends upon the principles of geometry for its accuracy; and ordinary triangulation, geodesy, and the rapid method of surveying and taking levels in rough country, known as tacheometry, are based on trigonometry and aided by logarithms. Tacheometry, indeed, though carried out by means of a specially constructed theodolite, may be regarded as the practical application of the familiar problem in trigonometry of finding the height and distance of an inaccessible tower. A proposition of Euclid forms the basis of the simplest and speediest method of setting out circular curves for railways; whilst astronomy has been resorted to for facilitating surveying in unexplored regions. The laws of statics are involved in the design of bridges, especially those of large span, and also of masonry dams, roofs, floors, columns, and other structures; whilst torsion, internal ballistics, the trajectory of a projectile, the forces of impact, and the stoppage of a railway train are dynamical problems. Hydrostatics and hydrodynamics provide the foundation of hydraulic engineering; though, owing to the complicated nature of the flow of water, observations and experiments have been necessary for obtaining correct formulæ of discharge. Geometrical optics has been employed for determining the forms of the lenses for giving a parallel direction to the rays proceeding from the lamps of a lighthouse, in accordance with the principles laid down by Fresnel. The theory of the tides, the tide tables giving the predicted tidal rise at the principal ports, and wave motion—questions of considerable importance to the harbour engineer—depend upon mathematical and astronomical calculations; whilst the stability and rolling of ships, the lines for a vessel of least resistance in passing through water, and the dimensions and form of screw propellers, to obtain the greatest speed with a given expenditure of power, have been determined by mathematical considerations aided by experiment. Electrical engineering depends very largely upon mathematical and physical problems, guided by the results of practical experience; and the possibility of the commercial success of the first Atlantic cable, depending upon the rate of transmission of the signals and the loss of electrical intensity in that long journey, has been shown by Dr. John Hopkinson in his "James Forrest" lecture, to have been determined by Lord Kelvin by the solution of a partial differential equation (*Proceedings Inst. C.E.* vol. cxviii. p. 330).

All branches of applied mathematics have, accordingly, been utilised by engineers, or, as in the case of several general principles and tidal calculations, by mathematicians to their benefit; but graphic statics will probably gradually supersede analytical methods for the calculation of stresses, as more rapid in operation, and less subject to errors, which are also more easily detected in graphic diagrams. Pure mathematics, in its higher branches, appears to have a less direct connection with engineering; but applied mathematics is so largely dependent upon pure mathematics, that the latter, including the calculus and differential equations, cannot be safely neglected by the engineer, though certain branches, as, for instance, probabilities, the theory of numbers, the tracing of curves, and some of the more abstruse portions of the subject, may be dispensed with.

Physics in Relation to Engineering.—Physics has been placed after mathematics, as many physical problems are determined by mathematics; but in several respects physics, with its very wide scope in its relation to the various properties of matter, is of equal importance to engineers, for there are few [illegible] engineering in which no part is borne by physical considerations.

The surveyor avails himself of physics when heights are [illegible] barometer, or by the temperature at which water [illegible]; and the spirit-level is a physical instrument adapted [illegible] for levelling across land. Evaporation, con[illegible] heat are of great importance in regard to [illegible]; and the expansive force of the [illegible], the diminution of friction, and the [illegible] are essential elements in the [illegible] engines. Allowance for expansion [illegible] has to be made in all large [illegible] to changes in temperature have to be taken into account. The temperature, also, which decreases with the elevation above the sea-level, and the distance from the equator, limits the height to which railways can be carried without danger of blocking by snow; whilst the temperature, by increasing about 1° F. with every 60 feet below the surface of the earth, limits the depth at which tunnels can be driven under high mountain ranges. Congelation of the soil is employed, as will be explained by M. Gobert, in excavations through water-bearing strata.

Compressed air is used by engineers for excluding the water from subaqueous foundations, so that excavations can be made and foundations laid, at considerable depths below the water-level, with the same certainty as on dry land. The compression of air, and its subsequent absorption of heat on being liberated and expanding in a chamber, are employed for refrigerating the chambers in which meat and other perishable supplies are preserved. Compressed air is employed for working the boring machinery in driving long tunnels through rock, and provides, at the same time, means of ventilation; and it also serves to convey parcels along pneumatic underground tubes. Moreover, the compressed-air and vacuum brakes are the most efficient systems of automatic and continuous brakes, which have done so much to promote safety in railway travelling, and in reducing the loss of time in the pulling up of frequently stopping trains. The production of a more perfect vacuum than can be produced by the ordinary air-pump, might have been supposed to be merely an interesting physical result (*Journal* of the Chemical Society, June 1864); but, in fact, the preservation of the heated filament of carbon in the incandescent electric light has been rendered possible only by the far more perfect vacuum obtained by the Sprengel vacuum pump, by which the air is exhausted down to so low a pressure as a two-hundred millionth of an atmosphere.

The illuminating power of different sources of light is of great importance in determining the distance at which the concentrated rays from a lighthouse can be rendered visible, as well as in relation to the lighting of streets and houses; and the refrangibility of the rays emitted, or the nature of their spectrum, should not be disregarded, as upon this depends the power of a light to penetrate mist and fog, which cut off the rays at the violet end of the spectrum, and have comparatively little influence on the least refrangible red rays (*Proceedings* Inst. C.E., vol. lvii. pp. 145–148). The effect also of the colouring of lights on their visibility is of interest in determining the shades of colour to be used for signals and ship-lights, and also the relative power of the lights required for different colours to secure equal illuminating power. Distinctions of colour are essential in these cases; but for distinguishing lighthouses, the use of coloured glasses has been abandoned, on account of their impairing the light emitted; and the desired indication has been effected by varying the number and duration of the flashes and eclipses in each lighthouse. The detection of colour-blindness is of interest to engineers, as this physical infirmity incapacitates men from acting as engine-drivers, signalmen, or navigating seamen. The use of compressed oil gas enables buoys and beacons to give a warning or guiding light for about three months without requiring attention; and the electric light has accelerated the passage through the Suez Canal from 30½ hours to 20 hours, and has greatly increased the capacity of the canal for traffic by enabling navigation to be carried on at night. The electric light also affords an excellent, safe, and cool light in the confined cabins on board ship, in the headings of long tunnels, and in the working-chambers filled with compressed air used for sinking subaqueous foundations.

Acoustics might seem to have little relation to engineering; but the soundness of the wheels of a train are tested by the noise they give when struck with a hammer; warning notes are emitted by railway and steamship whistles, the foghorn on board ship, and the whistling and bell-buoys employed for marking shoals or the navigable channel; whilst the striking of bells, the blast of steam sirens, and the explosion of compressed gun-cotton cartridges and rockets indicate the position of lighthouses in foggy weather. The most powerful sounds that can be produced by the help of steam appear to have a very limited range as compared with light; for, under ordinary conditions, the most powerful siren ceases to be audible at a distance of six or seven miles; whilst the transmission of sound is very much affected by the wind and the condition of the atmosphere. It seems possible that loud detonations at short intervals may be more readily heard than the continuous blast of a steam trumpet.

Electrical engineering is very intimately connected with physics, for it really is the application of electricity to industrial purposes. The very close relation between electricity and magnetism, discovered by Oersted in 1820, and further established by the remarkable researches of Faraday, has led to the present system of generating electricity by the relative movement of coiled conductors and electro-magnets, in dynamo-electric machines worked by a steam-engine or other motive power. The electrical current thus generated can be transmitted to a distance with little loss of energy; and it can either be used directly for lighting by arc or incandescent lamps, or be reconverted into mechanical power by the intervention of another dynamo. Electricity is also employed for the simultaneous firing of a series of mines, at a safe distance from the site of the explosion.

The convertibility of heat and energy, indicated by Mayer, forms the basis of thermodynamics; and the mechanical equivalent of heat, a physical problem of the highest interest, determined by Joule in 1843, furnishes a measure of the amount of work that can be possibly obtained by a given expenditure of heat in heat-engines.

The above summary indicates how the discoveries of physics are applied to many branches of engineering; and a knowledge of the laws of physics, and of the results of physical researches, appears, therefore, essential for the successful prosecution of engineering works. The very intimate relation of mechanical science to mathematics and physics, and the indebtedness of engineers to men of science outside the ranks of their own profession, are, indeed, evidenced by the roll of the Presidents of Section G, containing the names of Dr. Robinson, Mr. Babbage, Prof. Willis, Prof. Walker, and Lord Rosse.

Chemistry in Relation to Engineering.—Gas-making is in reality a chemical operation on a large scale, consisting in the destructive distillation of coal, the purification and collection of the resulting carburetted hydrogen, and the separation and utilisation of the residual products. Chemistry, accordingly, holds a very important place in the requirements of the gas engineer.

The manufacture of iron, steel, and other metals, and the formation of alloys, are essentially chemical operations; and the Bessemer and Gilchrist processes, by which steel is produced in large quantities directly from cast iron, by eliminating a portion of the carbon contained in it, and also the injurious impurities, silicon and phosphorus, in place of the former costly and circuitous method of removing the carbon from cast iron to form wrought iron, and then combining a smaller proportion of carbon with the wrought iron to form steel, are based on definite chemical changes, and necessitated chemical knowledge for their development.

Chemical analysis is needed for determining the purity of a supply of water, or the nature and extent of its contamination; and Dr. Clarke's process for softening hard water, by the addition of lime water, depends upon a chemical reaction. The methods, also, of purifying water by filtration, shaking up with scrap iron, and aëration, are chemical operations on an extensive scale; and their efficiency has to be ascertained by chemical tests.

Cements and mortars depend for their strength and tenacity, when mixed with water, upon their chemical composition and the chemical changes which occur. The value of Portland cement requires to be tested quite as much by a chemical analysis of its component parts as by the direct tensile strength of its briquettes; for an apparently strong cement may contain the elements of its own disruption, in a moderate proportion of magnesia or in an excess of lime. The chemical change which has been found to occur in the Portland cement of very porous concrete exposed to the percolation of sea-water under considerable pressure, by the substitution of the magnesia in sea-water for the lime in the cement, if proved to take place even slowly under ordinary circumstances, would render the duration of the numerous sea works constructed with Portland cement very precarious, and necessitate the abandonment of this very convenient material by the maritime engineer.

Explosives, which have rendered such important services to engineers in the construction of works through rock and the blasting of reefs under water, as well as for purposes of attack and defence, form an important branch of chemical research. The uses of gun-cotton as an explosive agent, though not for guns, have been greatly extended by the investigations of Sir Frederick Abel, and by the discovery that it can be detonated, when wet and unconfined, by fulminate of mercury; whilst smokeless powder, a more recent chemical discovery, seems likely, by its application to firearms, to produce important modifications in the conditions of warfare. The progress achieved by chemists in other forms of explosives has been marked by their successive introduction for blasting in large engineering works. Thus the removal of the rock in driving the Mont Cenis tunnel, in 1857–71, was effected by ordinary blasting powder; whilst the excavation of the longer St. Gothard tunnel, in 1872–82, was accomplished by the more efficient explosive dynamite (*Proceedings Inst. C.E.*, vol. xcv. p. 266). Moreover, the first great blast for removing the portion of Hallett's Reef which obstructed the approach to New York Harbour, was effected mainly by dynamite, together with vulcan powder and rendrock, in 1876; whereas the far larger Flood Rock, in mid-channel, was shattered in 1885 by rackarock, a mixture of potassium chlorate and nitrobenzol, and a much cheaper and a more efficient explosive under water than dynamite (*Ibid.*, vol. xcv. pp. 267–270). Rackarock is one of the series of safety explosives first investigated by Dr. Sprengel in 1870, which, consisting of a solid and a liquid, is safely and easily mixed for use; and these materials, being harmless previously to their admixture, can be stored in large quantities without risk (*Journal of the Chemical Society*, August 1873). The cost also of this large blast was greatly reduced by the sympathetic explosion of the bulk of the cartridges by the detonation of a series of primary exploders, placed at intervals along the galleries and fired simultaneously by electricity from the shore.

The utilisation of sewage belongs to agricultural chemistry; and the deodorisation of sewage, and its conversion into a commercial manure, are chemical processes. The disposal of sewage by irrigation is a branch of agriculture; and the innocuous character of the effluent fluid, discharged into the nearest stream or river, has to be ascertained by chemical analysis. Chemists have the opportunity of benefiting the community, and at the same time acquiring a fortune, by discovering an economical and efficient process for converting sewage on a large scale into a profitable saleable manure, so that inland towns may not have to dispose of their sewage at a loss, and that towns situated on tidal estuaries or the sea-coast may no longer discharge their sewage into the sea, but distribute it productively on the land.

The purifying of the atmosphere from smoke, rendered increasingly expedient by the growth of population, and the prevention of the dense fogs caused by it, by some practical method for more thoroughly consuming the solid particles of the fuel, still await the combined efforts of chemists and engineers.

Geology in Relation to Engineering.—A knowledge of the superficial strata of the earth is important for all underground works, and essential for the success of mining operations. Geology is indispensable in directing the search for coal, iron ore, and the various metals; and the existence of faults or other disturbances may greatly modify the conditions. The value of geology to the engineer is not, however, confined to the extraction of minerals, for it extends, more or less, to all works going below the surface.

The water-supply of a district, in the absence of a suitable river or stream, is dependent on the configuration and geology of the district; and the spread of London before the extension of waterworks, as pointed out by Prof. Prestwich, had to be confined to the limits of the gravel subsoil, in which shallow wells gave access to the water arrested by the stratum of underlying London clay. The sinking also of deep wells for a supply of water, and the depth to which they should be carried, are determined by the nature of the formation, the position of faults, and the situation of the outcrop of the water-bearing stratum. A geological examination, moreover, of a site proposed for a reservoir, to be formed by a reservoir dam across a valley, has to be made to ascertain the absence of fissures and the soundness of the foundation for the dam.

In the driving of long tunnels, the nature and hardness of the strata and their dip, the prospects of slips, and the possibility of the influx of large volumes of water, are geological considerations which affect the designs and the estimates of cost. The excavations also of large railway cuttings and ship canals are considerably affected, both as regards their side slopes and cost, by the nature and condition of the strata traversed.

Meteorology in Relation to Engineering.—The maximum pressure that may be exerted by the wind has to be allowed for in calculating the strains which roofs, bridges, and other structures are liable to have to bear in exposed situations; and con-

[illegible] records of anemometers for long periods are required for determining this pressure. The force of the wind also, and the direction, duration, and period of occurrence of severe gales, are important to the maritime engineer for estimating the effect of the waves in any special locality, for determining the quarter from which shelter is needed, and for ascertaining the seasons most suitable for the execution of harbour works, the repair of damages, and the carrying out of foundations of lighthouses and beacons on exposed rocks. The harbour engineer must, indeed, of necessity be somewhat of a meteorologist, for the changes in the wind and weather, the oscillations of the barometer, and the signs of an approaching storm are indications to him of approaching danger to his works, which he has to guard against; for the sea is an insidious enemy which soon discovers any weak spot, and may in a few hours destroy the work of months.

Continuous records of rainfall, as collected regularly by Mr. Symons from numerous stations in the United Kingdom, are extremely valuable to engineers for calculating the probable average yield of water from a given catchment area, the greatest and least discharges of a river or stream, the size of drainage channel needed to secure a low-lying area from floods, and the amount of water available for storage or irrigation in a hot, arid district. The loss of water by evaporation at different periods of the year, and under different conditions of soil and climate, the effect of percolation in reducing evaporation, and the influence of forests and vegetation in increasing the available rainfall, while equalising the flow of streams, are subjects of equal interest to hydraulic engineers and meteorologists.

Countries periodically visited by hurricanes, cyclones, or earthquakes, necessitate special precautions, and special designs for structures; and every additional information as to the force and extent of these visitations of nature is of value in enabling engineers to provide more effectually against their ravages.

Benefits conferred by Engineering upon Pure Science.—Engineering is generally concerned in the application of the researches of science for the benefit of mankind, and not in the extension of the domain of pure science, which necessitates greater concentration of attention and study than the engineer in practice is able to devote to it. Engineers, however, though never able to repay the ever-increasing debt of gratitude which they owe to past and present investigators of science, except in rendering these abstract researches of practical utility, have, nevertheless, been able incidentally to promote the progress of science. Thus mechanical science, by the construction of calculating machines, the planimeter, integrating machines, the tide-predictor and tidal harmonic analyser of Lord Kelvin, the self-registering tide-gauge, and various other instruments, has lightened the labours of mathematicians; whilst excavations for works, and borings have assisted the investigations of geologists. The mechanical genius of Lord Rosse led mainly to the success of the gigantic telescope, which has revealed so many secrets of the heavens; and the rapidity of locomotion, due to the labours of engineers, has greatly facilitated astronomical observations and physical discoveries, besides promoting the concourse of scientific men and the diffusion of knowledge. Electrical engineering, moreover, is so closely allied to electrical physics that the development of the one necessarily promotes the progress of the other. The observations also conducted by hydraulic and maritime engineers in the course of their practice aid in extending the [illegible] upon which the science of meteorology is based.

Engineering an Experimental Science.—Engineering, so far as it is based on mathematics, is an exact science, and the strains due to given loads on a structure can be accurately determined; but the strength of the materials employed has to be ascertained before any structure can be properly designed. Accordingly, the resistance of materials to tension, compression, and flexure, has to be tested, and their limit of elasticity and breaking weight determined. Thus, previously to the construction by Robert Stephenson, of the Britannia Tubular Bridge, the first wrought-iron girder bridge of large span erected, [illegible] experiments on various forms of wrought iron were [illegible] the eminent mathematician and mechanician [illegible] Hodgkinson, who had previously indicated the proper [illegible] for cast iron girders, and to whom the success of the [illegible] across the Menai Straits was in great measure due [illegible] Conway Tubular Bridges, Edwin Clark, [illegible] the [illegible] always now made of the materials [illegible] during the progress of any large [illegible] bridges are also subjected to severe [illegible] before being opened for public traffic, by which the safety of the structures and their rigidity, as measured by the amount of deflection, are ascertained, serving as a guide for subsequent designs.

Numberless experiments have been made on the flow of water in open channels, over weirs, through orifices, and along pipes; and the influences of the nature of the bed, the slope, depth, and size of channel, have been investigated by various hydraulicians. Mr. Thomas Stevenson measured the force of waves at some places on the Scotch coast ("The Design and Construction of Harbours," Thomas Stevenson, 3rd ed. pp. 52-56); Prof. Osborne Reynolds has examined the laws of tidal flow in a model of the inner estuary of the Mersey, and in specially shaped experimental models ("British Association Reports" for 1889, 1890, and 1891); and I have found it possible, in small working models of the Mersey and Seine, not merely to reproduce the configuration of the bed of the estuary out to sea, but also to observe the effects of different forms of training works in modifying sandy estuaries.[1] Mr. William Froude, after his retirement from active practice, devoted his abilities to experiments on the motion and resistance of ships in water, which have proved of inestimable value to the naval architect, and which formed the subject of his presidential address to this Section in 1875.

Electrical engineering is specially adapted for experimental investigation; and, in this branch, theory and practice are so closely allied that some of the most eminent exponents of the theory of the subject, such as Lord Kelvin and Dr. Hopkinson, have developed their theories into practical results. In most other branches, the investigator is generally distinct from the engineer in large practice; but it may be safely said that an able investigator and generaliser in engineering science, as, for instance, the late Prof. Rankine, accomplishes work of more value to the profession at large than the practical engineer, who, in the world's estimation, appears the more successful man.

Every branch of engineering science is more or less capable of being advanced by experimental investigations; and when it is borne in mind that the force of waves, the ebb and flow of tides in rivers, the influences of training works in estuaries, and the motion of ships at sea have been subjected to experimental research, it appears impossible to assign a limit to the range of experiments as a means of extending engineering knowledge. Problems of considerable interest, which can only be solved by experiments or by comprehensive generalisations from a number of examples, must frequently present themselves to engineers in the course of their practice, as they have to myself; and engineers would render a great service to the profession if they would follow up the lines of investigation thus suggested to them, in the true spirit of scientific inquiry.

Failures of Works due to Neglect of Scientific Considerations.—Before the amount and distribution of the stresses in structures were thoroughly understood, a disposition was naturally evinced to err on the side of excessive strength; and the materials in the various parts of the structure were not suitably proportioned to the load to be borne, resulting in a waste of materials and too great an expenditure on the works. Thus some of the early high masonry reservoir dams in Spain exhibit an excessive thickness towards the top, imposing an unnecessary load on the foundations; and in many of the earlier iron girder bridges more material was employed than was required for stability, and it was not properly distributed. Boldness engendered by increased experience, and dictated by motives of economy, has tended to make the engineers of the present day pursue an opposite course; and, under these circumstances, the correct calculation of the strains, the exact strength of the materials, and a strict appreciation of the physical laws affecting the designs become of the utmost importance.

The failures of many bridges may be explained by errors in design, defects in construction, or by economy carried beyond the limits of safety in pushing forward railways in undeveloped countries; but other failures are attributable to a disregard or underestimation of the influence of physical causes. Thus the Tay Bridge disaster, in 1879, was due to underestimating the amount and effect of the wind-pressure in an exposed situation, where it acted with a considerable leverage, owing to the height of the bridge, and was inadequately provided against by the

[1] *Proceedings* of the Royal Society, vol. xlv. pp. [illegible], and plates 2-4; vol. xlvii. p. [illegible]; and "Amélioration de la Partie Maritime des Fleuves, y compris leur Embouchure," L. F. Vernon-Harcourt, Paris Inland Navigation Congress, 1892, pp. 27-2[illegible], and [illegible], and plate [illegible].

small transverse width of the piers in proportion to their height, which were further weakened by bad workmanship in the bracing of their columns. The bursting of the Bouzey masonry dam in France this year must be attributed to an inadequate thickness at part of the cross-section, producing a tensional strain on the inner face with the reservoir full, aided by the instability resulting from a fissured foundation. The overthrow of the outer arms of the Madras breakwaters, during a cyclone in 1881, may be traced to an inadequate estimate of the force of the waves in a storm, in deep water, and with a great fetch across the Indian Ocean, beating against the portions of the breakwaters directly facing their course; for these outer portions, running nearly parallel to the coast-line, were not made any stronger than the inner portions placed at right angles to the shore and the direction of the waves, and situated for the most part in shallower water. The erosion of the bed of the Ganges Canal on the first admission of the water, necessitating the erection of weirs at intervals to check the current, resulted from an error in the calculated discharge of the channel with the given inclination, and the consequent undue velocity of the stream, producing scour. The failure of the jetty works at the outlet of the Rhone to effect any permanent deepening of the channel over the bar, was due to the unsuitable direction given to the outlet channel in view of the physical conditions of the site, and the concentration of all the discharge, and consequently all the alluvium carried down, into a single mouth, whereby the rate of deposit in front of this outlet has been considerably increased. The excessive cost, and consequent stoppage, of the Panama Canal works, though due to a variety of causes, must be partly attributed to want of due consideration of the strata to be excavated: for a cutting of 300 feet in depth, which may be possible in rock, becomes impracticable when a considerable portion has to be executed in very treacherous clay.

Occasionally failures of works may be attributed to exceptional causes or peculiarly unfavourable conditions; but in most cases, as in the instances given above, they are the result of errors or deficiencies in design, which might have been avoided by a more correct appreciation of the physical conditions involved.

Scientific Training of Engineers.—In most professions, preliminary training in those branches of knowledge calculated to fit a student for the exercise of his profession is considered indispensably necessary; and examinations to test the proficiency of candidates have to be passed as a necessary qualification for admission into the Army, Navy, Church, Civil Service, and both branches of the law. Special care is taken in securing an adequate preliminary training in the case of persons to whom the health of individuals is to be entrusted, not merely by experience in hospitals, but also by examinations in those branches of science and practice relating to medicine and surgery, before the medical student can become a qualified practitioner. If so much caution is exercised in protecting individuals from being attended by doctors possessing insufficient knowledge of the rudiments of their profession, how much more necessary should it be to ensure that engineers are similarly qualified, to whom the safety and well-being of the community, as well as large responsibilities in regard to expenditure, are liable to be entrusted! The duty of the engineer is to apply the resources of nature and science to the material benefit and progress of mankind; and it, therefore, seems irrational that no guarantee should be provided that persons, before becoming engineers, should acquire some knowledge of natural laws, and of the principles of those sciences which form the basis of engineering. The Institution of Civil Engineers has, indeed, of recent years required some evidence of young men having received a good education before their admission into the student class; but some of the examinations accepted as sufficient for studentship, such as a degree in any British university, afford no certainty in themselves that the persons who have passed them possess any of the qualifications requisite for an engineer; and it is quite unnecessary to become a student of the Institution in order to become an engineer. The Council of the Institution has no doubt been hitherto deterred from proposing the establishment of an examination in mathematics and natural science, as a necessary preliminary to becoming an engineer, by the remembrance that some of the most distinguished engineers of early days in this country were self-taught men; but since those days engineering and the sciences upon which it is based have made marvellous advances; and in view of these developments, and the excellent theoretical training given to foreign engineers, it is essential that British engineers, if they desire to retain their present position in the world, should arrange that the recruits to their profession may be amply qualified at their entrance in theoretical knowledge, in order to preserve the standard attained, and to be in a position to achieve further progress. No amount of preliminary training will, indeed, necessarily secure the success of an engineer, any more than the greatest proficiency would be certain to lead the medical student to renown as a physician or surgeon: but other conditions being equal, it will greatly promote his prospects of advancement in his profession, and his utility to his colleagues and the public. The engineers of the past achieved great results in the then early dawn of engineering knowledge, by sound common sense, a ready grasp of first principles and of the essential points of a question, capacity for acquiring knowledge, power of managing men and impressing them with confidence, and shrewdness in selecting competent assistants. These same qualities are still needed for success in the present day, coupled with an opportunity of exhibiting them; but far more knowledge of mathematics and other sciences is required now, owing to the enormous advances effected, if the progress of engineering science is to be maintained. Even though in some branches, engineers in large practice may not have the time, or retain the requisite facility, for solving intricate mathematical problems, they should be able readily to comprehend the full bearing of the principles presented, and to understand the nature of the solutions put before them, which nothing but the scientific faculty implanted by early training in mathematics and physics can adequately secure.

A qualifying examination for engineers would usefully stop persons at the outset from entering the profession, who failed to evince the possession of the requisite preliminary knowledge: it would indicate, by the subjects selected, the kind of training best calculated to fit a person to become a useful engineer; and it would protect the public, as far as practicable, from the injuries or waste of money that might result from the mistakes of ill-qualified engineers.

Specialising in Engineering.—Some branches of engineering have for a long time been kept distinct from others, such as the construction of steam-engines, locomotives, and marine engines, ship-building, heavy ordnance, hydraulic machinery, and other purely mechanical works, one or more of which have been treated as specialities by certain firms, and also gas lighting, and, more recently, electric lighting. In the department, however, of civil engineering in its narrower signification, as distinguished from mechanical engineering, engineers of former times were regarded as equally qualified to undertake any of the branches of public works; and the same engineer might be entrusted with the execution of roads, railways, canals, harbours, docks, sewerage works, and waterworks; while even steamships were not excluded from the category in Brunel's practice. The engineer of to-day, indeed, would be lacking that important factor for success, common sense, if he declined to execute any class of works which he might be asked to undertake; and a variety of works is very useful to the engineer in enlarging his views and experience, as well as in extending the range of his practice. The tendency, however, now in engineering, as in medicine, is for the engineer's practice to be confined to the special branch in which he had had most experience; a result which cannot fail to be beneficial to the public, and calculated to promote the progress of each branch. The powers of the human mind are too limited, and life is too short, for engineers to be able to acquire, in the present day, equal proficiency in the theory and practice of the several branches of engineering science, with their ever-widening scope and development; and, as in the domain of abstract science, general progress will be best achieved in engineering science by the concentration of the energies of engineers in the advancement of their special line of practice.

Value of Congresses on Special Branches of Engineering.—The scope of engineering science is extending so fast that it is impossible for the Institution of Civil Engineers, which, as the parent society, embraces every branch within its range of subjects, to give more than a very limited time for the consideration and discussion of papers relating to the non-mechanical branches of the profession comprised in public works. Mechanical, electrical, and gas engineers have special societies of their own for advancing their knowledge and publishing their views and experience, while sharing equally with the other branches in the benefits of the older Institution.

Congresses accordingly afford a valuable opportunity for railway, hydraulic, and sanitary engineers of expressing their views, and enlarging their experience by consultation and discussion with engineers of various countries. My experience of the six maritime, inland navigation, and water-works international congresses I have attended in England and abroad, has convinced me of the very great value of such meetings in collecting information, comparing views, and obtaining some knowledge of foreign works and methods; whilst the acquaintances formed with some of the most celebrated foreign engineers, afford opportunities of gaining further information about works abroad, and deriving experience from their progress and results.

Engineering Literature.—Lawyers have been defined as persons who do not possess a knowledge of law, but who know where to find the law which they may require. It may be hoped that a similar definition is not applicable to engineers; but with the rapid increase of engineering literature, it is most desirable that engineers should be able readily to refer to the information on any special subject, or descriptions of any executed works, which may have been published. Much valuable matter, however, is buried in the proceedings of engineering and scientific societies, and in various publications; and often a considerable amount of time is expended in fruitless search. This great waste of time and energy, and the loss of available information involved, led me a few years ago to suggest that a catalogue of engineering literature ought to be made, arranging the lists of publications relating to the several branches under separate headings. There is a possibility that this arduous and costly task may be partially accomplished in separate volumes; and, at any rate, the first step has been effected by the publication, under the auspices of the Paris Inland Navigation Congress of 1892, of a catalogue of the publications on inland navigation. A start has also been made in France, Italy, and England, towards the preparation of a similar catalogue on maritime works, which it may be hoped means one day will be found to publish on the meeting of some future congress. Engineers who have searched, even in the best libraries, for the published information on any special subject, will appreciate what a great boon an engineering subject catalogue would be to the profession, and indirectly to the public at large.

The occasional publication of comprehensive books on special branches of engineering, and concise papers on special subjects, by competent authorities, are extremely valuable in advancing and systematising engineering knowledge; but the time and trouble involved in the preparation of such publications must, like the organising of congresses, be regarded as a duty performed in the interests of the profession and science, and not as affording a prospect of any pecuniary benefit.

Concluding Remarks.—In this address I have endeavoured, though very imperfectly, to indicate how engineering consists in the application of natural laws and the researches of science for the benefit and advancement of mankind, and to point out that increased knowledge will be constantly needed to keep pace with, and to carry on, the progress that has been made. The great advantages provided by engineering works in facilitating communications and intercourse, and consequently the diffusion of knowledge, in increasing trade, in extending civilisation to [illegible], in multiplying the comforts of life, and affording [illegible] facilities of enjoyment and change of scene, may be [illegible] readily acknowledged; but the more gradual and [illegible], though not less important, benefits effected by [illegible] works are not so fully realised.

A comparison of engineering with the other chief branch of applied science, medicine, exhibits some similarities and [illegible]. In both professions, the discoveries of science are [illegible] for the benefit of mankind; but whilst physicians devote [illegible] to individuals, engineers are concerned in [illegible] being of the community at large. Persons [illegible] doctor when they are attacked by disease, or [illegible] accident; but they eagerly resort for enjoy[illegible] [illegible], mountain tramways, piers, great [illegible]; and they frequently avail themselves [illegible] locomotion to complete their [illegible] change of air and climate. Physicians [illegible] are ill: whereas engineers en[illegible] [illegible] and efficient drainage, to main[illegible] [illegible] respect, the evident results of [illegible] readily realised than the invisible, [illegible] benefits of engineering works. Statistics alone can reveal the silent operations of sanitary work; and probably no better evidence could be given of the inestimable value of good water and proper drainage on the health of the population of large towns, when aided by the progress of medical science, than the case of London, where, towards the close of the last century, the death-rate exceeded the birth-rate, and the numbers were only kept up by constant immigrations: whereas now, in spite of the vast increase of the population and the progressive absorption of the adjacent country into the ever-widening circle of houses, the number of births exceed the deaths by nearly nine hundred a week.

In engineering, as in pure science, it is impossible to stand still; and engineers require to be ever learning, ever seeking, to appreciate more fully the laws of nature and the revelations of science, ever endeavouring to perfect their methods by the light of fresh discoveries, and ever striving to make past experience and a wider knowledge stepping-stones to greater achievements. Engineers have a noble vocation, and should aim at attaining a lofty ideal; and, in the spirit of the celebrated scientific discoverers of the past, such as Galileo, Newton, Laplace, Cavendish, Lyell, and Faraday, should regard their profession, not so much as an opportunity of gaining a pecuniary reward, as a means of advancing knowledge, health, and prosperity.

The remarkable triumphs of engineering have been due to the patient and long-continued researches of successive generations of mathematicians, physicists, and other scientific investigators; and it is by the utilisation of these stores of knowledge and experience that engineers have acquired renown. A higher tribute of gratitude should perhaps be paid to the noble band of scientific investigators who, in pursuit of knowledge for its own sake, have rendered possible the achievements of engineering, than to those who have made use of their discoveries for the attainment of practical benefits; but they must both be regarded as co-workers in the promotion of the welfare of mankind. The advancement of science develops the intellectual faculties of nations, and enlarges their range; whilst the resulting progress in engineering increases their material comforts and prosperity. If men of science, by closer intercourse with engineers, could realise more fully the practical capabilities of their researches, and engineers, by a more complete scientific training, could gain a clearer insight into the scientific aspect of their profession, both might be able to co-operate more thoroughly in developing the resources of nature, and in furthering the intellectual and material progress of the human race.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SECOND SPRINGFIELD MEETING.

THE forty-fourth meeting of the American Association for the Advancement of Science was held at Springfield, Mass., August 29 to September 4, being the second meeting held at that city; the first was in 1859.

In the early history of the Association frequent meetings were held in New England, but fifteen years have passed since the last preceding New England meeting, held at Boston. The social and intellectual life of all New England cities ranks high, and the Association found a most appreciative and hospitable community.

A copy of the address of the retiring President, Dr. Daniel G. Brinton, on "The Aims of Anthropology," has already been sent to NATURE. It was a matter for regret that the author was unable to attend and read it personally.

The vice-presidential addresses were not quite so many as usual, owing to the resignation of Profs. Holden and Jordan as presidents of the Sections of Astronomy and Zoology, respectively, because of the change in place of meeting from California, where they reside, and where it was intended to meet if the trans-continental railroads had reduced fares sufficiently. The addresses delivered were by W. L. Stevens, on "Recent Progress in Optics"; William McMurtrie, on "The Relation of the Industries to the Advancement of Chemical Science"; William Kent, on "The Relation of Engineering to Optics"; J. Hotchkiss, on "The Geological Survey of Virginia, 1835–1841: its History and Influence in the Advancement of Geologic Science"; J. C. Arthur, on "The Development of Vegetable Physiology"; F. H. Cushing, on "The Arrow"; and B. E. Fernow, on "The Providential Function of Government in Relation to Natural Resources."

One of the first and most important matters of business presented was in reference to the proposed meeting of the British Association in Toronto in 1897. The writer offered a resolution cordially inviting the Association, in case they decide to accept the invitations already sent them from Toronto to hold the meeting there, to attend our meeting also as our guests, and requesting them to send early notice of the time of meeting to the Permanent Secretary of our Association, that ample time may be had to make suitable arrangements, and to renew the delightful memories of the Philadelphia meeting in 1884. This was referred to the Permanent Secretary with power.

Should the Association come to America as proposed, it seems probable that the long-deferred San Francisco meeting will then be held, as it is believed that many visitors will desire to cross the continent by the Canadian Pacific Railroad, which was incomplete at the time of the Montreal meeting in 1884; but many who attended that meeting went as far west as the road would then take them. As Sir Wm. C. Van Horne, President of that road, is a member of the British Association, and has been a member of ours, his influence is relied on to secure favourable rates of transportation. Still another factor is that the Christian Endeavour Societies expect to meet at San Francisco in 1897, and as they are a mighty army—70,000 attended the Boston meeting this summer—the railroads usually offer exceptional rates to secure their patronage, and the Associations can share in the benefit of the reduction.

Of the 207 papers read before the several Sections, many might be mentioned. The subject of colour and colour standards, on which Mr. Pillsbury had an article in a recent number of NATURE, was presented by him and others, and resolutions were passed looking toward the establishment of a colour standard. E. R. von Nardroff exhibited and described a new apparatus for studying colour phenomena. Colour photography was discussed and photographs exhibited by F. E. Ives.

A process for photographing the vocal cords in action has been discovered by F. S. Muckey and Wm. Hallock, and it is found that the pitch of a note is raised by rotating the arytenoid cartilages without increasing the tension of the cords, just as a violinist makes high notes by shortening the string with his finger. Voice analysis also has been studied by Messrs. Hallock and Muckey, by an ingenious system of resonators for the fundamental and seven overtones, covering three octaves from the fundamental C. These resonators are so arranged that the vibration of each causes the flickering of a tiny gas jet, and by observing these it can be seen which of the overtones are sounding, and by drawing straight or wavy lines to correspond with each of these, a picture of the tone can be made. This will enable a singer to see every tone in his voice, and learn wherein he needs to correct it.

The Weather Bureau of the United States supplied experts to fill up an afternoon in a joint meeting of four Sections. Willis L. Moore, the new chief of the bureau, spoke of the work in hand and that contemplated. An elaborate scheme of observation of upper strata of the air by kites and balloons and kite-balloons is to be carried out; and regular observations are to be made of "sensible temperature" by the wet bulb thermometer.

Frank N. Bigelow, in his paper on solar magnetic radiation and weather forecasts, made some very remarkable statements. The sun, he says, throws out curved lines of magnetic force. These are connected with sun-spots, and with storms on the earth. They have been studied by him so carefully that he fixes the time of the sun's axial revolution more accurately than ever before at 26·07928 days, with a probable error only in the last or possibly the two last figures. A surprising inference from his studies is that the earth has a crust 800 miles thick, and the sun has also a crust. Future investigation will supply data for a long forecast of seasonal weather conditions, years ahead. Cleveland Abbe followed with a paper on clouds and their nomenclature, and Alfred J. Henry with some very beautiful cloud photographs.

Electro-metallurgy has made rapid strides, and a paper on calcium carbide, by P. de Chalmot and J. T. Morehead, gave an account of the process used at their works in Spray, N.C., for cheap production of this compound by smelting together lime and coke in the electric furnace. This enables them to produce acetylene, the illuminating principle of gas, much cheaper than any other process.

A paper on the new process of making white-lead by electric action was read by R. P. Williams before the American Chemical Society, which met at Springfield two days earlier than the Association. Mr. Williams describes the process, which will work a revolution in this industry. Instead of acetate of lead, as in the old process, sodium nitrate is used together with sodium bicarbonate. A number of cells are filled with the solution, with plates of lead at one pole and of copper at the other. The current from a dynamo causes nitric acid to be liberated and to combine with the lead. A number of reactions occur, with the final production of white-lead in a very fine and uniform state and of superior colouring quality. The chemicals can be re-used indefinitely. As many as 500 pounds have already been made at one charge.

The Economic Section has always been one of great popular interest. The monetary question, monometallism or bimetallism, by J. W. Sylvester and Henry Farquhar; taxation in the United States, by Edward Atkinson; growth of great cities, by E. L. Corthell; manual training in horticulture, by W. R. Lazenby, were among the matters treated of. An effort was made to widen the scope of this Section by a change of name. Its name—Section of Economic Science and Statistics—was deemed peculiarly undesirable, and after much discussion of the respective merits of "sociology" and "social and economic science," the latter title was adopted as the name of Section I.

Buffalo was unanimously chosen as the next place of meeting, following the practice of the Association to meet at that city every tenth year, beginning with 1866, when 79 members there reorganised the Association after six years of suspended animation, during which no meeting had been held.

The time for meeting was much controverted. The Council recommended a change to Monday as the opening day, which met decided opposition, and on an informal vote 30 were opposed to it and only 27 favoured it; but opposition at length gave way, and the next meeting will begin on Monday, August 24, 1896, at Buffalo.

Officers elected were—President: Edward D. Cope, of Philadelphia. Vice-Presidents: A, Mathematics and Astronomy, William E. Story of Worcester; B, Physics, Carl Leo Mees of Terre Haute, Ind.; C, Chemistry, W. A. Noyes of Terre Haute, Ind.; D, Mechanical Science and Engineering, Frank O. Marvin of Lawrence, Kan.; E, Geology and Geography, B. K. Emerson of Amherst; F, Zoology, Theodore N. Gill of Washington; G, Botany, N. L. Britton of New York city; H, Anthropology, Alice C. Fletcher of Washington; I, Social Science, William R. Lazenby of Columbus, O. Permanent Secretary: F. W. Putnam of Cambridge. General Secretary: Charles R. Barnes of Madison, Wis. Secretary of the Council: Asaph Hall, Junr., of Ann Arbor, Mich. Secretaries of the Sections: A, Mathematics and Astronomy, Edwin B. Frost of Hanover, N.H.; B, Physics, Frank P. Whitman of Cleveland, O.; C, Chemistry, Frank P. Venable of Chapel Hill, N.C.; D, Mechanical Science and Engineering, John Galbraith of Toronto, Can.; E, Geology and Geography, A. C. Gill of Ithaca, N.Y.; F, Zoology, D. S. Kellicott of Columbus, O.; G, Botany, George F. Atkinson of Ithaca, N.Y.; H, Anthropology, John G. Bourke, United States Army; I, Social Science, R. T. Colburn of Elizabeth, N.J. Treasurer, R. S. Woodward of New York.

WM. H. HALE.

LETTERS TO THE EDITOR.

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August Meteors. Red Spot on Jupiter.

As supplementary to my paper on the August meteors (NATURE, No. 1347, August 22) and to Prof. A. S. Herschel's interesting letter on the same subject (No. 1349, September 5), I may note that a further comparison of the recent observations has revealed two additional instances of doubly observed meteors.

On August 11, 10h. 59m., Prof. Herschel at Slough recorded a meteor equal in brightness to a first magnitude star and moving swiftly along a path of 22½° from 264° + 52° to 252° + 31°, or from the head of Draco into Hercules. The meteor left a long, thin, white streak for 2 secs., and the duration of flight was estimated as 1 sec. Mr. H. Corder, at Bridgwater, observed the same object, noting the time as 10h. 58m., and the apparent path as 23° + 53½° to 14° + 50° between Cassiopeia and Andromeda.

The meteor was evidently a Perseid, and had a radiant at 30° + 57°. It was first seen when at a height of 95 miles above Oxford, and disappeared when 61 miles above Devizes. Its real length of path was 53 miles, and the earth point is indicated in the English Channel about 10 miles south of Lyme Regis, Dorsetshire.

On August 11, 11h. 43m., Prof. Herschel mapped a small bolide, rivalling Jupiter in brightness, and traversing with moderate speed a course of 15° from 229° + 50° to 225° + 44°, or from near ι Draconis to the head of Bootes. Duration of flight 1·5 sec.; the nucleus was evenly bright all the way, and it left a streak for 3 secs. Mr. Corder registered the same meteor, and gives the time as 11h. 42m., magnitude equal to Jupiter, and path as 60° + 62½° to 70° + 64° in Camelopardus. This object was also a Perseid, the radiant being at 32° + 52° near the cluster at χ Persei. The meteor at its first appearance was 75 miles high above a point 5 miles N. of Stratford-on-Avon, and at its disappearance 52 miles high over a place 5 miles W.N.W. of Great Malvern. Its real length of path was 34 miles, and earth point 6 miles S.E. of Aberdare.

Red Spot on Jupiter.—When twilight became too strong for comet-seeking on the morning of August 25 last, I turned my 10-inch reflector on Jupiter and saw the red spot, indefinitely, near its central transit. The planet had only just risen above the tops of some houses in this locality, and the telescopic image was by no means good, but I estimated the transit of the spot occurred at 4h. 24m. A.M. (August 24, 16h. 24m.), or about 9·4m. after Mr. Marth's zero meridian, System II., so that the longitude of the spot was 5°·7. The shouldering of the great south equatorial belt, east of the spot, was very conspicuous, and afforded an excellent guide to the position of the latter. A few minutes after the transit of the red spot I noticed a large white spot on the north side of the north equatorial belt, passing the central meridian. A power of 312 was used in these observations.

W. F. Denning.

Bristol, September 7.

Curious Optical Phenomenon.

The following description of an optical phenomenon, and its probable explanation, may be of interest. It will be observed that a similar experience occurring to one not accustomed to making optical experiments would very probably have caused him to believe that he had seen a ghost. It is therefore of importance psychologically.

The facts observed were as follows:—At about 1 A.M., August 26, I went to my bedroom; to get to it I had to pass through a small room which I used as a study. On entering it, though it was dark, and I had no lamp, the small room seemed brightly illuminated, about as bright as an 8 c.p. lamp would make it, apparently. To one side of a window in the room I saw a man standing, whom I recognised to be myself. The whole impression was very vivid and clear.

So far nothing was observed beyond what is described in the ordinary ghost story. I was much occupied with the consideration of a problem at which I had been working, and did not at first grasp the full signification of what I saw. On turning my head, the figure disappeared, but on looking towards the window, through which a very faint line came, the image reappeared. I then noticed that it was apparently standing in a position occupied, as I knew, by a large table. On more close examination, without, however, moving from the spot where I was standing, I saw that it had changed, and that it did not appear to have features; then it appeared to be flat against the wall, and I finally recognised it as an after-image of a shadow. On my first seeing it, however, it did not have this appearance at all, and I had evidently mentally supplied the features as one always does to the face of a friend who is seen at a distance which is really too great to admit of actual recognition.

I then got the impression of having seen the shadow before, and on considering the matter a few seconds, remembered that it was just before I had started for my room. I had been working in another room, endeavouring to solve a physical problem for four or five hours, and for about half an hour, or possibly more, had been steadily looking at a lamp (a habit of mine when thinking); I then got up, leaving the lamp lit, and went out on my way to my bed-room as mentioned above. On going out of the room my shadow was thrown by the lamp on the wall just to the right of the door. The passages were entirely dark, and it was not until I entered the room used as a study, that the faint light coming through the window and falling on the same spot of the retina that was previously occupied by the image of the dark doorway, stimulated the after-image.

I may say that my health was of the best, but that I had been smoking heavily for a few days previously, and the fact had begun to force itself upon me.

I would especially remark upon the apparent brightness of the apparition. I had never seen an after-image so bright. On going back to the room where the lamp was, I proved that the appearance of the shadow thrown as I went out of the room corresponded with that of the image seen, minus of course the features and colour, which had been supplied by the imagination.

In speaking of optical phenomena, I would say that an easy way of showing that the colours seen in the colour-top are due to lack of accommodation, is by taking a piece of red paper or cloth, and turning the top till the inner or outer line matches it exactly. Then, without moving or changing the speed of the top, place before the eye a convex glass. The colour on the top will disappear, but that of the cloth will of course remain. Similar experiments to those observed with the top can be observed by drawing dark lines on a piece of glass, and waving dark and white paper behind them.

R. A. F.

A Remarkable Flight of Birds.

The forms of birds flying at a great height and crossing the solar disc, as described by Mr. Bray in your issue of August 29, have been rather frequently seen here during the spring and autumn months, and the writer has always attributed such flights to migrating birds on passage. They have usually been noticed while observing the image of the sun projected on a card screen from the eyepiece of a small equatorial telescope; occasionally, however, they have attracted attention at night also, crossing the disc of the moon, upon which their forms are very clearly defined, and with careful focussing (which is very nearly the same as for parallel rays) it has almost been possible to identify the species from the shape of the wings and manner of flight; birds of the swallow tribe, in particular, have been clearly distinguished, and others resembling the thrush, possibly redwings or fieldfares, have been noticed. The direction of flight, according to the writer's experience, is nearly always towards the south in August and September, and the reverse in April.

On August 31, a continuous watch was kept on the moon from 8.50 to 9.35 P.M., using a power of 80 diameters on a reflector of 10 feet focus. Only eight birds were seen, however, four of them slowly crossing from north to south, the other two from west to east (nearly). They were evidently very distant. An estimate of the change of focus required for the apparently nearest bird gave ·15 inch. This would imply a distance of 7000 feet from the telescope, and the moon's altitude being about 14° the vertical height of this bird would be 7900 × sine 14° = 1000 feet (about). Some of the birds, judging from their apparent size, must have been two or three times more distant, and therefore higher in the same proportion.

It would be very interesting to obtain systematic observations of such flights of birds from various localities during the migrating seasons. Possessors of telescopes would find these observations a good exercise in that kind of patience or endurance which is so necessary in observing, for instance, a so-called meteor shower at its maximum!

The writer would be glad to receive notes on the subject from those of your readers who may care to watch for birds during the autumn. Estimates of the angle subtended by the spread wings would perhaps give the most reliable means of ascertaining the height of the birds, and their direction of flight can easily be obtained by reference to the diurnal motion of the sun or moon. It is hoped that by collecting data of this kind some new facts may be learned regarding the mysterious habits of our bird visitors.

J. Evershed.

Kenley, Surrey.

THE WOBURN EXPERIMENTAL FRUIT FARM.

On June 12 last a small party of those interested in agriculture and horticulture, including Mr. Herbert Gardner, Sir John Thorold, Prof. Armstrong, Prof. Warington, Dr. Voelcker, Mr. Charles Howard, Mr

Carruthers, Mr. George Murray, and others, visited Woburn to make the first formal inspection of an institution which, under the above somewhat unpretentious title, has been established by the joint action of the Duke of Bedford and Mr. Spencer Pickering, F.R.S., in order to supply what has hitherto been a great national want.

The object of this institution is to provide an experimental station where all matters connected with horticulture, and especially with the culture of hardy fruits, may be investigated both from the scientific and practical point of view.

The origin of such an enterprise is always a matter of some interest, and it becomes all the more so in after years, when, too often, the details of its conception and evolution are irretrievably lost. In the present instance we may trace the origin to an accident in a chemical laboratory. It was owing to such an accident some years ago that Mr. Pickering, whose work in physical chemistry is well known, was driven to seek health in a partial existence in the country. Not having the means, however, to procure this in the orthodox manner without abandoning his scientific work, he resorted to the somewhat unusual means of getting air and exercise by becoming an agricultural labourer at Rothamsted. From an agricultural labourer to a small farmer and landowner the steps were not so tedious as is generally the case, and for some few years past Mr. Pickering has turned his attention, after the manner of many landowners, to horticulture and practical fructiculture. To any one of a scientific turn of mind the unsatisfactory basis on which the culture of fruit depends cannot fail to be apparent. Its present condition is little better than that of horticulture some fifty years ago. It rests mainly on the hard-earned and often one-sided experience of practical men, gardeners, for the most part, or nurserymen.

But the pressure of business will rarely allow a nurseryman to indulge in anything approaching to systematic research, and even when he does obtain any important results, they are liable to be looked on askance, as being possibly tinctured by mercenary considerations. Moreover, even amongst the highest practical authorities there is hardly a single point in the cultivation of fruit on which unanimity of opinion prevails; indeed, on some of even the most elementary processes there seem to be as many opinions as there are so-called authorities.

The desirability of having some station where such matters might be patiently investigated, and from which results might issue free from any taint of commercial expediency, was evident to Mr. Pickering, and not having himself the capital or land necessary for such an undertaking, he applied for assistance to a former college friend, the Duke of Bedford. The Dukes of Bedford have during generations past identified themselves with the progress of agriculture and horticulture, the present holder of the title showing no tendency to be eclipsed by his predecessors in these matters. As was probable, such a scheme met with the hearty approval of the Duke, and the result was the establishment of the present institution, conducted jointly by himself and Mr. Pickering.

The fruit farm is on the Duke's land near Ridgmount Station, and almost adjoins the land which is given up to the use of the Royal Agricultural Society as an experimental agricultural station. About twenty acres have been devoted to the purpose, and of this some fifteen have already been planted.

Everything at present justifies the anticipation that this station will be conducted in the liberal and thorough-going manner which alone can produce results capable of commanding the confidence of horticulturists, and the energy with which the work has been commenced indicates that no time will be lost in obtaining trustworthy results. It is but twelve months since the field was bearing a crop of roots and weeds (especially the latter), yet in spite of the adverse season, the ground has been thoroughly cleaned, roads, hedges, and fences have been made, a house built on it, and over 500 experimental plots have been planted; also an extensive nursery has been planted, as well as collections of various ornamental and useful trees and shrubs. A fine crop of eighty different varieties of strawberries has been already gathered. With such work accomplished, it is scarcely necessary to say that an able manager is resident on the farm. The present manager, Mr. L. Castle, is a man whose experience and knowledge will command the confidence of practical horticulturists.

It is only possible here to indicate briefly the character of some of the experiments instituted. Besides strawberries—the investigation of which will embrace not only the respective merits of different varieties, but also the comparative values of the varieties at different ages, and the effects of certain manures on the crop—apples have been selected for the majority of the experiments already begun. Sixty different experiments are arranged to test different methods of planting, of root and branch treatment, and different manurial treatment, each experiment being made on eighteen trees, six of each of three varieties, all of the same age, and all raised on the same stock. These trees are all dwarf trees, and certain of the experiments are repeated with standard trees on the free-growing stock, and also with other dwarf trees of a fourth variety. Thirty-eight plots have been devoted to ascertaining the influence of different methods of training on the quantity and quality of the crop, and a collection of about 120 good varieties of apples has been made, each variety being grown on different stocks, and subjected in each case to different methods of treatment. This collection of apples is also so arranged that it may be utilised for the investigation of insecticides, without destroying the value of the results as regards the comparison of the different varieties. A smaller but interesting collection of apples of Scotch, Irish, and foreign origin has also been made. The numerous shelter hedges which have been planted are also of considerable interest, since, from an economical point of view, they also are experimental. They are composed of different varieties of nuts, plums, damsons, crabs, quince, medlars, and berberries.

Other experiments of greater scientific interest than the above are, we understand, either in progress or in contemplation; amongst these may be mentioned the influence of different stocks on the scion, and the great question of the effects of self- or cross-fertilisation. Such experiments, however, necessitate the lapse of a considerable amount of time before they can be said even to have been started, if they are to be started on a really satisfactory basis.

Those who are familiar with Mr. Pickering's chemical work will not fear that sufficient attention to minute details will be absent from the present undertaking. As instances of the thoroughness with which small questions are being examined, we may mention experiments on the relative merits of different arrangements of the same number of trees in a given area, and of the different direction in which the rows run as regards the points of the compass. Or, again, experiments on the influence of the nature, position, and inclination of the cut given in pruning a branch, and also the improvements which are being devised in methods of measuring the evaporating power of the air.

But it is very noteworthy that the strictly practical and economical aspects of horticulture will receive more attention than is usually the case at experimental stations. Six demonstration plots of a quarter of an acre each have been planted to illustrate how land may be most advantageously cropped by farmers, growers, and cottagers respectively. The initial cost of each of these plots is known, and an accurate account of the incoming and outgoing connected with each will be kept. In the nursery,

to which allusion has already been made, trees and bushes are being raised for distribution amongst the Duke's tenantry. We are pleased, however, to find that these practical steps for the promotion of fructiculture do not originate in any extravagant notions of the all-saving powers of fruit-growing to remedy the present agricultural distress. Much harm has been done in this country by the special pleading of those who are faddists on the subject, and who advocate their fad by holding up to view all the notable cases of success, and all the possible advantages to be gained, while they keep in the background all the difficulties and dangers, minimise the costs of planting, and hide the numerous cases of failure. No one can question the fact that fruit-growing in England is a profitable occupation when properly conducted under favourable conditions of soil, climate, and distance from market: nor can it be doubted that a certain proportion (perhaps 5 or 10 per cent.) of those who are now ordinary farmers could become fruit farmers with great advantage to themselves, and it must also be admitted that the distribution of some knowledge of fruit growing over the country generally would render the thousands of orchards attached to homesteads a source of small, or often substantial, profit to the holders, instead of being, as they are at present, a mere waste of land and money: but to imagine that every farmer can become a fruit grower is as absurd as imagining that every farmer could become a horse breeder. Even if such a metamorphosis were possible it would be suicidal; yet it should be pointed out that the fruit market in England is an exceptionally expansible one, and that prices of hard fruits would probably be but little affected even if the supply were doubled: the rapidly increasing importation of apples, which has now reached 3,000,000 bushels a year, has had no effect whatever on the market price of the fruit. These might have been grown in England just as well as abroad, for with a proper selection of varieties England need never fear a competition with foreign-grown apples.

It is certainly a fallacy to suppose that it is only in a few exceptionally favoured districts that fruit can be profitably grown: the appearance of the trees and the abundant crop of strawberries at the Woburn Experimental Fruit Farm are sufficient to demonstrate that a field of ordinary arable land of average fertility, with nothing to recommend it for fruit-growing beyond having a gentle slope to the south-west, and with a reputation amongst farmers of being the most unmanageable in the district, may be rendered highly suited for the production of fruit. To produce such results, however, right methods of procedure are, of course, essential, and nothing could be more striking than the difference between the bulk of the apple-trees at the farm, and those growing on two plots where the planting and subsequent treatment were such as is usually adopted by farmers: the ground where these trees were had, indeed, been properly trenched and cleaned once, but the trees had been carelessly planted, the branches had not been cut back, and the weeds had been subsequently allowed to grow: the result was that along the branches there were only a few half-dead leaves of not more than one-fifth of the proper size, and it would have required a trained horticulturist to have recognised that these trees were of the same variety as those which had been properly tended.

We were also much struck by the evidence which the results at the farm afforded of the hardiness of English fruit trees. No season could have been more trying to recently planted trees than that just experienced. A very wet autumn, during which the heavy soil of the farm was unworkable, was followed by a winter of almost unprecedented severity, and this, in its turn, by a still more trying period of drought. Yet, with the exception of the young stocks and a few strawberry plants, the mortality amongst the thousands of trees and bushes brought on to the ground in the autumn, was confined to about six individuals and half of these were killed through the improper method purposely adopted in planting them.

All readers of NATURE will wish success to an enterprise so well begun and so liberally conducted, which is clearly destined to afford results of high economic and scientific value.

THE REVISION OF THE "BRITISH PHARMACOPŒIA."

THE last edition of the "British Pharmacopœia" was issued in 1885, and though a thin volume of "Additions" was published by the General Medical Council in 1890, the progress of science and the requirements of medical practice have rendered necessary a complete revision of the official handbook. The work has accordingly been entrusted to a Committee of the Council, consisting of Sir Richard Quain, F.R.S., Chairman, the only remaining member of the Committee of 1885; Sir Dyce Duckworth and Mr. Carter, of London; Dr. Leech, of Manchester; Dr. Batty Tuke, of Edinburgh; Dr. Donald MacAlister, of Cambridge; Dr. McVail, of Glasgow; and Dr. Atthill and Dr. Moore, of Dublin. Dr. Nestor Tirard, of King's College, London, has been appointed secretary to the Committee, and Prof. Attfield, F.R.S., of the Pharmaceutical Society of Great Britain, general editor. On questions of chemistry, Dr. T. E. Thorpe, F.R.S., Principal of the Government Laboratory at Somerset House, with Prof. Emerson Reynolds, F.R.S., of Dublin, and Prof. Tilden, F.R.S., of the Royal College of Science, have been invited to act as scientific referees. Mr. W. T. Thiselton-Dyer, F.R.S., Director of the Royal Botanic Gardens, Kew, and Mr. Holmes, Curator of the Pharmaceutical Society's Museum, have received a similar invitation as regards botanical questions. The rapid growth of experimental pharmacology has, moreover, rendered it desirable to enlist expert assistance in regard to the physiological properties and actions of new remedies, and accordingly difficult questions of this nature will be referred to Dr. Lauder Brunton, of London, Prof. Fraser, of Edinburgh, and Prof. W. G. Smith, of Dublin. Lastly, on matters of pharmacy, the Pharmaceutical Society have been asked to give their valuable aid, and have promptly formed a strong committee of practical experts. To this committee many questions as to the compounding and preparation of drugs will doubtless have to be referred.

A circular inviting suggestions for the improvement of the "Pharmacopœia" has been addressed to the several universities and medical licensing corporations of the United Kingdom, and from the majority of these careful and elaborate replies have been received. They contain numerous proposals for the omission of doubtful or obsolete preparations, for the incorporation of new drugs that have come into practical use since 1885, and for the simplification and correction of the text in general.

In response to requests transmitted through the Privy Council to the medical authorities of the colonies and India, a very large body of materials, submitted with the object of adapting the "Pharmacopœia" to the requirements of the empire at large, have reached the editing committee. These open up a multitude of somewhat difficult questions; for though the "Pharmacopœia" is by law recognised as the official standard of reference at home, it has not the same legal sanction outside the British Isles. While therefore it is possible that something may be done as regards the recognition of important natural drugs used in Indian or colonial practice, it is highly probable that these may have to be relegated to a special appendix. The desire to go as far as may legally be practicable in making the "Pharmacopœia" an im-

perial one is, however, highly laudable, and should be encouraged with a view to the unification of British medical science. It is further announced that a long-deferred step is about to be taken by the introduction of the metric system into the body of the work. In the present edition the centimetres and grammes of science appear modestly in the supplementary pages dealing with volumetric processes, and then only as an alternative to grains and "grain-measures." We understand that in the new revision centimetres and grammes will be made official in all the monographs of the text, side by side with the still legalised grains and ounces, minims and drachms. This change will bring the British handbook into line with the official dispensatories of all other civilised States, and should tend to hasten the time when the international system of metric weights and measures shall acquire full legal authority in this country.

It thus appears that the Medical Council's Committee have undertaken the task of revision with an adequate sense of their responsibility. They have in the suggestions of the medical authorities at home and abroad, and in the useful digests of the literature of pharmacy, prepared from year to year by their reporter, Prof. Attfield, ample materials whereon to base their deliberations. As a body of physicians representing the supreme council of the profession, they are eminently qualified to judge as to the requirements of practical medicine and clinical therapeutics. Where their domain borders on that of the specialist in chemistry, botany, pharmacy, or physiological pharmacology, they propose to have recourse to the most skilled representatives of these branches of science. The result of their labours, thus conceived and carried out, will be awaited with interest not only by practitioners of medicine and pharmacy, and by manufacturing chemists, but by all who have sympathy with the application of science to human needs.

THE FIRST MERIDIAN.

At the recent Geographical Congress in London, the question of the first meridian was discussed with particular interest.

It was proposed that the first meridian should not be established officially, but should merely be settled with a view to producing an international map to the scale of millionths. M. A. de Lapparent has written an article in *La Nature* on the subject, of which the following is an analysis: it is a noteworthy occurrence that a Frenchman should have taken up the subject with such interest, for the French has hitherto been the only nation to reject the Greenwich meridian. In the preliminary discussions they have brought upon themselves many reproaches for hindering a scientific work the use of which every one had recognised, while they themselves had no principle to bring forward to support their objections. The matter has been much discussed amongst them, and at the Geographical Society of Paris, by a special commission, it was decided that the map should be accepted. It was considered best that France should not be the only country to refuse the project: nevertheless, it was decided to insist on the metric system being used, for here a principle was involved.

On this subject M. de Lapparent writes as follows:

"Thus, true to its habit of fighting for its views, France has again showed itself champion of the metric system, offering to make, for the scientific and rational interest, a sacrifice of national self-love. It would be impossible for it to capitulate on the question of the system, for here a principle is concerned; but the choice of a meridian, depending on no logical consideration, could be more easily granted. Evidently the proposed map, if ever produced was to be arranged so as to be a help to already existing aps, the latter being in great majority on the meridian of Greenwich; by wishing to impose the meridian of Paris which would not have been a success, it would have caused greater trouble than the contrary case. Henry IV. estimated that Paris was worth a mass; the French delegates, however, said on their side that the concession of a meridian, for a special and determined work, was quite worth the agreement which was expected to be established in view of the adoption, for the same purpose, of the metric system."

Many of our own countrymen have regretted that the public spirit prevented the system being used officially in Britain.

However, the acceptance of the Greenwich meridian well deserved a recompense, and the vote was unanimously carried that the metric system should be used for the map.

It is worth observing that the subject was discussed with remarkably few disagreements, considering that the congress was international. This seems to show that the time is fast approaching when national prejudices will be done away with if they support illogical theories; if principles are involved, it is right they should be adhered to, but they should not be allowed to hinder an enterprise profitable, perhaps, to all humanity.

NOTES.

The *Times* of yesterday published a telegram, dated September 17, from Sandefiord, Norway, received through Reuter's Agency, stating that advices received at Sandefiord from the Danish trading station of Angmagsalik, on the east coast of Greenland, state that towards the end of July a three-masted ship, with a short foremast, was seen by Eskimos on two occasions firmly embedded in drift ice. On the first occasion the ship was observed off Sermiligak, 65° 45′ lat. N., 36° 15′ long. W.; and the second time off Sermelik, 65° 20′ lat. N., 38° long. W. It is believed tha the vessel was Dr. Nansen's *Fram*, and that she was on her return journey. In any case, however, no positive news of the exploring vessel is expected to arrive until next year.

On Wednesday, Sept. 11, a Reuter telegram announced that the steam yacht *Windward*, which took out the Jackson-Harmsworth Polar Expedition, had arrived at Vardo, and on Thursday another telegram, through the same Company's agency, stated that the expedition, after leaving Archangel, passed the winter on Franz Joseph Land, from which place a start was made in the middle of July. The crew appear to have suffered severely from scurvy, and all the members of it are more or less weakened by the malady. Three of the men succumbed, and two others were removed to the hospital at Vardo.

The *Standard* states that the excavations that are being carried out by the Greek Archæological Society on the site of ancient Eleusis, a few miles from Athens, have just yielded some results of exceptional importance. In a very ancient and well-preserved tomb, there have been found, in addition to the skeleton of a woman, a number of articles, including earrings of fine gold, silver, and bronze, several finger rings, sixty-eight small vases of various shapes in terra-cotta, two tripods, three Egyptian scarabæi, and a small statuette of the goddess Isis in porcelain. These discoveries leave no doubt of the fact that the celebrated mysteries of Eleusis were of Egyptian origin, and were borrowed from the religious rites of the ancient Egyptians. These important relics have been deposited in the National Museum.

A Reuter's telegram of September 11, from Berne, reported the fall of a huge mass of ice from the Altels Glacier upon the hamlet of Spitalmatte, in the Upper Gemmi Pass, causing the death of at least ten persons, and the loss of, it is estimated, two hundred head of cattle. A stretch of land nearly two miles

in length has been overwhelmed, and the pass has been partially blocked.

THE death is recorded of Dr. L. Galassi, Professor of Medical Pathology in the University of Rome; Dr. Friedrich Miescher, sometime Professor of Physiological Chemistry, and Dr. von Sury, Professor of Forensic Medicine in the University of Basel.

DR. RUFFER is, we are sorry to learn, suffering from an attack of diphtheritic paralysis, and will not, in consequence, be able to deliver his intended course of lectures at the British Institute of Preventive Medicine, or, indeed, do any work for some time to come.

THE following lectures will be delivered at the Royal College of Physicians during the coming year: The Goulstonian Course by Dr. Patrick Manson; the Lumleian Lectures by Sir Dyce Duckworth; the Croonian Lectures by Dr. George Oliver; and the Bradshaw Lecture by Dr. Bradbury. The Croonian lecturer for 1897 is Dr. Greenfield.

THE Berlin Academy of Sciences will award the Steiner prizes, of the respective value of 4000 and 2000 marks, for papers in continuation of J. Steiner's work on curved surfaces. The essays must be submitted to the Academy before the end of 1899.

AMONG a number of plumassier's bird-skins, said to have been brought from the foot of the Charles Louis mountains in New Guinea, has been found the skin of a most remarkable new Bird of Paradise of the genus *Astrapia*, conspicuous for its crimson gorget and black-and-white tail. This specimen, which has been secured for the Tring Museum, has just been described by Mr. Walter Rothschild as *Astrapia splendidissima*.

A NEW part of the quarto *Transactions* of the Zoological Society, which will be issued on October 1, will contain an important memoir on the Dinornithidæ, by Prof. T. Jeffery Parker. The author enters at length upon the osteology, classification and phylogeny of these extinct birds, giving special attention to their cranial characters. Prof. Parker is inclined to associate the Moas with the Kiwis (Apterygidæ), rather than with any other existing family of the class of birds.

WITH the new number that has just been issued, the publication of that valuable American periodical *Insect Life* comes to an end. The cessation takes place, we are told, for administrative reasons. Happily, the good work which it accomplished will be continued in two series of bulletins from the Division of Entomology of the U.S. Department of Agriculture. A new series of general bulletins will be begun, and will contain short reports on special observations, and the miscellaneous practical and economic results of the work of the division, and in directions of general interest. This first series will be sent to all the present readers of *Insect Life* who desire them. The second series of bulletins, published at rarer intervals, will publish the results of the purely scientific work of the members of the office force, and will consist largely of longer or shorter monographic papers on groups of North American insects. This series will be distributed only to libraries and to working entomologists. The publication of the divisional series of circulars of information upon especially injurious insects, of farmers' bulletins upon special entomological topics (principally methods of treatment), and of occasional special reports will be continued.

THE Third Report of the Royal Commission appointed to inquire what light-houses and light-vessels it is desirable to connect with the telegraphic system of the United Kingdom by electrical communication, stated that the value of the warning conveyed to passing vessels by the display of storm signals, on the occasion of the approach of heavy gales, could scarcely be overestimated, and recommended that the light-houses on the most prominent points of the coast of the United Kingdom, with which electrical communication exists, should be made storm-warning stations. In compliance with this recommendation the Meteorological Council have now made arrangements for the supply of storm-warning telegrams to twenty-five prominent headlands on the coast, for the benefit of passing vessels, in addition to the telegrams at present forwarded to ports and harbours, which are intended more particularly for the use of vessels leaving the places at which the signals are hoisted. The signals used are canvas cones, with point upwards or downwards, to signify whether northerly or easterly, or southerly or westerly gales are expected, and are practically the same as those originally adopted in 1860 by Admiral FitzRoy, then chief of the Meteorological Department of the Board of Trade. The light-house authorities have readily assisted in carrying out the recommendation of the Royal Commission, by allowing their light-keepers to undertake the management of the signals.

WE have received a volume of meteorological observations made at Rousdon Observatory during the year 1894, under the superintendence of Mr. Cuthbert E. Peek. This observatory is situated a short distance within the eastern boundary of Devonshire, in close proximity to the cliff, at an elevation of 516 feet above mean sea level, and forms an important station of the Royal Meteorological Society. In addition to very complete meteorological observations, experiments of various kinds are carried on, in connection with evaporation, agriculture, &c. Mr. Peek remarks that, from an agricultural point of view, the year 1894 may be briefly summarised as a year of plenty, but with prices too low to pay for the cost of production. Since 1883, a daily comparison of the weather experienced at this observatory with that predicted for the district in the forecasts issued by the Meteorological Office has been made. The published daily weather reports were received the day following the date of issue, and the forecasts contained in them were therefore not seen until after the actual weather experienced had been recorded. The results have proved of much interest; for the year 1894, ninety-three per cent of the forecasts for wind and for weather, separately compared, were found to be trustworthy. A table of comparisons for the years 1884–94 shows that the percentage of successful forecasts has improved year by year.

THE preparation of artificial human milk has from time to time occupied the attention of investigators, but so far, according to Dr. Backhaus, no satisfactory substitute has been produced in the place of human milk. Dr. Backhaus has, however, quite recently endeavoured to supply this deficiency, and stimulated by Kehrer's method he has succeeded in producing so-called artificial human milk. The milk is carefully collected with the usual hygienic precautions of cleanliness, &c., and then submitted to fermentation by means of rennet, in the course of which a relatively rich milk serum is procured containing albumen and milk sugar. This serum is carefully sterilised, and by the addition of cream a material is produced which closely resembles human milk, which may be varied in composition according to the age or particular requirements of the individual. Since, however, our knowledge of the properties possessed by the natural fluids of the body has been recently extended in so remarkable a manner, the subject of artificial milks has become invested with new considerations, which a few years ago were not even suspected. In the course of his paper Dr. Backhaus points out that the sterilisation of milk should, if possible, be carried out on the large scale in dairies before distribution, that in this way better apparatus being to hand, more cleanly besides more effectual results will be obtained than when it is left in the hands of private individuals. As demonstrating the importance of freeing the milk from impurities before use, Dr. Backhaus mentions that

the city of Berlin alone consumes daily with its milk 300 cwt. of cow dung!

Although the extension of geological research into distant parts of the earth has shown that the divisions of time originally made in Europe are not always applicable to other areas, yet it is possible that the greatest geological division-lines that are recognised may represent world-wide periods of rapid change. Such is the view expressed by Prof. Le Conte in a paper on "Critical Periods in the History of the Earth," published by the University of California. He considers that in the evolution of the earth there must have been now and again, amid many smaller local changes, readjustments of the crust affecting the whole earth, with something approaching simultaneity. Such universal changes must be used to mark out the primary divisions of time: they are marked by widespread unconformities and the birth of great mountain-ranges, and as consequences of these changes in physical geology there follow remingling of faunas, the extinction of many types, the more rapid evolution of new forms, and the origin of new dominant classes. We thus have an alternation of short "critical" periods of extensive change and long periods of gradual change, the former marking the commencement of the great time-divisions of the earth's history. Four such critical periods can, in Prof. Le Conte's opinion, be recognised—the pre-Cambrian, the post-Palæozoic, the post-Cretaceous, and the Glacial. Comparing these with one another, he finds progressive change in their character; each one is shorter in duration than the previous one, and involves greater climatic changes and increased faunal effects from the introduction of new dominant types.

Dr. Gerhard Schott has published some interesting maps concerning the present conditions of sail navigation, which are appended to his paper on the subject appearing in the *Zeitschrift der Gesellschaft für Erdkunde*. They are chiefly compiled from log-books examined at the Deutsche Seewarte, Hamburg. The two main lines of voyages for German sailors are the "saltpetre trips" to the west coast of South America, and the "rice trips" to India and the Straits Settlements. A map divided into zones of equal travelling times from the Lizard shows the remarkable fact that the mouth of the Congo is one of the most difficult parts to reach in a sailing vessel. The Cape and Patagonia can be reached in the same time. The southern Indian Ocean forms a kind of racecourse along which the vessels speed to Australia in the same time as it would take to reach Zanzibar. Adelaide can be reached in ninety days, and so can Chile. New York, which requires forty days, is in that respect as distant as Panama, and is one of the most inaccessible ports for a sailing vessel, especially in the winter. The return is easier, and can be accomplished in twenty-five days, whereas the return from Panama takes sixty. The return from Australia is equally lengthy round the Cape as by Cape Horn, and the latter route is now preferred owing to the notoriously dangerous character of Cape Agulhas. Needless to say, the Suez Canal is quite useless for sailing vessels. Even apart from the fact that the Red Sea is most difficult to navigate, the canal dues exclude vessels whose vitality lies solely in the cheap freights they can offer in competition with steamers. With the modern construction of sailing vessels, which are built almost exclusively of iron and steel, the only enemies seriously feared are fogs, icebergs, and dead calms, to which we must add, in the much-frequented ocean highways of the northern Atlantic, the fast mail steamer. The average skipper does not mind a storm, but rather welcomes it, as it makes him go all the faster.

The *Journal of the Franklin Institute* states that the recent trials of electric locomotives at Nantasket Beach, near Boston, and at Baltimore, have so satisfactorily demonstrated the superiority of this class of motor over the steam locomotive for short hauls, that it is now very generally admitted that the near future will witness a very extensive application of the new form of motive power for short branch lines, tunnel haulage, &c. At the Nantasket Beach trials, it is stated that a speed exceeding sixty miles an hour was attained, and at Baltimore the test of the electric locomotive designed to draw trains through the tunnel, 7430 feet long, in that city, was highly successful. A maximum speed of fifty miles an hour is to be developed, and it is guaranteed that the locomotive will pull 1200 tons at a speed of thirty miles an hour. The system has been in practical and regular operation on the Nantasket Beach Railway since the end of June last.

According to the *Engineer*, a French physicist, M. Denayrouze claims to have discovered a means of increasing the illuminating power of gas about fifteen times. In his lamp M. Denayrouze employs a spherical-shaped metallic body, and a mantle capable of being raised to incandescence. In the body of the lamp is fixed a tiny motor, which works a ventilator, and which receives current from a couple of small accumulators. The electrical energy required is said to be only $\frac{1}{4}$ volt and $\frac{1}{15}$ of an ampere, and to be sufficient to force a current of air through the mantle and to cause the gas to burn with remarkable brilliancy. The burner is said to consume seven litres of gas per carcel, and lamps have been made having an illuminating power of 800-candle power.

Speaking of some experiments in marching, which have recently been carried out at the request of the German War Office, by some students of medicine of the Friedrich Wilhelm Institute in Berlin, who for the purpose wore the regulation uniforms and carried the full field service equipments, the *British Medical Journal* says:—"The marches performed varied from 22 to 33 miles, and were executed in all kinds of weather. The weights or loads carried varied from 48 to 68 lbs., the full service equipment of the German infantry soldier averaging 70 lbs. That of our own infantry does not usually exceed 60 lbs. The conclusions arrived at by the medical officers in charge of the experimental observations were practically as follows: When the load is not excessive and does not exceed 48 lbs. a march of twenty-five miles executed in cool weather (60 F.) is readily performed, and has no deleterious effects upon the man, even if continued for some days consecutively. With a mean temperature of 70° F. a similar load carried the same distance has a considerable temporary effect upon the organism, necessitating a rest of at least ten hours in the twenty-four. A load of 68 lbs. could not be carried twenty-five miles without inducing grave physiological disturbance, necessitating a full day's rest on the following day. This weight was not readily carried day by day without derangement of health over greater distance than fifteen miles. A weight of 60 lbs. was the maximum weight which could be carried on consecutive days for twenty-five miles by a man weighing 11 stone during ordinary summer weather consistently with health. It is not stated whether the men by whom these experiments were made were picked individuals, or what was their dietary."

The current number of *The Leisure Hour* contains an interesting article by E. Whymper, on some high mountain observatories, accompanied by illustrations and short accounts of the difficulties experienced and the results attained. The observatories described are:—Mount Washington, in New Hampshire, U.S.A., 6286 feet high; it was established in 1870, but is now closed. Pike's Peak, in Colorado, 14,134 feet high, was erected in 1873, and closed in 1888. This station was celebrated for its electrical storms. The most elevated station is on the top of the Misti, near Arequipa, in Peru. This is 19,200 feet above the sea, but notwithstanding its great elevation, the ascent is comparatively easy. About twelve miles to the north

... is a mountain called Chachani, about 20,000 feet high; an observatory was established just below the snow-line, at the height of 10,050 feet, in the years 1892–3, but is now abandoned. The article contains a graphic account of the difficulties of establishing two observatories on Mont Blanc, one at 14,320 feet, and the other on the summit, at 15,780 feet, by M. Vallot and M. Janssen, respectively. The meteorograph for the summit of Mont Blanc has been constructed by M. Richard at a cost of £750, and the clockwork is calculated to remain in action for eight months.

USEFUL and practical publications continue to issue from the various botanical experiment stations in the United States. We have on our table the following: From Kansas State Agricultural College, *Bulletin* No. 50, comprising a list of Kansas weeds, with descriptions, and figures of the seedling forms; from Cornell University, an essay, by Mr. G. F. Atkinson, on "Damping Off," containing a description, with figures, of the various parasitic fungi which accompany this phenomenon, including a new species, *Volutella leucotricha*; and "Studies in Artificial Cultures of Entomogenous Fungi," by Mr. R. H. Pettit, also illustrated by plates.

THE Report of the Botanical Exchange Club of the British Isles for the current year is issued, with a list of Desiderata. The main portion of the very useful work done by this Association rests with two or three individuals. This work would be greatly promoted by the addition of a few new subscribers, who should address themselves to Mr. Charles Bailey, College Road, Whalley Range, Manchester.

THE following colonial botanical publications have reached us:—The *Bulletin* of miscellaneous information of the Royal Botanic Gardens, Trinidad, for July, containing a number of notes on native and cultivated plants in the colony, by Mr. J. H. Hart; *Botany Bulletin*, No. 10, of the Department of Agriculture, Brisbane, consisting of contributions to the Queensland flora, by Mr. F. M. Bailey; *Proceedings* of the Royal Society of Queensland, vol. xi. pt. 1, with the annual address of the President, Mr. R. L. Jack, on "The Higher Utilitarianism."

MESSRS. G. PHILIP AND SON have reprinted for Dr. Mill the paper on "The English Lakes," which, under the title of "On the Bathymetrical Survey of the English Lakes," the author contributed to the July and August numbers of the *Geographical Journal*. The book is nicely got up, and is illustrated by numerous photographic views, maps, and diagrams.

A NEW edition—the third—of Clowes and Coleman's "Quantitative Chemical Analysis" has been sent to us by Messrs. J. and A. Churchill. The work has undergone certain changes since the publication of the second edition, the matter having been increased, the text revised, and some new figures added.

THE September part of *Science Progress* contains the following articles:—"Progress in the Study of the Ancient Sediments," by J. E. Marr; "On the Respiratory Function of Stomata," by F. Frost Blackman; "The Zoological Position of the Trilobites," by H. M. Bernard; "Some Metasomatic Changes in Limestones," by A. Harker; and "The Decomposition Products of Proteids," by Dr. T. Gregor Brodie.

THE series of small books, entitled "Encyclopédie Scientifique des Aide-Mémoire," which is being brought out conjointly by Messrs. Gauthier-Villars and G. Masson, of Paris, has had another volume added to it by the publication of "Cubature des Terrasses et Mouvement des Terres," by G. Dariès.

THE paper "On the Cost of Warships," which was read by Dr. Elgar at this year's summer meeting of the Institution of Naval Architects, has been issued in pamphlet form by the Institution. The pamphlet also contains a report of the discussion on the paper which took place at the meeting.

WE have received the *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, fourth series, vol. ix., No. 3, 4, and 5, and the *Journal of the Asiatic Society of Bengal*, vol. lxiv., part 2, No. 2.

MR. R. W. PAUL, of Hatton Garden, has sent to us advance sheets of his new catalogue of electrical testing and measuring instruments. Many of the instruments are figured.

THE University Correspondence College has issued its Intermediate Arts Guide, No. x., with the papers set at London University, July 1895, and articles on the special subjects for 1896, and its London Inter. Science and Prel. Sci. Guide No. vii., with the papers set at London University, July 1895.

THE August numbers of the *Journal of the Royal Microscopical Society* and of *Clinical Sketches* have reached us; also part vi. of the *Katalog der Bibliothek der Kaiserlichen Leopoldino-Carolinischen Deutschen Akademie der Naturforscher*, Halle; and Messrs. Friedlander and Sohn, Berlin, have sent us No. x. to xiv. of *Naturae Novitates*.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♂) from India, presented by Miss E. S. Cooper; a Smith's Dwarf Lemur (*Microcebus smithi*) from Madagascar, presented by Miss Ruby Woolcott; a Yellow-fronted Amazon (*Chrysotis ochrocephala*) from Guiana, presented by Mr. W. Page; a Beautiful Grass Finch (*Poephila mirabilis*, ♂) from Australia, presented by Mr. Gerard O'Shea; a Brazilian Tortoise (*Testudo tabulata*) from Brazil, deposited; three Boas (*Boa constrictor*) from Brazil, purchased; a Wapiti Deer (*Cervus canadensis*, ♂), two Triangular-spotted Pigeons (*Columba guinea*), a Spotted Pigeon (*Columba maculosa*), two Crested Pigeons (*Ocyphaps lophotes*), two Half-collared Doves (*Turtur semitorquatus*), two Vinaceous Doves (*Turtur vinaceus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SPECTRUM OF MARS.—In connection with the recent discussion as to the presence or absence of the bands of water vapour in the spectrum of Mars, Dr. Janssen has published further particulars of the observations made by him in 1867 (*Comptes rendus*, July 29). He points out that even with the quantity of vapour in our own atmosphere, the bands would be all but invisible to an observer on Mars if the solar light were reflected normally from the earth's surface, and since the general conditions of the planet point to its atmosphere being less important than our own, it is easy to understand that the detection of the bands is a very delicate observation. To reduce the absorptive effect of the terrestrial atmosphere, observations should be made at a high altitude, and the use of the lunar spectrum as a term of comparison is also important. As to the apparatus required, Dr. Janssen does not consider large telescopes indispensable, as even with them the telluric bands can only be observed in their totality. Previous to observing the spectrum of Mars, Dr. Janssen had been engaged in an extensive study of the spectrum of water vapour as exhibited by a tube 37 metres in length. The observations of Mars were made on May 12–15, 1867, from a station on Mount Etna at an altitude of nearly 3000 metres; at meridian passage the altitude of the planet was 72°, and at sunset, when the observations were commenced, it was still more than 60° above the horizon, while the moon was a little lower. The cold was excessive during the nights of observation, and the quantity of vapour contained in the atmosphere overlying the place of observation would not be able to give indications of the telluric groups near C and D, according to the experiments with the long tube. Under these highly favourable conditions, Dr. Janssen found feeble but certain indications of the groups at C and D, and he is confident that future researches will justify the conclusion at which he arrived.

APPARATUS TO ILLUSTRATE DOPPLER'S PRINCIPLE.—The movement of the lines in a spectrum due to the approach or recession of the source of light is now so thoroughly well known, and has become of such importance in astronomical questions, that a laboratory experiment to illustrate this fact will be of interest. The idea, which we owe to the Russian astronomer, A. Belopolsky, and which was published in the *Memorie della Società Degli Spettroscopisti Italiani*, is as follows:—We know that the wave length of light ray can be varied by reflecting the light into a movable reflector, the amount of variation depending on the velocity of the reflector and the angles of incidence and reflection. By allowing the light to fall as vertical as possible on to the reflector, the variation of the wave-length can be magnified at will by increasing the number of reflectors. Now the apparatus suggested consists of two cylinders with parallel axes capable of being rotated very rapidly in opposite directions. On the surfaces of each a large number of reflectors are fixed, which are so arranged that when a ray of light from a heliostat falls on the reflector of the first cylinder, then from this on to a reflector on the second cylinder, and so on backwards and forwards, and finally into the slit of a spectroscope.

By closing first half the slit and photographing the spectrum, and then, on the same plate, photographing again the spectrum, only this time using the other half of the slit, the movement of the lines will thereby be doubly recorded on the plate, the double displacement being due to the two directions of rotation of the cylinders during the first and second exposure respectively.

Whether this idea can be carried out practically is yet to be seen, for there are many difficulties connected with it, such as the great velocities of the cylinders, perfect rigidity, &c., which will be hard to overcome.

THE PRÆSEPE CLUSTER.[1]

THIS work belongs to a class of investigations whose number has been steadily increasing in the last few years. The discussion of the relative motion of stars in loosely aggregated groups is a study that may throw light on intricate questions connected with the structure of the cosmos; and in this point of view, the Pleiades group has been discussed by several astronomers since Bessel laid the foundation for such inquiries more than fifty years since. The cluster in Perseus, the stars about the nebula of Orion and some other groups have already engaged the attention of astronomers, but nothing more complete or more interesting has appeared than the present investigation due to Dr. Schur; and it will hold its own till lapse of time gives a more trustworthy hold upon the small mutual displacements which successive investigations may reveal, for greater accuracy of measurement can scarcely be expected.

The present work divides itself naturally into three sections. In the first is given the results of a thorough examination of the instrument and of the constants of reduction, together with the triangulation of the group undertaken by Dr. Schur. In the second part is presented the measurements of position angle and distance of the stars by Dr. Winnecke, made with the Bonn heliometer in 1857 and 1858; and in the third, the comparison of the results of the measurements made with the Bonn and Göttingen heliometers respectively.

The investigation of the errors that accompany heliometrical measurement and their elimination, however complete and satisfactory, will only be of interest to experts in the use of this delicate instrument; but as evidence of the accuracy finally attained, we may quote the resulting values of the scale, derived from the measurement of the distances between stars in different parts of the heavens, whose places were determined with great accuracy for the reduction of the heliometer observations made in the Transit of Venus expedition. The places of the "Victoria" stars have been taken from Dr. Gill's paper:—

	Dr. Schur's value.	Dr. Ambronn's value.
	$''$	$''$
Stars in Cygnus	40·01601	40·01915
,, Hydra	40·01506	40·01610
,, near Pole	40·01562	40·01678
"Victoria" stars	40·01750	40·01710

In a measurement of approximately 2°, the two observers would assign values different by only 0″·22, a degree of accuracy upon which they may be congratulated.

[1] "Astronomische Mittheilungen von der Königlichen Sternwarte zu Göttingen." Die Oerter der helleren Sterne der Praesepe. Von Dr. Wilhelm Schur. (Göttingen, 1895.)

Notwithstanding this apparent accuracy, there still remains an unexplained discrepancy between measures made with the heliometer and the distances deduced from meridian observations. Dr. Gill has called attention to this peculiarity, and has suggested an explanation which does not seem to be satisfactory to Dr. Schur, or to apply to the Göttingen instrument, where a distance of about 1000″ appears to be measured too small by approximately a quarter of a second. This difference disappears for distances of about 5000″, and reappears with an opposite sign for the greatest distances possible to measure with the Göttingen heliometer. Dr. Schur employs, and justifies the employment of an empirical correction of the form—

$$\text{Correction} = a_s + bs^2 + cs^3$$

where the unit of s is 1000 seconds. On the assumption that the correction disappears for $s = 5$, and is at a maximum for $s = 1·3$, he derives the following values for the coefficients:—

$$\text{Correction} = 0''·473\,(s - 0·50s^2 + 0·06s^3).$$

The investigation of the corrections to the readings of the position circle is made with quite as much care as that devoted to measures of distance, but the probable error of a distance measure is only half as great as that of a measure of angle. This result, confirmed as it is by similar discussions in the case of other heliometers, induces Dr. Schur to base his triangulation of the group on measures of distances, reserving the measures of position angle for the orientation of the entire group after the solution of the triangles. The observations began in February 1889, and are continued till March 1892, and embrace forty-five stars of the group. The combined measures give rise to 123 measured distances, and each of these is compared with the distance computed from Asaph Hall's catalogue of the stars of the Præsepe Group ("Washington Observations," 1869, Ap. iv.), giving rise to as many equations of condition. These are collected into an enormous normal equation of seventy-four unknowns. The solution of such an equation is sufficient to make the boldest arithmetician waver, and seek some approximate solution, but Dr. Schur preferred to adhere strictly to the method of elimination proposed by Gauss, and after weeks of labour brought his work to a successful conclusion. Such a labour so carried out in the University of Gottingen, is a not unfitting tribute to the memory of the great mathematician whose name is connected with that particular form of solution. With a similar disregard to the quantity of labour involved, and with all the accuracy attainable, Dr. Schur finally fixes the coordinates of the forty-five stars under consideration.

A melancholy interest is attached to the second part of the memoir in which the results of Winnecke's measures are given to the world. The introduction is the work of that distinguished astronomer, and it will be a matter of sincere regret to all that his state of health has not permitted him to continue to the end an investigation of so much value and thoroughness. That the task of completion and editing has fallen to Dr. Schur is fitting and appropriate, and must have been to him a labour of love. The principal difference in the methods of observation at Bonn (where Winnecke's observations were made) and Göttingen consists in the greater reliance placed by Winnecke on the measures of position angle, a confidence scarcely warranted by the probable error deduced from the observations, which Dr. Schur gives as follows:—

Probable error in distance of 2000″ ... $= \pm\, 0''·218$
,, ,, in position angle (in a great circle) $= \pm\, 0''·379$

The final result is to give a catalogue of the places of 45 stars for the epoch 1858, which are comparable with the catalogue of Dr. Schur for the epoch 1890·54. The comparison of these two catalogues and the discussion of the proper motion forms the third section of the work.

Dr. Schur first examines the relative accuracy of the two catalogues, and decides in favour of the more modern, in the proportion shown by the following:—

	Göttingen.	Bonn.
Probable error of distance (4000″)	$\pm\, 0''·193$ ...	$\pm\, 0''·354$
,, ,, position angle	$\pm\, 0''·359$ ...	$\pm\, 0''·506$

From considerations based on these and similar facts drawn from meridian observations, Dr. Schur concludes that a difference of 0″·27 in the place assigned to a star in the two catalogues can hardly be regarded as a proof of the existence of proper motion. The difference between the coordinates both in R.A. and Declination, though larger than this quantity, is everywhere small and negative. The proper motion of ten of the stars has also been

determined by Dr. Auwers from the meridian observations of Bradley and Mayer, and these show in the mean a correction to the heliometrically deduced proper motions of $-0''.0003$ and $+0''.030$ in R.A. and Declination, respectively. This discrepancy is subsequently traced to corrections due to the fundamental catalogues employed, and the final star places given on p. 208 9 possess an accuracy that will make them of value for many purposes.

Finally, a comparison is instituted between the proper motion of the group as observed, and the motion that might be expected from the progressive motion of the solar system. The result is not in very satisfactory agreement. The parallactic displacement of the solar system is

	$\Delta\alpha$	$\Delta\delta$
	$0''.0010$	$-0''.020$
Proper motion, Auwers	$-0''.0044$	$+0''.007$
„ „ other sources	$-0''.0041$	$-0''.032$

The question of absolute parallax enters here, and to this point Dr. Schur promises to return, possibly in connection with photographic researches.

W. E. P.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following appointments have recently been made abroad:—Bale, Dr. R. Metzner, of Freiburg, to the Chair of Physiology: Barcelona, Dr. Gil Saltor Lavall to the Chair of Surgical Pathology: Breslau, Dr. Jacobi, Professor of Forensic Medicine: Bonn, Dr. Finkler, Ordinary Professor of Hygiene: Columbian University, Wisconsin, Dr. W. Reed to the Chair of Bacteriology and Pathology, and Dr. M. T. Phillips to that of Hygiene: Granada, Dr Rafael Molla y Rodriguez, of Havana, Professor of Clinical Surgery: Genoa, Dr. Canalis, Ordinary Professor of Hygiene: Harvard, Dr. H. C. Ernst, Professor of Bacteriology: New York (Polyclinic) Dr. Wilbur B. Marple Professor of Ophthalmology, Dr. W. R. Pryor Professor of Gynæcology, and Dr. W. R Townsend, Professor of Orthopædic Surgery: Prague (Bohemian University), Dr. J. V. Rohon Extraordinary Professor of Histology: Tomsk, Dr. F. Kruger Extraordinary Professor of Medical Chemistry: Wurzburg, Dr. K. Rieger Ordinary Professor of Psychiatry: Zurich, Dr. H. von Wyss Extraordinary Professor of Forensic Medicine.

DR. J. H. HYSLOP has been appointed Professor of Logic and Ethics in Columbia College, New York. Dr. J. Allen Gilbert, of Yale, goes to the University of Iowa as Assistant Professor of Psychology.

ACCORDING to *Science*, Dr. Wilhelm Roux, of Innsbruck, has been called to the chair of Anatomy in the University of Halle; Dr. K. Seubert, of Tubingen, to the chair of Chemistry in the Technical High School, Hanover, and Dr. Kallius, of Göttingen, to the chair of Anatomy at Tubingen.

MESSRS. E. B. TITCHENER AND J. E. CREIGHTON have been made full professors in the Sage School of Philosophy in Cornell University.

PROF. MARK W. HARRINGTON has accepted the presidency of the University of Washington.

THE Aberdeen Town Council have agreed to give an annual contribution of £200 for the establishment of a department for instruction in agriculture, in connection with the University of Aberdeen, provided that a similar sum be given by the County Council.

THE prospectus of the Science, Art and Technical Schools, Plymouth, for the fourth session, 1895-96, has been issued. Copies may be had of the Secretary.

WE have received a copy of the syllabus of lectures to be delivered in the Engineering Department of the City of London College, Moorfields, during the coming session.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 9.—M. Marey in the chair.—A memoir was presented by M. Wladimir de Nicolaiew, entitled "On an attempt to show currents of electric displacement [illegible] the magnetic induction of iron in the alternative [illegible]."—Results of solar observations, made at the Royal Observatory of the Roman College, during the first quarter of 1895, by M. P. Tacchini. The diminution of frequency of spots was maintained during this quarter with a secondary minimum in January. Protuberances showed the same minimum although the season was unfavourable for their observation.—On the forces developed by differences of temperature between the two main plates of a beam with continuous trusses, by M. H. Deslandres. From the experiments made, differences of temperature between the upper and lower plates of a continuous girder cause supplementary forces of compression and extension, frequently reaching in the hot season 2 kg. per millimetre.—Observations on M. Deslandres' note, by M. Maurice Lévy. An exact demonstration giving the means of deducing the strains in every case.—On a theorem in geometry, by M. Mendeléef.—On nitro-substitutions, by MM. C. Matignon and Deligny. The conclusions are given: (1) Isomerides of position have always been found to have the same heats of combustion within the errors of experiment; one only need be examined from a number of isomerides. (2) The mean difference in heats of combustion of a compound and its nitro-derivative is 45 Cal. Hence is deduced the equation

$$RCH + NO_3H \text{ liq.} = RCNO_2 + H_2O \text{ liq.} + 36.7 \text{ Cal.}$$

that is, the exact value found by Berthelot for the formation of nitro-hydrocarbons.—On the explosion of endothermic gases, by M. L. Maquenne. The conditions of propagation of an explosive wave initiated by detonators are given, and the influence of this explosive character on the industrial applications of acetylene is pointed out.—Influence of the winter 1894-95 on the marine fauna, by M. Pierre Fauvel.—On a gigantic terrestrial tortoise, from a specimen living in Egmont Islands, by M. Th. Sauzier. Dimensions are given of a specimen of *Testudo Daudinii*, and compared with the dimensions of other known tortoises and the fossil *T. Perpiniana*. Results of palæontological excavations in the Upper Miocene of the "colline de Montredon," by M. Ch. Depéret.—On a superior limit to the mean area affected by an earthquake, by M. F. de Montessus de Ballore. From Japanese observations it is deduced that this higher limit is 1200 square kilometres.

BOOKS, PAMPHLET, and SERIALS RECEIVED.

BOOKS.—Mental Physiology: Dr. T. B. Hyslop (Churchill).—A Text-Book on Applied Mechanics: Prof. A. Jamieson, Vol. 1 (Griffin).—Justus von Liebig: W. A. Shenstone (Cassell).—The English Lakes: Dr. H. R. Mill (Philip).—Light: H. P. Highton (Rivington).—Facts about Processes, Pigments, and Vehicles: A. P. Laurie (Macmillan).—Ostwald's Klassiker der Exakten Wissenschaften, No 63 to 66 (Leipzig, Engelmann).—Müller-Pouillet's Lehrbuch der Physik und Meteorologie, new edition, by Drs. Pfaundler and Lummer (Braunschweig, Vieweg).—British Museum (Natural History) Mineral Department: An Introduction to the Study of Rocks (London).

PAMPHLET.—The Cost of Warships: Dr. F. Elgar (Institution of Naval Architects).

SERIALS.—American Naturalist, September (Philadelphia).—Psychological Review, September (Macmillan).—Strand Magazine, September (Newnes).—Picture Magazine, September (Newnes).

CONTENTS.

THURSDAY, SEPTEMBER 26, 1895.

PERSONALITY.

The Diseases of Personality. By Th. Ribot. Authorised translation. Second revised edition. (Chicago: The Open Court Publishing Company, 1895.)

THE importance of a work bears little relation to its bulk, so no surprise need be felt at a masterly and very suggestive *résumé* of recent inquiries into a question of the highest interest being compressed into this thin volume of less than 160 pages of good readable type. The work itself is not new, though it is so in its present translated form. It is practically up to date, and affords an excellent study for those to whom what Tennyson calls "the abysmal deeps of Personality" are wholly mysterious, as well as to those others who have sounded them in part.

First as regards consciousness: there are two views, the old and the new. The old view regards it as the fundamental property of the soul or mind; the new view regards it as an event superadded to the more regular activity of the brain, depending on conditions as yet unknown, and appearing or disappearing according to their presence or absence. The old view fails to account for the vast substratum of unconscious mental activity whose existence is now beyond dispute, and it apparently fails to account for intermissions of consciousness, whose existence can hardly be denied even when the fullest allowance is made for the effects of forgetfulness. The new view is simpler than the old one, and much more consistent with observed facts, especially such as are obtained from the study of mental disease, which is a subtle analyser of mental functions. Many persons are loth to admit that the highest manifestations of the human mind are fugitive phenomena, subordinate to those of a lower grade; but whatever be the origin of consciousness, its value is none the less. From the point of view of the evolutionist, it is not the origin of a faculty that is of consequence, but the elevation to which that faculty attains. However consciousness may have come into existence, its first appearance on the earth must have been a fact of the first magnitude, for it is the basis of the recollections, which capitalise the past of each animal for the profit of its future, and give it new chances of survival. On the automaton view of life, consciousness changes the animal from a simple automaton into one of an incomparably higher order. The author quotes much from "Les colonies animales" of Perrier, to show the steps through which consciousness first became developed in the animal world, starting from associations of individuals that are almost independent of one another, but which, owing to their contiguity and mutual pressure, cannot be wholly unaffected by their neighbours. The next step is the appearance of a colonial consciousness, where a colony is formed of individuals in which some division of labour takes place, and the function of locomotion is centralised. But because a colony acquires colonial consciousness, it does not follow that each of the individuals that compose it loses its particular consciousness; thus the severed ray of a star-fish continues to creep, to follow, or, it may be, to deviate under conditions from a given route, and to quiver when excited, and thus to betray a consciousness of its own which, before it was severed, was subordinated to the consciousness of the whole star-fish. By degrees this colonial consciousness confiscates for its benefit all the particular ones.

The author maintains that consciousness is not like a central point from which alone feelings radiate and to which they all arrive, but that it is a complexus of separate phenomena, each of a particular class, bound up with certain unknown conditions of the brain, existing only when they exist, lacking when they disappear. Hence the sum of the states of consciousness in man is very inferior to the sum of all his nervous actions. Conscious personality is only an abstract of the vast amount of work that takes place in the nervous centres. Its basis is formed by the diffused bodily sensations which, being elementary causes, serve as a warp upon which is woven some gorgeous pattern of tapestry that corresponds to the higher feelings. The general consciousness of the organism serves as the support of all the rest, and forms, in the author's opinion, the real basis of conscious personality.

Personal identity is an unsatisfactory phrase. A man feels to be the same in his ego at different periods, because the great majority of his bodily feelings continue the same, owing to his structural sameness. The so-called identity is due to the large preponderance of unchanging elements, which characterise a healthy state; but in disease this habitual predominance may fail either wholly or temporarily, leading in the one case to a sense of a complete change of personality, in the other to that of multiple and alternating personalities. A few but adequate number of specimen cases are given. A somewhat comic instance is that by Hack Tuke, of a patient who had lost his ego (that is the one which was familiar to him), and was in the habit of searching for himself under his bed. (*Cf.* the speech of Saturn, "Search Thea, search . . ." in Keats' "Hyperion.")

The rather common cases in which a man believes himself to have become changed into a new person, are considered by the author to be mostly superficial; that is, to be due to local rather than to general disorder. I myself witnessed a case which showed that the imagined personality was not well sustained. It was at a lunatic asylum, where I went accompanied by a photographer to take specimens for composite photography. He mounted his camera in a ward, and a batch of patients were brought up. One of them was duly placed in front of the camera, the others were led to a bench behind the operator to wait their turn. It happened that one of these had the mania that he was a great commander, let us say, Alexander the Great, and he chafed internally at not having had precedence. When my photographer's head was under the dark cloth, and his body in the attitude appropriate to the occasion, Alexander the Great could restrain himself no longer, but nipped the projecting rotundity of the poor man's hinder end with his teeth. I abstain from dwelling on the tableau, or on the care with which the smarting photographer, in his further operations, squeezed himself into a corner that guarded his rear. The point is this, that a man who was thoroughly pervaded with the idea of being

a mighty conqueror, would not have made that kind of attack.

Without attempting to condense further this already condensed and very readable little volume written by a distinguished inquirer, I will conclude by saying that it well deserves a place in any general library.

FRANCIS GALTON.

SATELLITE EVOLUTION.

Satellite Evolution. By James Nolan. Pp. 114. (Melbourne, &c.: George Robertson and Co., 1895.)

IN this book Mr. Nolan discusses the part played by tidal friction in the evolution of satellites. Although the subject is one of much scientific interest, his work is hardly likely to attract the attention it deserves, because the unmathematical reader will find the reasoning hard to follow, whilst the mathematician will be repelled by prolixity, due to the author's treatment of the problem by means of general reasoning.[1] The first fifty pages of the book appear to be virtually contained in the single equation which states the effect of tidal friction in increasing the mean distance of a satellite. It might perhaps be interesting to some to discuss the various elements of the problem in detail, but those who are able to comprehend an analytical formula are not very likely to have the patience to follow such a discussion.

I shall not accordingly follow Mr. Nolan in detail, but will pass at once to the conclusion to which he tends. On p. 9 he says:—

"Though Mr. Darwin made elaborate calculations to support his theory respecting the part played by tidal friction on the evolution of the earth and moon, he seems to have dismissed the Jovian and Saturnian systems with the conclusion that their satellites, unlike our moon, could not be traced much further in than the present distances of their respective planets; and that as the relation between the mass of the planet and satellite, or relation of rotational to orbital momentum is very different in the case of the earth and moon to that for other planets and satellites, their modes of evolution may have differed considerably. He seems to have gone something further into the possible effects of solar tidal friction on the planets revolving round the great central body, or at least has come to the correct conclusion that the efficiency of such tides would be too small to effect any appreciable change during the natural lifetime of a solar system."

He then proceeds to show that, if the earth and Jupiter rotate under the influence of tides subject to the same frictional resistance, the proportionate rate of increase of the moon's mean distance is much smaller than that of all of Jupiter's satellites, save one. In other words, four out of five of Jupiter's satellites would have their mean distances increased by, say, one per cent. in a much shorter time than would the moon. He then pursues the same train of reasoning with respect to Saturn and Mars.

It appears to me that Mr. Nolan is correct in these conclusions, and we are thus led to suppose that tidal friction may have played a much more important part in the evolution of satellites than I was disposed to allow it.[1] He points out (p. 70) that the satellites of Jupiter are probably much younger than the moon; "when the moon was younger, her relative rate of recession was faster, as now is the case for some satellites in other systems." He finally concludes (p. 78) that the majority of satellites in each system may be traced to a position corresponding with that of the rings of Saturn.

But before arriving at this result, the author has treated another problem, in which, in my opinion, his conclusion is incorrect. On p. 45, he considers the effects of tidal friction on such a ring as that of Saturn. He says:—

Tidal friction "could have no effect if the ring were perfectly even all round. When composed of individual bodies it could not be or remain so. Each individual would be unaffected by the tides of the others, and would recede at the same rate as if it were the only body in the ring. The moon recedes at exactly the same rate as she would were there no solar tides: and if there were a second moon there would be no interference with the recession of the first . . . Then if the bodies composing the rings are 'as the sand on the sea shore for multitude' tidal friction must still effect the usual progressive change, unless each individual body be small enough to be unaffected at the distance, whether composing a ring or not. This must have a dissolving effect on the ring, or tend to shape certain sections of it into so many bodies, which, having increased their mass at the expense of the ring, finally recede therefrom, either to circle round at a great distance or be precipitated into the planet increasing its rotation speed."

It would seem that the process here sketched is an essential part of Mr. Nolan's theory of the evolution of satellites, but I believe it to be founded on erroneous premises. He omits in fact to notice the necessary condition for neglecting the effects of the tides raised by one satellite on the mean distance of another; this is, that the periodic times of the two shall not be equal to one another. If the periodic times of two satellites are unequal, we need not invoke tidal friction to bring the two bodies near to one another. On the other hand, if four or eight satellites be equally spaced round a planet and revolve with the same periodic time, tidal friction would only influence their motions to a very small extent. I am therefore unable to follow Mr. Nolan in this part of his work.

Several other points in the early history of satellites are considered by Mr. Nolan, but I am unable to touch on them within the limits of a review.

Notwithstanding all that has been written by him and others, we are still far from a consistent theory of the formation of a satellite. In my own papers I have ventured to throw out suggestions (which have but too often been quoted as positive theories), and it still seems to me at least, that neither the present contribution of the author nor the theories of others are adequate.

This work touches on subjects of interest, and although it seems open to much criticism, I for my part welcome the extension given by Mr. Nolan to the part played by tidal friction in evolutionary astronomy.

G. H. DARWIN.

[1] [illegible]

[1] The argument by which I was led to an erroneous conclusion on this point will be found in *Phil. Trans.*, part ii., 1881, p. [illegible].

OUR BOOK SHELF.

Die Lehre von der Elektrizitat und deren Praktische Verwendung. By Th. Schwartze. (Leipzig: J. J. Weber, 1895.)

THE author in his preface says that his intention in writing this book was to give the bearing of the latest scientific results in electricity on electro-technology. He goes on to say that the contents will probably appear peculiar. The first of the above statements, taken in conjunction with the title of the book, will probably give as erroneous an idea of the contents as it is possible to obtain. For if there is one thing the author does not do, it is to give the bearing of the few modern discoveries, or lines of thought, which he mentions on the practical applications of electricity.

For all intents and purposes the book may be divided into two parts. The first of these deals with the question of the fundamental principles of general physics and with some mechanical problems, such as moment of inertia, oscillations of a pendulum, wave-motion, &c. The second part deals more particularly with electric and magnetic phenomena.

Throughout the greater part of the book, but particularly in the first part, the reader will probably heartily endorse the author's view, that the contents of the book are peculiar; for the subject of dimensions is treated at great length, so that, for at any rate the first three hundred pages, there is hardly a page without at least one dimensional equation. The appearance of some of these dimensional equations, however, are certainly peculiar, for the author attempts to introduce a set of dimensions in terms of what he calls "Linearkraft," "Flächenkraft," and "Volumenkraft." These quantities he indicates by the symbols L, L^2 and L^3, regardless of the fact that in those dimensional formulæ, in which length, mass, and time are taken as the fundamental units, the symbol L is used for a length. Even the author himself seems to have got muddled when such equations as $[M^2L^2]=[ML^2]$ are allowed to appear, and the state of mind of the student, whose command of dimensions is limited, after reading the book, is lamentable to think of. In the chapter dealing with the dimensions of the electrical and magnetic units, no mention is made of the effect of the properties of the medium, and although Rücker's name is mentioned in the preface in connection with the subject of dimensions, no mention is made of his proposal to consider the specific inductive capacity and the permeability of the medium as subsidiary fundamental units, and to indicate their presence in the dimensional formulæ. The more purely electrical portion of the book calls for little remark, and contains a somewhat elementary treatment of the subject of electrostatics, such as the calculation of the capacity of some simple forms of condensers, &c. There are also chapters dealing with uni-directed currents, thermo-electricity, electrolysis, electro-magnetic induction, and the dynamo. Finally, about seventy pages are devoted to what is called "electro-tectiniches," in which the commoner forms of electrical measuring instruments are shortly described.

While only a very short account is given of Hertz's work, contrary to what one would expect in a German work, considerable space is devoted to a description of Elihu Thomson's more showy experiments with rapidly alternating currents.

LETTERS TO THE EDITOR.

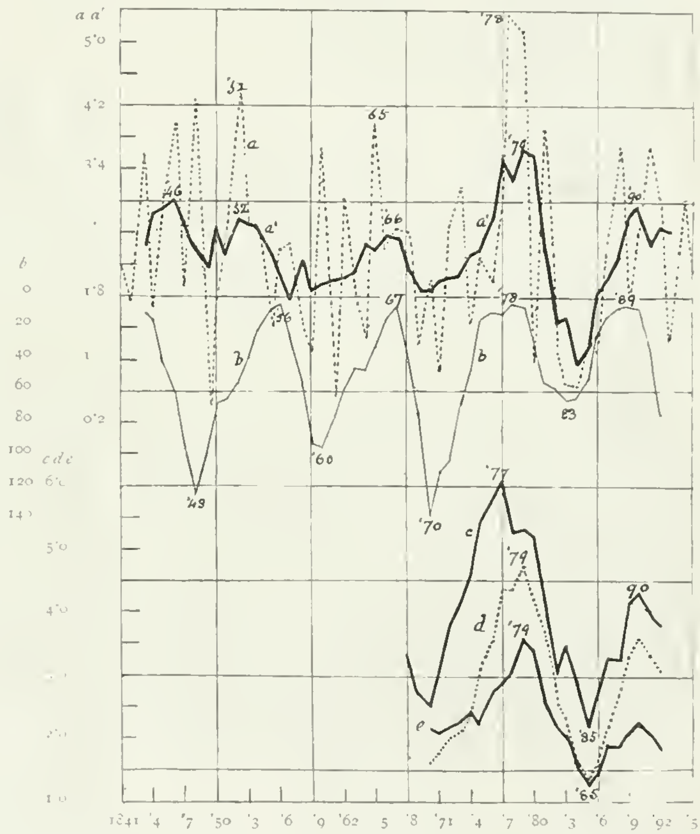
[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Rain in August.

AUGUST being a harvest month, and the holiday month *par excellence* in this country, its weather is a matter of concern to multitudes. I propose to show how the rainfall of August at Greenwich has varied in the last half-century (1841–95).

This variation appears to me rather to suggest sun-spot influence: but whatever may be thought about this, it may be interesting to observe how far the kind of correspondence here pointed out is maintained in the future.

In the accompanying diagram we have (*a*) a dotted curve showing the variation of August rainfall, and the values have been smoothed with averages of 5, yielding the continuous curve *a'*. Underneath (*b*) is an inverted sun-spot curve.



a. Rainfall in August, Greenwich. *a'*. The same, smoothed (5-av.). *b.* Inverted sun-spot curve. *c, d, e.* Rainfall in August at Haverfordwest, Llandudno, and Boston (smoothed).

A considerable correspondence may here be traced, especially in the last three waves; the crests or maxima of the smoothed rainfall curve coming near the sun-spot minima, and the hollows or minima of the former near the sun-spot maxima.

It seems specially noteworthy that in each year following a sun-spot maximum year we have had a very dry August. Thus (the August average being 2·38) we have:

Sun-spot max.	1848	...	Rainfall of August		1849, 0·45 in.
,,	1860	...	,,	,,	1861, 0·57 ,,
,,	1870	...	,,	,,	1871, 0·86 ,,
,,	1883	...	,,	,,	1884, 0·67 ,,

The data previous to 1841 are, I suppose, less reliable: but I may add these two cases of August rainfall under average:

Sun-spot max. 1830	...	Rainfall of August 1831, 2·14 in.
" 1837	...	" " 1838, 0·93 "

By way of showing that in other parts of the country there has been, in recent years at least, a similar variation, I add three similarly smoothed curves of August rainfall for Haverfordwest, Llandudno, and Boston (Lincoln) respectively ([illegible], *d*, *e*). The data, however, do not extend back further than 1866.

The case of Greenwich may be presented as follows:—Take each maximum sun-spot year, and a year on either side, and tabulate the August rainfall in each of these. Indicate by the letters *d* (for dry) and *w* (for wet) whether this rainfall has been below or above the average. Then we have:

Maximum.						
1847, 1848, 1849	...	1·95	4·25	0·45	...	*d w d*
1859, 1860, 1861	...	1·13	3·68	0·57	...	*d w d*
1869, 1870, 1871	...	1·21	2·02	0·86	...	*d d d*
1882, 1883, 1884	...	1·16	0·71	0·67	...	*d d d*

Here we find ten cases of a dry August out of twelve. Those twelve values give an average of 1·55 inches.

Now do the same with minimum sun-spot years:—

Minimum.						
1842, 1843, 1844	...	1·78	3·62	1·71	...	*d w d*
1855, 1856, 1857	...	1·40	2·42	2·50	...	*d w w*
1866, 1867, 1868		2·42	2·04	2·61	...	*w w w*
1877, 1878, 1879	...	2·00	5·38	5·19	...	*w w w*
1888, 1889, 1890	...	3·73	1·81	2·54	...	*w d w*

Here we find eleven cases of a wet August out of fifteen. Those fifteen cases give an average of 2·84 inches.

It would be interesting to know to what extent such relations exist elsewhere, and perhaps some of your readers may be disposed to investigate the matter.

A. B. M.

Alteration in the Colours of Flowers by Cyanide Fumes.

It is well known that the yellows of some insects are turned to red by the fumes from potassium cyanide; but I have not, after some inquiry, been able to obtain any literature describing the effects of said fumes upon the colours of flowers. The reactions I have observed are very curious, and while it seems improbable that they are hitherto wholly unknown, it may not be amiss to direct attention to them. A few lumps of the cyanide are placed in a corked tube, covered with a little cotton, and the flowers are placed on the cotton. It is probably necessary that the day should be hot, or the tube slightly warmed. The pink flowers of *Cleome integrifolia* and *[illegible]* turn to a brilliant green-blue, and finally become pale yellow. A purple-red *Verbena* becomes bright blue, then pale yellow. The purple flowers of *Solanum elaeagnifolium* go green-blue and then yellow. The white petals of *Argemone [illegible]* turn yellow—the natural colour of *A. mexicana*. The pale yellowish flowers of *Mentzelia nuda* turn a deeper yellow. Flowers of *[illegible]*, var., turn pale yellow. White flowers of *[illegible]* turn yellow. The scarlet flowers of *[illegible]* turn pale dull pink, resembling somewhat a natural variety of the same. Any of your readers will doubtless obtain similar results with the flowers growing in their vicinity.

T. D. A. COCKERELL.

Las Cruces, New Mexico, U.S.A., September 3.

ON THE CONSTITUENTS OF THE GAS IN CLEVEITE.

WE have investigated the spectrum of the gas discovered in the mineral cleveite by Ramsay, and have found it to be most regular. It consists of six series of lines, the intensity of the lines in each series decreasing with decreasing wave-lengths. Similar series of lines have been observed in many spectra. The first series was discovered by Dr Huggins in the ultra-violet spectra of a number of stars. It proved to belong to hydrogen, and to be the continuation of the four strong hydrogen lines in the visible part of the spectrum. Johnstone Stoney had already shown that three of the wave-lengths of the visible hydrogen lines were most accurately proportional to the values 9/5, 4/3, 9/8, when Balmer discovered that these values were given by the formula

$$\frac{m^2}{m^2 - 4}$$

for $m = 3, 4, 6$, and that the other wave-lengths of the series were proportional to the values obtained by substituting for m the other entire numbers greater than three. The series has now been followed from $m = 3$ to $m = 20$, the lines growing weaker and weaker to the more refrangible side, and approaching each other closer and closer. The formula shows that they approach a definite limit for large values of m. This is seen more clearly when we consider wave-numbers instead of wave-lengths, which according to the formula would be proportional to

$$1 - \frac{4}{m^2}.$$

Many series of lines similar to the hydrogen series were discovered by Liveing and Dewar. They have called them harmonic series, and have compared them to the series of over-tones of a vibrating body. They have been further studied by Rydberg and by Kayser and Runge. We cannot here enter into any detailed account. We only want to explain so much as to make the conclusions understood which we have drawn from the spectrum of the gas in cleveite. The wave-lengths λ of the lines belonging to the same series are always approximately connected by a formula somewhat similar to Balmer's

$$1/\lambda = A - Bm^{-2} - Cm^{-4}.$$

A determines the end of the series towards which the lines approach for high values of m, but does not influence the difference of wave-numbers of any two lines. B has nearly the same value for all the series observed, and C may be said to determine the spread of the series, corresponding intervals between the wave-numbers being larger for larger values of C. As B is approximately known, two wave-lengths of a series suffice to determine the constants A and C, and thus to calculate approximately the wave lengths of the other lines. It was by this means that we succeeded in disentangling the spectrum of the gas in cleveite, and showing its regularity.

In the spectrum of many elements two series have been observed for which A has the same value, so that they both approach to the same limit. In all these cases the series for which C has the smaller value, that is to say which has the smaller spread, is the stronger of the two. In the spectrum of the gas in cleveite we have two instances of the same occurrence. One of the two pairs of series, the one to which the strong yellow double line belongs, consists throughout of double lines whose wave-numbers seem to have the same difference, while the lines of the other pair of series appear to be all single. Lithium is an instance of a pair of series of single lines approaching to the same limit. But there are also many instances of two series of double lines of equal difference of wave-numbers ending at the same place as sodium, potassium, aluminium, &c. There are also cases where the members of each series consist of triplets of the same difference of wave-numbers as in the spectrum of magnesium, calcium, strontium, zinc, cadmium, mercury. But there is no instance of an element whose spectrum contains two pairs of series ending at the same place. This suggested to us the idea that the two pairs of series belonged to different elements. One of the two pairs being by far the stronger, we assume that the stronger one of the two remaining series belongs to the same element as the stronger pair. We thus get two spectra consisting of three series each,

two series ending at the same place, and the third leaping over the first two in large bounds and ending in the more refrangible part of the spectrum. This third series we suppose to be analogous to the so-called principal series in the spectra of the alkalis, which show the same features. It is not impossible, one may even say not unlikely, that there are principal series in the spectra of the other elements. But so far they have not been shown to exist.

Each of our two spectra now shows a close analogy to the spectra of the alkalis.

We therefore believe the gas in cleveite to consist of two, and not more than two, constituents. We propose to call only one of the constituents helium, the one to which the bright yellow double line belongs, whose spectrum altogether is the stronger one, while the other constituent ought to receive a new name.

We have confirmed this rather hypothetical conclusion by the following experiment. The connection leading from our supply of cleveite gas to the vacuum tube contained a side branch parting from it and joining it again. There were stopcocks on either side of the side branch, and a third one in the side branch. In the main tube between the ends of the side branch a plug of asbestos was tightly inserted. To prepare the vacuum tube only the tap leading to the supply was closed, the whole space up to this tap being carefully evacuated. Now the side branch was closed, and the tap leading to the supply was opened. Then we observed that the light of the electric discharge in the vacuum tube was at first greenish, and after a while grew yellow. By cutting off the current of gas after a sufficiently short time, we succeeded in making a vacuum tube which remained greenish. On examining it in a small spectroscope with which we could overlook the whole spectrum, we found that the intensities of the lines had changed. The yellow line was scarcely as bright as the green line 5016, and the red line 7065 had apparently decreased relatively to 7282 and 6678, although it was still stronger than 7282. The two lines that had decreased in intensity belong to the second set of series, while the others are members of the first set. The other visual lines of the second set could not very well be examined because they are more in the violet part.

This observation confirms our spectroscopic result. The gas in cleveite may be taken to be a mixture of two gases of different density, of which the lighter one is more rapidly transmitted through the plug of asbestos. There is, however, the objection to be raised, that in the green tube the pressure is less, and that the difference of intensities is due to the pressure being different. This must be further inquired into.

We were not satisfied with the visual observation of the change of intensities in our green tube, but thought it desirable to test the conclusion by the bolometric measurement of the two lines that we have discovered in the ultra-red part of the spectrum. If we were right, the ultra-red line of smaller wave-length, which belongs to the second set of series, ought to have decreased in intensity relatively to the other ultra-red line. This we found to be so indeed. In the yellow tubes the intensity of the smaller wave-length was to that of the other on an average as 3 to 1, while in the green tubes it was as 1·8 to 1. This confirmation we consider the more valuable as it does not depend on any estimation which may be biassed by the personal opinion of the observer, but is based on an objective numerical determination.

Another confirmation may be gathered from the spectrum of the sun's limb and that of several stars. Let us confine our attention to the six strongest lines in the visible part of the spectrum :

7066, 6678, 5876, 5016, 4922, 4472.

The first, third, and sixth belong to the second set of series ; the second, fourth and fifth to the first set. These six lines have all been observed in the spectrum of the sun's limb, as Norman Lockyer and Deslandres have pointed out. Now, according to their appearance in the spectrum of the sun's limb, they may be classed in two groups, one group being always present, the other group being sometimes present. C. A. Young long ago called attention to the difference in the frequency of appearance of the chromospheric lines. He has given them frequency numbers, roughly estimating the percentage of frequency with which the lines were seen during the six weeks of observation at Sherman in the summer of 1872. According to Young, 7066, 5876, 4472 have the frequency number 100, while 6678, 5016, 4922 have the numbers 25, 30, 30, showing that one of the two constituents was always present, while the other was only seen about once in every four cases.

The lines of both constituents have been observed in the spectra of a considerable number of stars β, δ, ϵ, ζ, γ Orionis, α Virginis, β Persei, β Tauri, η Ursæ majoris, β Lyræ. In the spectrum of β Lyræ, thirteen lines have been identified with certainty. But the most interesting case in point is the spectrum of Nova Aurigæ, that wonderful star whose sudden appearance was announced to astronomers in 1892 by an anonymous post-card. In the spectrum of Nova Aurigæ the two lines 5016 and 4922 were very strong, while 4472 was weak and 5876 has only been seen by Dr. Huggins, we believe only on one occasion, and appears to have been very weak. Now 5016 and 4922 belong to the lighter constituent, and are together with 6678 the strongest lines in the visible part of the spectrum ; while 5876 and 4472 are the strongest lines of the other constituent in the visible part of the spectrum. In Nova Aurigæ, therefore, the lighter constituent gave a much brighter spectrum than helium proper. But there may here be raised an objection, which indeed we do not know how to refute. Why has the line 6678 not been observed ? It is a pity that the red part of the spectrum cannot be more easily photographed. Nova Aurigæ has now become very weak, and besides the spectrum is quite altered, so that we shall never know whether the red line 6678 was really absent or has only escaped notice.

From the fact that the second set of series is on the whole situated more to the refrangible part of the spectrum, one may, independently of the diffusion experiment, conclude that the element corresponding to the second set is the heavier of the two. In the spectra of chemically related elements like Li, Na, K, Rb, Cs, or Mg, Ca, Sr, or Zn, Cd, Hg, the series shift to the less refrangible side with increasing atomic weight. But it appears that in the spectra of elements following each other in the order of their atomic weights in a row of the periodic system like

Na, Mg, Al ;
K, Ca ;
Cu, Zn ;
Rb, Sr ;
Ag, Cd, In ;

the series shift the opposite way, so that the spectrum of the element of greater atomic weight is as a whole situated further to the more refrangible side. Now in our case the density of the gas has been determined by Langlet (published by Cleve) and by Ramsay to be about double the density of hydrogen. Assuming the atomic weights of the two constituents to be between that of lithium and that of hydrogen, they would both belong to the same row of the periodic system, and therefore the more refrangible set of series would correspond to the greater atomic weight.

For convenience of reference all the observed lines are given in the following table, the wave-lengths being abridged to tenth-metres.

Lighter Constituent.

Principal series.		First subordinate series.		Second subordinate series.
20400		6678	...	7282
5016	...	4922	...	5048
3905	...	4388	...	4438
3614		4144	..	4169
3448	.	4000	.	4024
3355	...	3927	...	3936
3297	...	3872	..	3878
3258		3834	..	3838
3231		3806	..	3808
3213		3785	...	

Heavier Constituent (Helium proper).

		Double lines.		Double lines.
11220	.	5876	...	7066
3889	.	4472	...	4713
3188	..	4026	...	4121
2945	..	3820	...	3868
2829	...	3705	...	3733
2764	...	3634	...	3652
2723	..	3587	...	3599
2696	...	3555	...	3563
2677	..	3531	..	3537
		3513	..	3517
		3499	...	3503
		3488	...	3491
		3479	...	3482
		3472		
		3466		
		3461		

C. RUNGE AND F. PASCHEN.

NOTES.

THE third International Congress of Zoologists (an account of the proceedings at which will appear in a subsequent issue of NATURE) has just been held at Leyden, and appears to have been a great success. No fewer than twenty nationalities were represented, and the arrangements for the comfort of the members were all that could be wished. It was decided to hold the next meeting (in 1898) in England, and Sir William H. Flower was elected President. During the meeting it was announced that the Senate of the University of Utrecht had conferred degrees upon Sir William H. Flower, M. Milne-Edwards, of Paris, and Prof. Weismann, of Freiburg.

TELEGRAMS from St. John's, dated September 22, announced the return, in the steamer *Kite*, of the Peary Expedition. The result of the expedition was a most disappointing one, as Lieut. Peary and his companions were unable to extend their journeyings beyond Independence Bay, which point was the furthest north reached by Lieut. Peary in his expedition of 1892. The main cause of failure was the loss of all the stores of provisions, save one, which had been got together and deposited along the intended line of march last year, all having been buried by perhaps the heaviest snowfall known, which obliterated all traces of them. The sufferings endured by the explorers, on the verge of starvation as they were for the greater part of the time, can hardly be estimated, and when, on July 31, the *Kite* arrived, they were utterly broken down and ill, but they subsequently recovered under careful treatment. The expedition, according to a later telegram, will not be entirely barren of scientific results, as Lieut. Peary is reported to have mapped Whale Sound, and completed his studies of the Eskimo Highlanders. He has also brought back another year's meteorological record. The relief expedition, too, is credited with obtaining the largest collection of Arctic fauna and flora ever acquired, and Prof. Salisbury, of Chicago University, did good geological work.

A COMMUNICATION was made to the press on Friday last by Reuter's Agency with reference to the movements of the Jackson-Harmsworth Polar Expedition. It was admitted that the intelligence received had been made in a somewhat meagre and disjointed form; but from it could be gleaned that on September 7 of last year the expedition arrived safely on the coast of Franz Josef Land and in the locality of Cape Flora. On September 10 the ice closed round the *Windward*, and she was frozen in for the winter. On February 23 the sun returned, and on March 10 Mr. Jackson started on his northern journey, with a quantity of stores, and made his first depôt. Various journeys to and fro with provisions, &c., were made, and depôts formed, the most northern of which was about 100 miles from the camp. The *Windward* has, it is expected, now set sail for home, bearing letters and journals of the early part of the expedition.

THE expedition to Alaska of the United States Geological Survey, for the purpose of examining into the coal and gold mines of the territory, has returned safely to San Francisco after a successful and very interesting season, during which, incidentally, many of the glaciers and volcanos were studied. Messrs. Becker and Dall will return to Washington by October 1, to submit their report upon the mineral resources to the Director of the Survey, which will be printed as soon as the necessary analyses, &c., can be made.

WE have to record the death, at Berlin, at the age of seventy-six, of Prof. Bardeleben, the eminent surgeon and author of "Lehrbuch der Chirurgie und Operationslehre."

THE death is announced, from Bendigo, Victoria, of Dr. Paul Howard MacGillivray, well known as a medical man and for his researches on Polyzoa.

AT the meeting of the Entomological Society of London, to be held on Wednesday, October 2, the following papers will be read:—"Contributions towards the History of Maruina, a New Genus of Diptera" (*Psychodidæ*), by Dr. Fritz Muller; "Remarks on the Homologies and Differences between the First Stages of Pericoma and those of Maruina," by Baron Osten Sacken.

THE annual meeting of the Federated Institution of Mining Engineers has just taken place at Hanley, and papers were read on "The Use of Steel Girders in Mines," "Economic Minerals of the Province of Ontario," and "Gold Mining in Nova Scotia." The Institution seems to be in a flourishing condition, the membership having risen from 1189 in 1889-90, to 2199 at the present time. The prizes for papers on "The Prevention of Accidents in Mines" have been awarded as follows: (1) Mr. A. Kirkup (2) Mr. W. N. Drew; Messrs. E. A. Allport and A. Noble were bracketed for the third place.

THE Royal Society of New South Wales offers its medal and the sum of £25 for the best communications (provided such be deemed of sufficient merit) on original research in the following subjects:—"The Origin of Multiple Hydatids in Man"; "The Occurrence of Precious Stones in New South Wales, with a description of the Deposits in which they are found"; "The Effect of the Australian Climate on the Physical Development of the Australian-born Population"; "The Physiological Action of the Poison of any Australian Snake, Spider, or Tick"; "The Chemistry of the Australian Gums and Resins"; "The Embryology and Development of the Echidna or Platypus"; "The Chemical Composition of the Products from the so-called Kerosene Shale of New South Wales"; "The Mode of Occurrence, Chemical Composition and Origin of Artesian Water in New South Wales." The competition is open to all, and is not subject to any restriction, save that the communication to be successful must be either wholly or in part the result of the competitor's own original observation or research. The suc-

cessful essays will be published in the Society's annual volume, and fifty copies of the paper will be supplied to their writers free of charge. Particulars as to the latest dates for sending papers, and all other necessary information, may be obtained from the Honorary Secretaries, at the house of the Royal Society of New South Wales, 5, Elizabeth-street, Sydney.

The Manchester Trades Council has recently passed a resolution strongly in favour of the Report of the Select Committee of the House of Commons on Weights and Measures, in which the Council expresses the hope that no efforts will be spared to make the Committee's recommendations law. As can be readily understood, the New Decimal Association is much encouraged by the attitude taken in so important a commercial centre as Manchester, and it is to be hoped that at no distant date their efforts will be crowned with success, and that the present cumbrous system will be for ever abandoned.

The metric system of weights and measures is to be obligatory in the United States of Mexico from September 16, 1896. This system has been in use in the Government departments of Mexico for some time past, but a decree recently passed makes it the sole legal system throughout the Republic, and will make an end of the various old Spanish measures hitherto in vogue in ordinary business transactions.

Dr. van Rijckevorsel and Herr van Bemmelen are engaged on a research which has for its object to determine the influence of elevation above sea-level on the magnetic elements. For this purpose an accurate magnetic survey must be made of some moderately high mountain, of non-magnetic material and sufficiently far removed from magnetic masses. The Righi seems to fulfil these conditions most satisfactorily; but in order to decide the matter, Herrn. van Rijckevorsel and van Bemmelen selected thirty stations, distributed on the low ground round the Righi in three concentric circles with the mountain as centre. The magnetic elements have been determined at these stations, but the calculations are not yet completed. If these indicate no traces of disturbance, due to the Righi or its surroundings, the survey of the mountain will be proceeded with.

The latest number of the *Records* of the Geological Survey of India contains a translation of a paper by Dr. F. Kurtz, on the Lower Gondwana beds of Argentina (from *Revista del Mus. de la Plata*). In this is recorded an important discovery of plant-remains in shales at Bajo de Velis. These fossils are well-preserved, and while being quite different from the Argentine plant-remains already found, show a close affinity to the plants of the Kaharbari beds of the Lower Gondwanas of India, as well as to those of the Ekka-Kimberley beds of South Africa, the Newcastle and Baccus-Marsh beds of Australia, and the Mersey beds of Tasmania. The previously-known plant-bearing beds of Argentina consisted of two series—one containing a Rhætic flora, resembling that of the Stormberg (Upper Karoo) beds of South Africa, the Hawkesbury beds of Australia, and the Rajmahal (Upper Gondwana) series of India; the other containing a flora of Lower Carboniferous character. The newly-discovered flora must be intermediate in age between these two—that is to say, it cannot be older than Upper Carboniferous, nor younger than Triassic; and with it must go the flora of the important coal-bearing Upper Gondwana beds of India. These have already been assigned to the Upper Carboniferous (at lowest) by Messrs. Medlicott and Blanford, and the Indian Survey, and the new discoveries in Argentina give a satisfactory confirmation of their views.

We note the publication of the first *Bulletin des Observations Météorologiques*, 1894, by the Observatory of St. Louis, St. Heliers, Jersey, containing monthly means from direct observations and from self-recording instruments. The Director of this new Observatory is the Rev. M. Dechevrens, who has already done good work at Zi-ka-wei, near Shanghai, and by the investigation of the typhoons of the China Seas, in connection with the Shanghai Meteorological Society. The St. Louis Observatory is provided with a tower about 150 feet high, for the special study of vertical wind currents and atmospheric electricity.

The Acclimatisation Society of Moscow must be credited with more than ordinary originality and ingenuity in its efforts to improve the system of bee-keeping in vogue among the Russian peasants. Antiquated and unremunerative methods of hive management are still in general use in Russia; and, in order to diffuse a knowledge of the more rational methods of modern apiarists, the Society last year organised a travelling bee-keeping exhibition upon a novel and, as it proved, most successful plan. A barge, 70 metres long and 8 metres broad, was procured and fitted up with a museum, a garden with trees and flower-beds, hives of all kinds, old and new, and a number of hives with living bees; there were also dwelling-rooms for the travelling staff. The museum contained examples of bee-keeping appliances and products, together with a set of preparations illustrating the structure and life-history of bees and their natural enemies. The staff in charge of the exhibition consisted of a practical bee-keeper, two entomologists, and ten men-servants for the vessel. The floating exhibition was towed down the river out of Moscow by twenty horses, ten on each bank; and six towns and about twenty villages were visited between the old capital and the town Kaluga. The travelling was done during the night. During the day, from 8 a.m. to 9 p.m., a halt was made at some town or village; the objects in the museum were explained to visitors by the staff, and the methods of working the model hives were demonstrated to the bee-keeping country folk. The exhibition has worked with great success. The great expense which this interesting and instructive exhibition demanded was most willingly defrayed by Herr F. Motschalkin, who is himself an enthusiastic bee-keeper.

A new determination of the lowest temperature at which a hot body becomes visible is published by Sgr. P. Pettinelli, in the *Nuovo Cimento*. He heated a cast-iron cylinder 30 cm. long and 14 cm. broad in a wrought-iron jacket over a Bunsen burner to a temperature of 460° C., as indicated by an air thermometer, and then observed its flat end in a dark room from a point 60 cm. above it. When it had cooled to about 415°, the red heat vanished and gave way to an indefinite hazy glow. This glow completely disappeared at 404°, and repeated observations gave an error of only 3°. Highly emissive substances, such as the "mantles" made by Auer and others for incandescent gas lighting, became visible at the same temperature; but reflecting surfaces had to be heated 20° higher before they appeared to the eye, and glass still more. These low temperature rays were found to traverse glass and water like ordinary light rays, but they suffer a comparatively greater absorption. Different eyes differ slightly in their capacity of seeing them, the maximum divergence being about 6°. But then the extent of surface must be the same. Sgr. Pettinelli found that if he screened off all but 1/40th of the surface, the body had to be heated 6° higher than before to become visible; if 1/200th, 20° higher: and if 1/800th only was exposed to view, the minimum temperature of visibility was 460°. Hence he rightly concludes that the contradictory results obtained by previous experimenters are due to differences in the areas of the hot bodies investigated.

The Irish elk (*Megaceros hibernicus*) has hitherto had a somewhat isolated position as the only species of its genus known to naturalists up to the present. A new claimant to the same generic title has, however, been recently unearthed in Germany,

and has been described by Prof. Nehring, of Berlin, under the name *M. Ruffii*. The new species is intermediate in many of its characters between the Irish elk and the fallow deer (*Dama vulgaris*). It appears to have lived during the first interglacial epoch, while the Irish elk flourished at a somewhat later geological period. It may possibly, therefore, be regarded as the ancestor of the latter type. The antlers of *M. Ruffii* have fewer "points" or processes than those of *M. hibernicus*; and, although the skull of the animal was as large as or even larger than that of *M. hibernicus*, the antlers were markedly smaller and diverged from one another much less widely than in the case of the latter species. A restoration of the animal accompanies Prof. Nehring's description in *Wild und Hund* for July 10, 1895. From this picture the differences between this new species and *M. hibernicus* may be at once detected.

SOME important experiments of great practical interest have just been published by Dr. Breslauer on the antiseptic properties possessed by disinfectants mixed with different fats in the shape of ointments. As long as fourteen years ago Koch pointed out that carbolic acid combined with olive oil or "carbolised oil," contrary to the prevailing impression, possessed no antiseptic properties. Dr. Breslauer has extended these experiments to an exhaustive examination of various disinfectants, such as carbolic acid, corrosive sublimate, boric acid, nitrate of silver, &c., in combination with oil, vaseline, fat, lanolin anhydricum, lanolin, and unguentum leniens. It was found that the degree of antiseptic power possessed by the disinfectant depended, in a very remarkable manner, upon the particular diluent employed, and that in all cases the best antiseptic results were obtained with disinfectants in combination with lanolin or unguentum leniens. Thus in a series of experiments on the antiseptic effect produced by adding five per cent. of carbolic acid to various substances, it was ascertained that the *Staphylococcus pyogenes aureus* was still living after being immersed in carbolised oil for three days, in carbolised vaselin it survived one day, in fat four hours, in lanolin anhydricum two hours, in lanolin thirty minutes, and in unguentum leniens twenty minutes. Similar results were obtained not only with other bacteria, but also with different disinfectants. Dr. Breslauer has also examined the bactericidal properties of other ointments in frequent use, such as unguentum zinci, unguentum cinereum (benzoatum), and unguentum præcipitatum album, and whilst the two latter were found to be possessed of highly antiseptic properties, the former exercised no perceptible effect whatever. In employing ointments it would appear, therefore, advisable to use the disinfectant selected in combination with lanolin or unguentum leniens instead of supplying vaseline, oil, or other fats, the addition of the latter, according to Dr. Breslauer, serving only to reduce the antiseptic action of the disinfectant. This subject is curiously one which has had, so far, hardly any attention bestowed upon it, and with the exception of some experiments by Gottstein, published in 1889, and, still more recently, an inquiry by Ludwig Bach into the antiseptic effect of various eye ointments, Dr. Breslauer's communication seems to be the only one which has appeared.

WE have recently received two new parts of the *Indian Museum Notes*, from the Trustees of the Museum, being vol. iii. [illegible] 4 [illegible] 5. Part 4 is devoted to an account of the insects and [illegible] the tea-plant in India, and includes full de[illegible], good figures of the principal insects, [illegible] occasionally of their parasites also. The [illegible] the more important plant-feeding orders; but what [illegible] remarkable is the very large number of [illegible] injurious to the tea plant, as compared [illegible]. Thus, only three beetles are mentioned, [illegible], and *Curculionidæ* respectively: as against nineteen *Lepidoptera Heterocera* of various families. The pamphlet concludes with a practical appendix on insecticides. It must not, however, be supposed that a treatise of seventy pages can possibly exhaust the subject of the enemies of any particular plant, especially when they are discussed in detail. A glance at the most important European book on entomological botany (Kaltenbach's "Pflanzenfeinde") is sufficient to show us that many plants are attacked by hundreds of different species of insects; and if this is the case in Europe, it cannot but be true to a still greater extent in tropical countries. But fortunately insects are not always uniformly abundant. They are affected by variations of the season: parasites, and many other influences which are more or less obvious to us; and it is only occasionally that one or other of the numerous species which feed upon any given plant becomes sufficiently abundant to cause any serious injury to it. The other number of the *Indian Museum Notes* before us (part 5) is more varied in its contents. It contains an account of the progress of entomology in the Indian Museum, from 1884–1894, by Mr. E. C. Cotes; some short papers by different entomologists on Indian *Diptera* and *Rhynchota*, and a series of miscellaneous notes on insects of all orders, by Mr. Cotes. This part is not only illustrated, like the other, by numerous woodcuts, but also contains three well-executed plain plates.

THREE important papers by Prof. E. D. Cope, and two by Prof. W. B. Scott, make up, with seven plates, the part recently distributed (vol. ix. part 4) of the *Journal* of the Academy of Natural Sciences of Philadelphia. Prof. Cope treats of new and little known Palæozoic and Mesozoic fishes, and describes Cyphornis—an extinct genus of birds. The genus is established on a species of bird represented by the superior part of a tarsometatarse, obtained by Dr. G. M. Dawson from a bed of indurated greenish clay of unknown age from Vancouver Island. The bird appears to possess real affinities with the Steganopodes, combined with affinities to more primitive birds with a simple hypotarsal structure. "The presumed affinity with the Steganopodes," remarks Prof. Cope, "indicates natatory habits, and probable capacity for flight. Should this power have been developed in *Cyphornis magnus*, it will have been much the largest bird of flight thus far known." Another paper by Prof. Cope is on extinct *Bovidæ*, *Canidæ* and *Felidæ*, from the Pleistocene of Southern Kansas and Western Central Oklahoma. Prof. W. B. Scott's memoir on the structure and relationships of *Ancodus* supplements the extensive investigations of Kowalevsky and Filhol by giving an account of the American species of that genus, and by showing the points of resemblance and differences between the approximately contemporaneous species of *Ancodus* in America and Europe. Prof. Scott concludes his valuable paper as follows:—"With the facts at present known, all seem to point to the origin of *Ancodus* in the Old World and its migration to America, in the interval between the Eocene and the Oligocene (Uinta and White River), yet until the American artiodactyls from the middle and upper Eocene are far better known than at present, such a conclusion cannot be regarded as final." The second paper by Prof. Scott deals with the osteology of *Hyænodon*—a genus described by him in 1877, so far as the materials then available would permit. The Princetown expedition of last year resulted in the collection by Mr. Hatchet of several more or less complete skeletons representing a number of species. These specimens of *Hyænodon* enabled Prof. Scott to supplement the earlier account with the present paper, in which is given a restoration of the skeleton of the very curious and remarkable animal with which it deals.

MESSRS. ROWLAND WARD AND CO., of Piccadilly, are sending out invitations to naturalists to inspect a mounted example of the White Rhinoceros (*Rhinoceros simus*) from Zululand. The

two specimens brought home about two years ago were from Northern Mashonaland. Thus this animal, until lately supposed to be quite extinct, has now been found in a second locality. But these are now the only two spots on the face of the earth where this huge creature, formerly abundant in the Cape Colony, still exists, in very dwindling numbers, which will, no doubt, be now rapidly diminished.

A COMMITTEE of six gentlemen has been appointed by the Governor-General of Goa, India, to carry on excavations in the ancient city of Goa, in rearch of relics of the traditional grandeur of the past, and to take the necessary steps for the preservation of the monuments of Portuguese rule in India in the earlier time.

AN electrical forge, where the whole of the heating required is done by electricity, is in operation at Niagara Falls, the power being supplied by the great cataract. The cost of making a horse-shoe at the electric forge is, it is stated, much less than at an ordinary coal forge. We hear, too, that corn is being threshed by electricity, with very satisfactory results, at Mjolby in Sweden.

WE have received from Mr. W. Radcliffe, of Andreas School, Isle of Man, the inventor of the "Gonagraph," an instrument for drawing perfectly accurate equilateral triangles, squares, pentagons, hexagons, heptagons and octagons, an arithmetical puzzle. The puzzle consists of nineteen small cubes, having a face on each numbered with one of the first nineteen numbers, which are to be placed upon squares, symmetrically arranged on a board, five on the middle row, and two rows of four and three squares to right and left of this. The numbers are to be so arranged that their sum along each of twelve straight lines shall make up thirty-eight. This sum is also obtainable from other symmetrical arangements. It will thus be seen that the puzzle is of the nature of a magic square, and is a very ingenious one. The author has favoured us with his solution, which naturally is at present kept back. He has not furnished us with a clue to his arrangement, and we have in vain searched for it; nor does he say whether he has attempted any extension of the puzzle to thirty-seven or a higher number of cubes. The "thirty-eight" puzzle can be obtained direct from the inventor in a small box for sixpence.

A DESCRIPTION has been sent to us of a new arc lamp for projection purposes, which has been devised by Mr. Cecil M. Hepworth. The instrument has three regulating discs or milled heads of vulcanite, which project at the back, so as to be under the control of the lanternist. The top and bottom discs are for the purpose of regulating the positions of the carbons, and the middle disc has three duties to perform, viz. to bring the carbons slowly together as their points waste in consumption, by a push action to cause the carbons instantaneously to touch, and by a spring to as quickly separate, while by an upward movement the worm-wheel is thrown completely out of gear, and the carbons can be rapidly separated or brought together by hand, a provision necessary for the saving of time when inserting new carbons.

THE September part of the *Proceedings* of the Physical Society of London has reached us, and contains, in addition to the usual valuable supplement of "Abstracts of Physical Papers from Foreign Sources," the following papers:—"A Theory of the Synchronous Motor," by W. G. Rhodes (continuation); "On the Use of an Iodine Voltameter for the Measurement of Small Currents," by Prof. E. F. Herroun, "On the Condensation and the Critical Phenomena of Mixtures of Ethane and Nitrous Oxide," by Dr. Kuenen; "An Electro-Magnetic Effect," by F. W. Bowden; and "The Electrical Properties of Selenium," by Shelford Bidwell, F.R.S.

THE September-October part of the *Physical Review* (Macmillan) contains the following articles: "A Study of the Polarisation of the Light emitted by Incandescent Solid and Liquid Surfaces," by R. A. Millikan, "Alternating Currents when the Electromotive Force is of a Zigzag Wave Type," by E. C. Rimmington, "On Ternary Mixtures," by W. D. Bancroft, part 2; and minor contributions.

BOURNE's Handy Assurance Manual for 1895, by William Schooling, has been published. It contains in a small compass a whole host of information likely to be of use to those who are interested in insurance matters, and appears to have been compiled with great care.

WE have received from Messrs. G. W. Wilson and Co., Limited, 2 St. Swithin Street, Aberdeen, copies of their catalogues of lantern slides. The list of subjects illustrated is a very full one, and the catalogues may be had upon application.

ON the completion of the fiftieth year of its existence, the editor of the *Botanische Zeitung* publishes a very useful index of the papers contained in the first fifty volumes.

THE September number of the *Irish Naturalist* has just appeared, and is entirely devoted to reports of the Galway conference and excursion of the Irish Field Club Union, held in July.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*, ♀) from India, presented by Miss Larkin; a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. W. Aldridge; a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by Mrs. Griffith; a —— Monkey (*Cercopithecus*, sp. inc.) from Africa, presented by Miss Pigott; two Vulpine Phalangers (*Phalangista vulpina*, ♂ ♀) from Australia, presented by Mr. F. J. Horniman; a Magpie (*Pica caudata*), British, presented by Mr. H. E. Blandford; an Orange-cheeked Amazon (*Chrysotis autumnalis*) from Central America, presented by the Rev. W. J. Loftie; a Martinique (*Ionornis martinicus*), captured off the Island of Ascension, presented by Mr. H. W. Power; a Smooth Snake (*Coronella lævis*), a Common Viper (*Vipera berus*), British, presented by Mr. G. J. S. Warner; a Brown Capuchin (*Cebus fatuellus*) from Guiana, three Grant's Francolines (*Francolinus granti*) from East Africa, two Egyptian Trionyx (*Trionyx niloticus*) from the Congo, deposited; a Two-toed Sloth (*Cholopus didactylus*) from Brazil, a Yellow-naped Amazon (*Chrysotis auripalliata*) from Central America, purchased.

OUR ASTRONOMICAL COLUMN.

THE ORBIT OF μ^2 BOOTIS (Σ 1938).—Dr. T. J. J. See gives in the *Astr. Nach.*, No. 3309, Bd. 138, the results of his researches on this star. This double was discovered by Sir William Herschel in 1781, and since the time of Struve it has been very abundantly observed. In all parts of the orbit the pair is sufficiently wide to be seen with a 6-inch telescope. The investigation gives the following elements of μ^2 Bootis; other elements are given for comparison.

	P	T	e	a	Ω	i	λ	Authority
				″				
1	146·649	1[illegible]51·57	0·529	1·32[illegible]	[illegible]4·7	49·4	87·1	Mädler 18[illegible]
2	122·6	66·0	0·401	1·105	166·1	47·5	23·[illegible]	Winnagradsky 18[illegible]
3	314·34	60·88	0·5641	1·761	163·2	41·[illegible]	54·4	Hind 18[illegible]
4	20[illegible]·4	65·2	0·[illegible]1	—	172·0	4[illegible]	2[illegible]·1	Wilson 18[illegible]
5	198·93	65·5	0·4057	—	169·[illegible]	46·4	2[illegible]·6	Klinkerfues
6	290·07	63·51	0·174	1·5	185·0	44·4	17·7	Doberck 1875
7	289·29	60·51	0·5974	1·47	17[illegible]·7	[illegible]	2[illegible]	Doberck 18[illegible]
[illegible]	206·[illegible]	62·55	0·509	1·[illegible]	166·7	55·2	40·9	Pritchard 1873
9	219·42	65·3	0·527	1·26	163·[illegible]	43·9	329·75	See 1895

The apparent orbit is:

$$\text{Major axis} = 2''.656$$
$$\text{Minor axis} = 1''.480$$
$$\text{Angle of major axis} = 173^\circ.5$$
$$\text{,, \ ,, periastron} = 186^\circ.7$$
$$\text{Distance of star from centre} = 0''.038$$

The computed and observed places seem to justify the new elements given above. The period thus will hardly be varied by as much as ten years, while the resulting alteration will be small in proportion.

THE BRITISH ASSOCIATION.

SECTION K.

BOTANY.

OPENING ADDRESS BY W. T. THISELTON-DYER, M.A., F.R.S., C.M.G., C.I.E., DIRECTOR OF THE ROYAL GARDENS.

THE establishment of a new Section of the British Association, devoted to Botany, cannot but be regarded by the botanists of this country as an event of the greatest importance. For it is practically the first time that they have possessed an independent organisation of their own. It is true that for some years past we have generally been strong enough to form a separate department of the old Biological Section D, on the platform of which so many of us in the past have acted in some capacity or other, and on which indeed many of us may be said to have made our first appearance. We shall not start then on our new career without the remembrance of filial affection for our parent, and the earnest hope that our work may be worthy of its great traditions.

The first meeting of the Section, or, as it was then called, Committee, at Oxford was held in 1832. And though there has been from time to time some difference in the grouping of the several biological sciences, the two great branches of biology have only now for the first time formally severed the partnership into which they entered on that occasion. That this severance, if inevitable from force of circumstances, is in some respects a matter of regret, I do not deny. Specialisation is inseparable from scientific progress; but it will defeat its own end in biology if the specialist does not constantly keep in touch with those fundamental principles which are common to all organic nature. We shall have to take care that we do not drift into a position of isolation. Section D undoubtedly afforded a convenient opportunity for discussing many questions on which it was of great advantage that workers in the two different fields should compare their results and views. But I hope that by means of occasional conferences we shall still, in some measure, be able to preserve this advantage.

RETROSPECT.

I confess I found it a great temptation to review, however imperfectly, the history and fortunes of our subject while it belonged to Section D. But to have done so would have been practically to have written the history of botany in this country since the first third of the century. Yet I cannot pass over some few striking events.

I think that the earliest of these must undoubtedly be regarded as the most epoch-making. I mean the formal publication by the Linnean Society, in 1833, of the first description of "the nucleus of the cell," by Robert Brown ("Misc. Bot. Works," i. 512). It seems difficult to realise that this may be within the recollection of some who are now living amongst us. It is, however, of peculiar interest to me that the first person who correctly distinguished this all-important body, and indicated it in a figure, was Francis Bauer, thirty years earlier, in 1802. This remarkable man, whose skill in applying the resources of art to the illustration of plant anatomy has never, I suppose been surpassed, was "resident draughtsman for fifty years to the Royal Botanic Garden at Kew." And it was at Kew, and in a flower of *Phajus grandifolius*, no doubt grown there, that the discovery was made.

It must be confessed, with no little admiration that, on refreshing my memory by a reference to Robert Brown's paper, I read again the vivid account which he gives in a footnote of the phenomena, so painfully familiar to many of us who have been taught, of circulation in the staminal hair of *Tradescantia*. Sir Joseph Hooker (*Proc. Linn. Soc.*, 1887-88, 65) has well remarked that "the supreme importance of this observation. . . . leading to undreamt-of conceptions of the fundamental phenomena of organic life, is acknowledged by all investigators." It is singular that so profound an observer as Robert Brown should have himself missed the significance of what he saw. The world had to wait for the discovery of protoplasm by Von Mohl till 1846, and till 1850 for its identification with the sarcode of zoologists by Cohn, who is still, I am happy to say, living and at work, and to whom last year the Linnean Society did itself the honour of presenting its medal.

The Edinburgh meeting of the Association, in 1834, was the occasion of the announcement of another memorable discovery of Robert Brown's. I will content myself with quoting Hofmeister's ("Higher Cryptogamia," 432) account of it. "Robert Brown was the discoverer of the polyembryony of the *Coniferæ*. In a later treatise he pointed out the origin of the pro-embryo in large cells of the endosperm, to which he gave the name of corpuscula." The period of the forties, just half a century ago, looks in the retrospect as one of almost dazzling discovery. To say nothing of the formal appearance of protoplasm on the scene, the foundations were being laid in all directions of our modern botanical morphology. Yet its contemporaries viewed it with a very philosophical calm. Thwaites, who regarded Carpenter as his master, described at the Oxford meeting in 1847 the conjugation of the *Diatomaceæ*, and "distinctly indicated," as Carpenter ("Memorial Sketch," 140) says, "that conjugation is the primitive phase of sexual reproduction." Berkeley informed me that the announcement fell perfectly flat. A year or two later Suminski came to London with his splendid discovery (1848) of the archegonia of the fern, the antheridia having been first seen by Nägeli in 1844. Carpenter (*loc. cit.*, 141) gave me, many years after, a curious account of its reception. "At the Council of the Ray Society, at which," he said, "I advocated the reproduction of Suminski's book on the 'Ferns,' I was assured that the close resemblance of the antherozoids to spermatozoa was quite sufficient proof that they could have nothing to do with vegetable reproduction. I do not think," he added—and the complaint is pathetic—"that the men of the present generation, who have been brought up in the *light*, quite apprehend (in this as in other matters) the utter darkness in which we were then groping, or fully recognise the deserts of those who helped them to what they now enjoy." This was in 1875, and I suppose is not likely to be less true now.

The Oxford meeting in 1860 was the scene of the memorable debate on the origin of species, at which it is interesting to remember that Henslow presided. On that occasion Section D reached its meridian. The battle was Homeric. However little to the taste of its author, the launching of his great theory was, at any rate, dignified with a not inconsiderable explosion. It may be that it is not given to the men of our day to ruffle the dull level of public placidity with disturbing and far-reaching ideas. But if it were, I doubt whether we have, or need now, the fierce energy which inspired then either the attack or the defence. When we met again in Oxford last year the champion of the old conflict stood in the place of honour, acclaimed of all men, a beautiful and venerable figure. We did not know then that that was to be his farewell.

The battle was not in vain. Six years afterwards, at Nottingham, Sir Joseph Hooker delivered his classical lecture on Insular Floras. It implicitly accepted the new doctrine, and applied it with admirable effect to a field which had long waited for an illuminating principle The lecture itself has since remained one of the corner-stones of that rational theory of the geographical distribution of plants which may, I think, be claimed fairly as of purely English origin.

HENSLOW.

Addressing you as I do at Ipswich, there is one name written in the annals of our old Section which I cannot pass over—that of Henslow. He was the Secretary of the Biological Section at its first meeting in 1832, and its President at Bristol in 1836. I suppose there are few men of this century who have indirectly more influenced the current of human thought. For in great measure I think it will not be contested that we owe Darwin to him. As Romanes has told us ("Memorial Notices," 13): "His letters written to Prof. Henslow during his voyage round the world overflow with feelings of affection, veneration, and obligation to his accomplished master and dearest friend—feelings which throughout his life he retained with no diminished intensity. As he used himself to say, before he knew Prof.

Henslow the only objects he cared for were foxes and partridges." I do not wish to overstate the facts. The possession of "the collector's instinct, strong in Darwin from his childhood, as is usually the case in great naturalists," to use Huxley's (*Proc. R.S.*, xliv. vi.) words, would have borne its usual fruit in after life, in some shape or other, even if Darwin had not fallen into Henslow's hands. But then the particular train of events which culminated in the great work of his life would never have been started. It appeared to me, then, that it would not be an altogether uninteresting investigation to ascertain something about Henslow himself. The result has been to provide me with several texts, which I think it may be not unprofitable to dwell upon on the present occasion.

In the first place, what was the secret of his influence over Darwin? "My dear old master in Natural History" ("Life," ii. 317) he calls him; and to have stood in this relation to Darwin[1] is no small matter. Again, he speaks of his friendship with him as "a circumstance which influenced my whole career more than any other" (i. 52). The singular beauty of Henslow's character, to which Darwin himself bore noble testimony, would count for something, but it would not in itself be a sufficient explanation. Nor was it that intellectual fascination which often binds pupils to the master's feet; for, as Darwin tells us, "I do not suppose that any one would say that he possessed much original genius" (i. 52). The real attraction seems to me to be found in Henslow's possession, in an extraordinary degree, of what may be called the Natural History spirit. This resolves itself into keen observation and a lively interest in the facts observed. "His strongest taste was to draw conclusions from long-continued minute observations" (i. 52). The old Natural History method, of which it seems to me that Henslow was so striking an embodiment, is now, and I think unhappily, almost a thing of the past. The modern university student of botany puts his elders to blush by his minute knowledge of some small point in vegetable histology. But he can tell you little of the contents of a country hedgerow; and if you put an unfamiliar plant in his hands he is pretty much at a loss how to set about recognising its affinities. Disdaining the field of nature spread at his feet in his own country, he either seeks salvation in a German laboratory or hurries off to the Tropics, convinced that he will at once immortalise himself. But *cælum non animum mutat*; he puts into "pickle" the same objects as his predecessors, never to be looked at again; or perhaps writes a paper on some obvious phenomena which he could have studied with less fatigue in the Palm House at Kew.

The secret of the right use of travel is the possession of the Natural History instinct, and to those who contemplate it I can only recommend a careful study of Darwin's "Naturalist's Voyage." Nothing that came in his way seems to have evaded him or to have seemed too inconsiderable for attention. No doubt some respectable travellers have lost themselves in a maze of observations that have led to nothing. But the example of Darwin, and I might add of Wallace, of Huxley, and of Moseley, show that that result is the fault of the man and not of the method. The right moment comes when the fruitful opportunity arrives to him who can seize it. The first strain of the prelude with which the "Origin" commences are these words: "When on board H.M.S. *Beagle* as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America." But this sort of vein is not struck at hazard or by him who has not served a tolerably long apprenticeship to the work.

When one reads and re-reads the "Voyage," it is simply amazing to see how much could be achieved with a previous training which we now should think ludicrously inadequate. Before Henslow's time the state of the natural sciences at Cambridge was incredible. In fact, Leonard Jenyns ("Memoir," 175), his biographer, speaks of the "utter disregard paid to Natural History in the University previous to his taking up his residence there." The Professor of Botany had delivered no lectures for thirty years, and though Sir James Smith, the founder of the Linnean Society, had offered his services, they were declined on the ground of his being a Nonconformist (*ibid.*, 37).

As to Henslow's own scientific work, I can but rely on the judgment of whose who could appreciate it in relation to its time. According to Berkeley (*ibid.*, 56), "he was certainly one of the first, if not the very first, to see that two forms of fruit might exist in the same fungus." And this, as we now know, was a fundamental advance in this branch of morphology. Sir Joseph Hooker tells me that his papers were all distinctly in advance of his day. Before occupying the chair of botany, he held for some years that of mineralogy. Probably he owed this to his paper on the Isle of Anglesey, published when he was only twenty-six. I learn from the same authority, that this to some extent anticipated, but at any rate strongly influenced, Sedgwick's subsequent work in the same region.

Botanical Teaching.

Henslow's method of teaching deserves study. Darwin says of his lectures "that he liked them much for their extreme clearness." "But," he adds, "I did not study botany" (i. 48). Yet we must not take this too seriously. Darwin ("Voyage," 421), when at the Galapagos, "indiscriminately collected every thing in flower on the different islands, and fortunately kept my collections separate." Fortunately indeed; for it was the results extracted from these collections, when worked up subsequently by Sir Joseph Hooker, which determined the main work of his life. "It was such cases as that of the Galapagos Archipelago which chiefly led me to study the origin of species" (iii. 159).

Henslow's actual method of teaching went some way to anticipate the practical methods of which we are all so proud. "He was the first to introduce into the botanical examination for degrees in London the system of practical examination" ("Memoir," 161). But there was a direct simplicity about his class arrangements characteristic of the man. "A large number of specimens . . . were placed in baskets on a side-table in the lecture-room, with a number of wooden plates and other requisites for dissecting them after a rough fashion, each student providing himself with what he wanted before taking his seat" (*ibid.*, 39). I do not doubt that the results were, in their way, as efficient as we obtain now in more stately laboratories.

The most interesting feature about his teaching was not, however, its academic aspect, but the use he made of botany as a general educational instrument. "He always held that a man of *no* powers of observation was quite an exception" (*ibid.*, 163). He thought (and I think he proved) that botany might be used "for strengthening the observant faculties and expanding the reasoning powers of children in all classes of society" (*ibid.*, 99). The difficulty with which those who undertake now to teach our subject have to deal is that most people ask the question, What is the use of learning botany unless one means to be a botanist? It might indeed be replied that as the vast majority of people never learn anything effectively, they might as well try botany as anything else. But Henslow looked only to the mental discipline; and it was characteristic of the man and of his belief in his methods that when he was summoned to Court to lecture to the Royal family, his lectures "were, in all respects, identical with those he was in the habit of giving to his little Hitcham scholars" ("Memoir," 149); and it must be added that they were not less successful.

This success naturally attracted attention. Botanical teaching in schools was taken up by the Government, and continues to receive support to the present day. But the primitive spirit has, I am afraid, evaporated. The measurement of results by means of examination has been fatal to its survival. The teacher has to keep steadily before his eyes the necessity of earning his grant. The educational problem retires into the background. "The strengthening of the observant faculties," and the rest of the Henslowian programme must give way to the imperious necessity of presenting to the examiner candidates equipped with at least the minimum of text-book formulas reproducible on paper. I do not speak in this matter without painful experience. The most astute examiner is defeated by the still more astute crammer. The objective basis of the study on which its whole usefulness is built up is promptly thrown aside. If you supply the apple blossom for actual description, you are as likely as not to be furnished with a detailed account of a buttercup. The training of observation has gone by the board, and the exercise of mere memory has taken its place. But a table of logarithms or a Hebrew grammar would serve this purpose equally well. Yet I do not despair of Henslow's work still bearing fruit. The examination system will collapse from the sheer impossibility of carrying it on beyond a certain point. Freed from its trammels, the teacher will have greater scope for individuality, and the result of his labours will be rewarded after some intelligent system of inspection. And here I may claim support from an unexpected quarter. Mr. Gladstone has recently written to a

[1] As I shall have frequent occasion to quote the "Life and Letters" I shall insert the references in the text.

[illegible]: "I think that the neglect of natural history, in all its multitude of branches, was the grossest defect of our old system of training for the young: and, further, that little or nothing has been done by way of remedy for that defect in the attempts made to alter or reform that system." I am sure that the importance and weight of this testimony, coming as it does from one whose training and sympathies have always been literary, cannot be denied. That there is already some revival of Henslow's methods, I judge from the fact that I have received applications from Board Schools, amounting to some hundreds, for surplus specimens from the Kew Museums. Without a special machinery for the purpose I cannot do much, and perhaps it is well. But my staff have willingly done what was possible, and from the letters I have received I gather that the labour has not been wholly misspent.

MUSEUM ARRANGEMENT.

This leads me to the last branch of Henslow's scientific work which I am able to touch, that of the arrangement of museums, especially those which being local have little meaning unless their purpose is strictly educational. I think it is now generally admitted that, both in the larger and narrower aspects of the question, his ideas, which were shared in some measure by Edward Forbes, were not merely far in advance of his time, but were essentially sound. And here I cannot help remarking that the zoologists have perhaps profited more by his teaching than the botanists. I do not know how far Sir William Flower and Prof. Lankester would admit the influence of Henslow's ideas. But, so far as my knowledge goes, I am not aware that, at any rate in Europe, there is anything to be seen in public museums comparable to the educational work accomplished by the one at the College of Surgeons and the Natural History Museum, and by the other at Oxford.

I have often thought it singular that in botany we have not kept pace in this matter with our brother naturalists. I do not doubt that vegetable morphology and a vast number of important facts in evolution, as illustrated from the vegetable kingdom, might be presented to the eye in a fascinating way in a carefully arranged museum. The most successful and, indeed, almost the only attempt which has been made in this direction is that at Cambridge, which, I believe, is due to Mr. Gardiner. But our technical methods for preserving specimens still leave much to desire. Something more satisfactory will, it may be hoped, some day be devised, and the whole subject is one which is well worth the careful consideration of our Section. Henslow at least effected a vast improvement in the mode of displaying natural objects; and a collection prepared by his own hands, which was exhibited at one of the Paris exhibitions, excited the warm admiration of the French botanists, who always appreciate the clear illustration of morphological facts.

OLD SCHOOL OF NATURAL HISTORY.

If the old school of natural history of which Henslow in his day was a living spirit is at present, as seems to be the case, continually losing its hold upon us, this has certainly not been due to its want of value as an educational discipline, or to its sterility in contributing new ideas to human knowledge. Darwin's "Origin of Species" may certainly be regarded as its offspring, and of this Huxley (*Proc. R.S.*, xliv. xvii.) says with justice: "It is doubtful if any single book except the 'Principia,' ever worked so great and rapid a revolution in science, or made so deep an impression on the general mind." Yet Darwin's biographer, in that admirable "Life" which ranks with the few really great biographies in our language, remarks (i. 155): "In reading [illegible] books one is reminded of the older naturalists rather than of the modern school of writers. He was a naturalist in the old sense of the word, that is, a man who works at many [illegible], not merely a specialist in one." This is no [illegible] but [illegible] not exactly hit off the distinction between the [illegible] study which has gone out of fashion and that which [illegible]. The older workers in biology were occupied [illegible] with the external or, at any rate, grosser features of [illegible] relation to surrounding conditions; the [illegible] on the other hand, are engaged on the study of internal [illegible]. Work in the laboratory, with its neces[illegible] the place of research in the field. One [illegible] that the use of the compound microscope [illegible]. Asa Gray has compared Robert Brown with Darwin [illegible] British naturalists who have "more [illegible] their influence upon science in the nineteenth century" (NATURE, x. 80). Now it is noteworthy that Robert Brown did all his work with a simple microscope. And Francis Darwin writes of his father: "It strikes us nowadays as extraordinary that he should have had no compound microscope when he went his *Beagle* voyage; but in this he followed the advice of Robert Brown, who was an authority on such matters" (i. 145). One often meets with persons, and sometimes of no small eminence, who speak as if there were some necessary antagonism between the old and the new studies. Thus I have heard a distinguished systematist describe the microscope as a curse, and a no less distinguished morphologist speak of a herbarium having its proper place on a bonfire. To me I confess this anathematisation of the instruments of research proper to any branch of our subject is not easily intelligible. Yet in the case of Darwin himself it is certain that if his earlier work may be said to rest solely on the older methods, his later researches take their place with the work of the new school. At our last meeting Pfeffer vindicated one of his latest and most important observations.

The case of Robert Brown is even more striking. He is equally great whether we class him with the older or the modern school. In fact, so far as botany in this country is concerned, he may be regarded as the founder of the latter. It is to him that we owe the establishment of the structure of the ovule and its development into the seed. Even more important were the discoveries to which I have already referred, which ultimately led to the establishment of the group of Gymnosperms. "No more important discovery," says Sachs ("History," 142), "was ever made in the domain of comparative morphology and systematic botany. The first steps towards this result, which was clearly brought out by Hofmeister twenty-five years later, were secured by Robert Brown's researches, and he was incidentally led to these researches by some difficulties in the construction of the seed of an Australian genus." Yet it may be remembered that he began his career as naturalist to Flinders's expedition for the exploration of Australia. He returned to England with 4000 "for the most part new species of plants." And these have formed the foundation of our knowledge of the flora of that continent. Brown's chief work was done between 1820 and 1840, and, as Sachs (*loc. cit.*, 139, 140) tells us, "was better appreciated during that time in Germany than in any other country."

MODERN SCHOOL.

The real founder of the modern teaching in this country in both branches of biology I cannot doubt was Carpenter. The first edition of his admirable "Principles of Comparative Physiology" was published in 1838, the last in 1854. All who owe, as I do, a deep debt of gratitude to that book will agree with Huxley ("Memorial Sketch," 67) in regarding it as "by far the best general survey of the whole field of life and of the broad principles of biology which had been produced up to the time of its publication. Indeed," he adds, "although the fourth edition is now in many respects out of date, I do not know its equal for breadth of view, sobriety of speculation, and accuracy of detail."

The charm of a wide and philosophic survey of the different forms under which life presents itself could not but attract the attention of teachers. Rolleston elaborated a course of instruction in zoology at Oxford in which the structures described in the lecture-room were subsequently worked out in the laboratory. In 1872 Huxley organised the memorable course in elementary biology at South Kensington which has since, in its essential features, been adopted throughout the country. In the following year, during Huxley's absence abroad through ill-health, I arranged, at his request, a course of instruction on the same lines for the Vegetable Kingdom.

That the development of the new teaching was inevitable can hardly be doubted, and I for my part am not disposed to regret the share I took in it. But it was not obvious, and certainly it was not expected, that it would to so large an extent cut the ground from under the feet of the old Natural History studies. The consequences are rather serious, and I think it is worth while pointing them out.

In a vast empire like our own there is a good deal of work to be done and a good many posts to be filled, for which the old Natural History training was not merely a useful but even a necessary preparation. But at the present time the universities almost entirely fail to supply men suited to the work. They neither care to collect, nor have they the skilled aptitude for

observation. Then, though this country is possessed at home of incomparable stores of accumulated material, the class of competent amateurs who were mostly trained at our universities, and who did such good service in working that material out, is fast disappearing. It may not be easy indeed in the future to fill important posts even in this country with men possessing the necessary qualifications. But there was still another source of naturalists, even more useful, which has practically dried up. It is an interesting fact that the large majority of men of the last generation who have won distinction in this field have begun their career with the study of medicine. That the kind of training that Natural History studies give is of advantage to students of medicine which, rightly regarded, is itself a Natural History study, can hardly be denied. But the exigencies of the medical curriculum have crowded them out; and this, I am afraid, must be accepted as irremediable. I cannot refrain from reading you, on this point, an extract from a letter which I have received from a distinguished official lately entrusted with an important foreign mission. I should add that he had himself been trained in the old way.

"I have had my time, and must leave to younger men the delight of working these interesting fields. Such chances never will occur again, for roads are now being made and ways cut in the jungle and forest, and you have at hand all sorts of trees level on the ground ready for study. These bring down with them orchids, ferns, and climbers of many kinds, including rattan palms, &c. But, excellent as are the officers who devote their energy to thus opening up this country, there is not one man who knows a palm from a dragon-tree, so the chance is lost. Strange to say, the medical men of the Government service know less and care less for Natural History than the military men, who at least regret they have no training or study to enable them to take an intelligent interest in what they see around them. A doctor nowadays cares for no living thing larger or more complicated than a *bacterium* or a *bacillus*."

But there are other and even more serious grounds why the present dominance of one aspect of our subject is a matter for regret. In the concluding chapter of the "Origin," Darwin wrote: "I look with confidence to the future—to young and rising naturalists." But I observe that most of the new writers on the Darwinian theory, and, oddly enough, especially when they have been trained at Cambridge, generally begin by more or less rejecting it as a theory of the origin of species, and then proceed unhesitatingly to reconstruct it. The attempt rarely seems to me successful, perhaps because the limits of the laboratory are unfavourable to the accumulation of the class of observations which are suitable for the purpose. The laboratory, in fact, has not contributed much to the Darwinian theory, except the "Law of Recapitulation," and that, I am told, is going out of fashion.

The Darwinian theory, being, as I have attempted to show, the outcome of the Natural History method, rested at every point on a copious basis of fact and observation. This more modern speculation lacks. The result is a revival of transcendentalism. Of this we have had a copious crop in this country, but it is quite put in the shade by that with which we have been supplied from America. Perhaps the most remarkable feature is the persistent vitality of Lamarckism. As Darwin remarks: "Lamarck's one suggestion as to the cause of the gradual modification of species—effort excited by change of conditions—was, on the face of it, inapplicable to the whole vegetable world" (ii. 189). And if we fall back on the inherited direct effect of change of conditions, though Darwin admits that "physical conditions have a more direct effect on plants than on animals" (ii. 319), I have never been able to convince myself that that effect is inherited. I will give one illustration. The difference in habit of even the same species of plant when grown under mountain and lowland conditions is a matter of general observation. It would be difficult to imagine a case of "acquired characters" more likely to be inherited. But this does not seem to be the case. The recent careful research of Gaston Bonnier only confirms the experience of cultivators. The modifications acquired by the plant when transported for a definite time from the plains to the Alps, or *vice versâ*, disappear at the end of the same period when the plant is restored to its original conditions (*Ann. d. S. nat.*, 7e sér. xx. 355).

Darwin, in an eloquent passage, which is too long for me to quote ("Origin," 426), has shown how enormously the interest of Natural History is enhanced "when we regard every production of nature as one which has had a long history," and "when we contemplate every complex structure . . . as the summing up of many contrivances." But this can only be done, or at any rate begun, in the field, and not in the laboratory.

A more serious peril is the dying out amongst us of two branches of botanical study in which we have hitherto occupied a position of no small distinction. Apart from the staffs of our official institutions, there seems to be no one who either takes any interest in, or appreciates in the smallest degree, the importance of systematic and descriptive botany. And geographical distribution is almost in a worse plight, yet Darwin calls it, "that grand subject, that almost keystone of the laws of creation" (i. 356).

I am aware that it is far easier to point out an evil than to remedy it. The teaching of botany at the present day has reached a pitch of excellence and earnestness which it has never reached before. That it is somewhat one-sided cannot probably be remedied without a subdivision of the subject and an increase in the number of teachers. If it has a positive fault, it is that it is sometimes inclined to be too dogmatic and deductive. Like Darwin, at any rate in a biological matter, "I never feel convinced by deduction, even in the case of H. Spencer's writings" (iii. 168). The intellectual indolence of the student inclines him only too gladly to explain phenomena by referring them to "isms," instead of making them tell their own story.

ORGANISATION OF SECTION.

I am afraid I have detained you too long over these matters, on which I must admit I have spoken with some frankness. But I take it that one of the objects of our Section is to deliver our minds of any perilous stuff that is fermenting in it. But now, having taken leave of the past, let us turn to the future.

We start at least with a clean slate. We cannot bind our successors, it is true, at other meetings. But I cannot doubt that it will be in our power to materially shape our future, notwithstanding. When we were only a department I think we all felt the advantage of these annual meetings, of the profitable discussion, formal and informal, and of the privilege of meeting so many of our foreign brethren who have so generously supported us by their presence and sympathy.

I am anxious, then, to suggest that we should conduct our proceedings on as broad lines as possible. I do not think we should be too ready to encourage papers which may well be communicated to societies, either local or central.

The field is large; the labourers as they advance in life can hardly expect to keep pace with all that is going on in it. We must look to individual members of our number to help us by informing and stimulating addresses on subjects they have made peculiarly their own, or on important researches on which they have been specially engaged.

NOMENCLATURE.

There is one subject upon which, from my official position elsewhere, I desire to take the opportunity of saying a few words. It is that of Nomenclature. It is not on its technical side, I am afraid, of sufficient general interest to justify my devoting to it the space which its importance would otherwise deserve. But I hope to be able to enlist your support for the broad common-sense principles on which our practice should rest.

As I suppose, every one knows we owe our present method of nomenclature in natural history to Linnæus. He devised the binominal, or, as it is often absurdly called, the binomial system. That we must have a technical system of nomenclature I suppose no one here will dispute. It is not, however, always admitted by popular writers who have not appreciated the difficulty of the matter, and who think all names should be in the vernacular. There is the obvious difficulty that the vast majority of plants do not possess any names at all, and the attempts to manufacture them in a popular shape have met with but little success. Then, from lack of discriminating power on the part of those who use them, vernacular names are often ambiguous; thus Bullrush is applied equally to *Typha* and to *Scirpus*, plants extremely different. Vernacular names, again, are only of local utility, while the Linnean system is intelligible throughout the world.

A technical name, then, for a plant or animal is a necessity, as without it we cannot fix the object of our investigations into its affinity, structure, or properties ("Linn. Phil.," 210). "Nomina si nescis perit et cognito rerum.

In order to get clear ideas on the matter let us look at the

logical principles on which such names are based. It is fortunate for us that these are stated by Mill, who, besides being an authority on logic, was also an accomplished botanist. He tells us ("System of Logic," i. 132): "A naturalist, for purposes connected with his particular science, sees reason to distribute the animal or vegetable creation into certain groups rather than into any others, and he requires a name to bind, as it were, each of his groups together." He further explains that such names, whether of species, genera, or orders, are what logicians call connotative; they *denote* the members of each group, and *connote* the distinctive characters by which it is defined. A species, then, connotes the common characters of the individuals belonging to it; a genus, those of the species; an order, those of the genera.

But these are the logical principles, which are applicable to names generally. A name such as *Ranunculus repens* does not differ in any particular from a name such as John Smith, except that one denotes a species, the other an individual.

This being the case, and technical names being a necessity, they continually pass into general use in connection with horticulture, commerce, medicine, and the arts. It seems obvious that, if science is to keep in touch with human affairs, stability in nomenclature is a thing not merely to aim at, but to respect. Changes become necessary, but should never be insisted upon without grave and solid reason. In some cases they are inevitable unless the taxonomic side of botany is to remain at a standstill. From time to time the revision of a large group has to be undertaken from a uniform and comparative point of view. It then often occurs that new genera are seen to have been too hastily founded on insufficient grounds, and must therefore be merged in others. This may involve the creation of a large number of new names, the old ones becoming henceforth a burden to literature as synonyms. It is usual in such cases to retain the specific portion of the original name, if possible. If it is, however, already preoccupied in the genus to which the transference is made, a new one must be devised. Many modern systematists have, however, set up the doctrine that a specific epithet once given is indelible, and whatever the taxonomic wanderings of the organism to which it was once assigned, it must always accompany it. This, however, would not have met with much sympathy from Linnæus, who attached no importance to the specific epithet at all: "Nomen specificum sine generico est quasi pistillum sine campana" ("Phil.," 219). Linnæus always had a solid reason for everything he did or said, and it is worth while considering in this case what it was.

Before his time the practice of associating plants in genera had made some progress in the hands of Tourneford and others, but specific names were still cumbrous and practically unusable. Genera were often distinguished by a single word; and it was the great reform accomplished by Linnæus to adopt the binominal principle for species. But there is this difference. Generic names are unique, and must not be applied to more than one distinct group. Specific names might have been constituted on the same basis: the specific name in that case would then have never been used to designate more than one plant, and would have been sufficient to indicate it. We should have lost, it is true, the useful information which we get from our present practice in learning the genus to which the species belongs; but theoretically a nomenclature could have been established on the one name principle. The thing, however, is impossible now even if it were desirable. A specific epithet like *vulgaris* may belong to hundreds of different species belonging to as many different genera, and taken alone is meaningless. A Linnean name, then, though it consists of two parts, must be treated as a whole. "Nomen omne plantarum constabit nomine generico et specifico" ("Phil." 212). A fragment can have no vitality of its own. Consequently, if superseded, it may be replaced by another which may be perfectly independent.[1]

It constantly happens that the same species is named and described by more than one writer, or different views are taken of specific differences by various writers; the species of one are therefore "lumped" by another. In such cases, where there is a choice of names, it is customary to select the earliest published. I agree, however, with the late Sereno Watson (NATURE, xlvi. 54) that "there is nothing whatever of an ethical character inherent in a name, through any priority of publication or position, which should render it morally obligatory upon any one to accept one name rather than another." And in point of fact Linnæus and the early systematists attached little importance to priority. The rigid application of the principle involves the assumption that all persons who describe or attempt to describe plants are equally competent to the task. But this is far from being the case that it is sometimes all but impossible even to guess what could possibly have been meant.[1]

In 1872 Sir Joseph Hooker ("Flora of British India," i. vii.) wrote: "The number of species described by authors who cannot determine their affinities increases annually, and I regard the naturalist who puts a described plant into its proper position in regard to its allies as rendering a greater service to science than its describer when he either puts it into a wrong place or throws it into any of those chaotic heaps, miscalled genera, with which systematic works still abound." This has always seemed to me not merely sound sense, but a scientific way of treating the matter. What we want in nomenclature is the maximum amount of stability and the minimum amount of change compatible with progress in perfecting our taxonomic system. Nomenclature is a means, not an end. There are perhaps 150,000 species of flowering plants in existence. What we want to do is to push on the task of getting them named and described in an intelligible manner, and their affinities determined as correctly as possible. We shall then have material for dealing with the larger problems which the vegetation of our globe will present when treated as a whole. To me the botanists who waste their time over priority are like boys who, when sent on an errand, spend their time in playing by the roadside. By such men even Linnaeus is not to be allowed to decide his own names. To one of the most splendid ornaments of our gardens he gave the name of *Magnolia grandiflora*: this is now to be known as *Magnolia fœtida*. The reformer himself is constrained to admit, "The change is a most unfortunate one in every way" ("Garden and Forest," ii. 615). It is difficult to see what is gained by making it, except to render systematic botany ridiculous. The genus *Aspidium*, known to every fern cultivator, was founded by Swartz. It now contains some 400 species, of which the vast majority were, of course, unknown to him at the time: yet the names of all these are to be changed because Adamson founded a genus, *Dryopteris*, which seems to be the same thing as *Aspidium*. What, it may be asked, is gained by the change? To science it is certainly nothing. On the other hand, we lumber our books with a mass of synonyms, and perplex every one who takes an interest in ferns. It appears that the name of the well-known Australian genus *Banksia* really belongs to *Pimelea*; the species are therefore to be renamed, and *Banksia* is to be rechristened *Sirmuellera*, after Sir Ferdinand von Mueller: a proposal which, I need hardly say, did not emanate from an Englishman.

I will not multiply instances. But the worst of it is that those who have carefully studied the subject know that, from various causes which I cannot afford the time to discuss, when once it is attempted to disturb accepted nomenclature it is almost impossible to reach finality. Many genera only exist by virtue of their redefinition in modern times; in the form in which they were originally promulgated they have hardly any intelligible meaning at all.

It can hardly be doubted that one cause of the want of attention which systematic botany now receives is the repulsive labour of the bibliographical work with which it has been overlaid. What an enormous bulk nomenclature has already attained may be judged from the "Index Kewensis," which was prepared at Kew, and which we owe to the munificence of Mr. Darwin. In his own studies he constantly came on the track of names which he was unable to run down to their source. This the "Index" enables to be done. It is based, in fact, on a manuscript index which we compiled for our own use at Kew. But it is a mistake to suppose that it is anything more than the name signifies, or that it expresses any opinion as to the validity of the names themselves. That those who use the book must judge of for themselves. We have indexed existing names, but we have not added to the burden by making any new ones for species already described.

What synonymy has now come to may be judged by an example supplied me by my friend Mr. C. B. Clarke. For a single species of *Fimbristylis* he finds 135 published names under six

1 M. Alph[illegible] ([illegible]) [illegible] but in a letter published in the *Bull. de* [illegible] *France* ([illegible]) "the real merit of Linnæus has been to combine for all plants the generic name with the specific epithet. It is im[illegible] the 'name' of a species consists, [illegible] in the combination, not in the specific epithet, [illegible] a mere fragment of the name, and meaningless when taken by itself."

1 Darwin, who always seems to me, almost instinctively, to take the right view in matters relating to natural history, is ("Life," vol. i. p. 364) dead against the new "practice of naturalists appending for perpetuity the name of the *first* describer to species. He is equally against the priority craze:—"I cannot yet bring myself to reject very *well-known* names" (*ibid.*, p. 369).

genera. If we go on in this way we shall have to invent a new Linnæus, wipe out the past, and begin all over again.

Although I have brought the matter before the Section it is not one in which this, or indeed any collective assembly of botanists, can do very much. While I hope I shall carry your assent with the general principles I have laid down, it must be admitted that the technical details can only be appreciated by experienced specialists. All that can be hoped is a general agreement amongst the staffs of the principal institutions in different countries where systematic botany is worked at; the free-lances must be left to do as they like.

Publications.

I have dwelt at such length on certain aspects of my subject that perhaps, without great injustice, you may retort on me the complaint of one-sidedness But when I survey the larger field of botany in this country, the prospect seems to me so vast that I should despair even if I had my whole address at my disposal of doing it justice. I think that its extent is measured by the way in which the publications belonging to our subject are maintained. First of all we have access to the Royal Society, a privilege of which I hope we shall always continue to take advantage for communications which either treat of fundamental subjects, or at least are of general interest to biologists. Next to this we have our ancient Linnean Society, with a branch of its publications handsomely and efficiently devoted to systematic work. Then we have the *Annals of Botany*, which has now, I think, established its position, and which brings together the chief morphological and physiological work accomplished in the country. Lastly, we have the *Journal of Botany*, a less ambitious but useful periodical, which is mainly devoted to the labours of English botanists. I remember there was a time when I thought that this, at any rate, was an exhausted field. But it is not so: knowledge in its most limited aspects is inexhaustible if the labourer have the necessary insight. The discoveries of Mr. Arthur Bennett amongst the potamogetons of the Eastern Counties is a striking and brilliant instance.

Besides the publication of the *Annals* we owe to the Oxford Press a splendid series of the best foreign text-books issued in our own language. If the thought has sometimes occurred to one's mind that we were borrowers too freely from our indefatigable neighbours, I, at least, remember that the late Prof. Eichler paid us the compliment of saying that he preferred to read one of these monumental books in the English translation rather than in the original. I believe it is no secret that botany owes the aid that Oxford has rendered it in these and other matters in great measure to my old friend the Master of Pembroke College, than whom I believe science has no more devoted supporter.

Palæobotany.

I have said much of recent botany; I must not pass over that of past ages. Two notable workers in this field have passed away since our last meeting. Saporta was with us at Manchester, and we shall not readily forget his personal charm. If some of his work has about it a too imaginative character, the patience and entire sincerity with which he traced the origin of the existing forms of vegetation in Southern Europe to their ancestors in the not distant geological past will always deserve attentive study. But in the venerable, yet always useful, Williamson we lose a figure whose memory we shall long preserve. With rare instinct he accumulated a wealth of material illustrative of the vegetation of the Carboniferous epoch, which, I suppose, is unique in the world. And this was prepared for examination with incomparable patience either by his own hands or under his own eyes. He illustrated it with absolute fidelity. And if he did not in describing it always use language with which we could agree, nothing could ruffle either his imperturbable good nature or the noble simplicity of his character. Truth to tell, we were often in friendly warfare with him. But I rejoice to think that before his peaceful end came he had patiently reconsidered and abandoned all that we regarded as his heresies, but which were, in truth, only the old manner of looking at things. And I think that if anything could have contributed to make his departure happy, it was the conviction that the completion of his work and his scientific reputation would remain perfectly secure in the hands of Dr. Scott.

Vegetable Physiology.

Turning again to the present, the difficulty is to limit the choice of topics on which I would willingly dwell. In an address which I delivered at the Bath meeting in 1888, I ventured to point out the important part which the action of enzymes would be found to play in plant metabolism. My expectations have been more than realised by the admirable work of Prof. Green on the one hand, and of Mr. Horace Brown on the other. The wildest imagination could not have foreseen the developments which in the hands of animal physiologists would spring from the study of the fermentative changes produced by yeast and bacteria. These, it seems to me, bid fair to revolutionise our whole conceptions of disease. The reciprocal action of ferments, developed in so admirable a manner by Marshall Ward in the case of the ginger-beer plant, is destined, I am convinced, to an expansion scarcely less important.

But, perhaps, the most noteworthy feature in recent work is the disposition to reopen in every direction fundamental questions. And here, I think, we may take a useful lesson from the practice of the older Sections, and adopt the plan of entrusting the investigation of special problems to small committees, or to individuals who are willing to undertake the labour of reporting upon special questions which they have made peculiarly their own. These reports would be printed *in extenso*, and are capable of rendering invaluable service by making accessible acquired knowledge which could not be got at in any other way.

We owe to Mr. Blackman a masterly demonstration of the fact, long believed, but never, perhaps, properly proved, that the surface of plants is ordinarily impermeable to gases. Mr. Dixon has brought forward some new views about water-movement in plants, which I confess I found less instructive than many of my brother botanists. They are expressed in language of extreme technicality; but, as far as I understand them, they amount to this. The water moving in the plant is contained in capillary channels; as it evaporates at the surface of the leaves a tensile strain is set up, as long as the columns are not broken, to restore the original level. I can understand that in this way the "transpiration current" may be maintained. But what I want to know is how this explains the phenomena in the sugar maple, a single tree of which will yield, I believe, 20–30 gallons of fluid before a single leaf is expanded.

We owe to Messrs. Darwin and Acton the supply of a "Manual of Practical Vegetable Physiology," the want of which has long been keenly felt. Like the father of one of the authors, "I love to exalt plants" (i. 98). I have long been satisfied that the facts of vegetable physiology are capable of being widely taught, and are not less significant and infinitely more convenient than most of those which can be easily demonstrated on the animal side. How little any accurate knowledge of the subject has extended was conspicuously demonstrated in a recent discussion at the Royal Society, when two of our foremost chemists roundly denied the existence of a function of respiration in plants, because it was unknown to Liebig!

Assimilation.

The greatest and most fundamental problem of all is that of assimilation. The very existence of life upon the earth ultimately depends upon it. The veil is slowly, but I think surely, being lifted from its secrets. We now know that starch, if its first visible product, is not its first result. We are pretty well agreed that this is what I have called a "proto-carbohydrate." How is the synthesis of this effected? Mr. Acton, whose untimely end we cannot but deeply deplore, made some remarkable researches, which were communicated to the Royal Society in 1889, on the extent to which plants could take advantage of organic compounds made, so to speak, ready to their hand. Loew, in a remarkable paper, which will perhaps attract less attention than it deserves from being published in Japan (*Bull. College of Agric. Imp. Univ. Tokio*, vol. i.), has from the study of the nutrition of bacteria, arrived at some general conclusions in the same direction. Bokorny appears recently to have similarly experimented on algæ. Neither writer, however, seems to have been acquainted with Acton's work. The general conclusion which I draw from Loew is to strengthen the belief that form-aldehyde is actually one of the first steps of organic synthesis, as long ago suggested by Adolph Baeyer. Plants, then, will avail themselves of ready-made organic compounds which will yield them this body. That a sugar can be constructed from it has long been known, and Bokorny has shown that this can be utilised by plants in the production of starch.

The precise mode of the formation of form-aldehyde in the process of assimilation is a matter of dispute. But it is quite clear that either the carbon dioxide or the water, which are the materials from which it is formed, must suffer dissociation. And this requires a supply of energy to accomplish it. Warington has drawn attention to the striking fact that in the case of the nitrifying bacterium, assimilation may go on without the intervention of chlorophyll, the energy being supplied by the oxidation of ammonia. This brings us down to the fact, which has long been suspected, that protoplasm is at the bottom of the whole business, and that chlorophyll only plays some subsidiary and indirect part, perhaps, as Adolph Baeyer long ago suggested, of temporarily fixing carbon oxide like hæmoglobin, and so facilitating the dissociation.

Chlorophyll itself is still the subject of the careful study by Dr. Schunck, originally commenced by him some years ago at Kew. This will, I hope, give us eventually an accurate insight into the chemical constitution of this important substance.

The steps in plant metabolism which follow the synthesis of the proto-carbohydrate are still obscure. Brown and Morris have arrived at the unexpected conclusion that "cane-sugar is the first sugar to be synthesised by the assimilatory processes." I made some remarks upon this at the time (*Journ. Chem. Soc.*, 1893, 673), which I may be permitted to reproduce here.

"The point of view arrived at by botanists was briefly stated by Sachs in the case of the sugar-beet, starch in the leaf, glucose in the petiole, cane-sugar in the root. The facts in the sugar-cane seem to be strictly comparable (*Kew Bulletin*, 1891, 35-41). Cane sugar the botanist looks on, therefore, as a 'reserve material.' We may call 'glucose' the sugar 'currency' of the plant, cane-sugar its 'banking reserve.'

"The immediate result of the diastatic transformation of starch is not glucose, but maltose. But Mr. Horace Brown has shown in his remarkable experiments on feeding barley embryos that, while they can readily convert maltose into cane-sugar, they altogether fail to do this with glucose. We may conclude, therefore, that glucose is, from the point of view of vegetable nutrition, a somewhat inert body. On the other hand, evidence is apparently wanting that maltose plays the part in vegetable metabolism that might be expected of it. Its conversion into glucose may be perhaps accounted for by the constant presence in plant tissues of vegetable acids. But, so far, the change would seem to be positively disadvantageous. Perhaps glucose, in the botanical sense, will prove to have a not very exact chemical connotation.

"That the connection between cane-sugar and starch is intimate is a conclusion to which both the chemical and the botanical evidence seems to point. And on botanical grounds this would seem to be equally true of its connection with cellulose.

"It must be confessed that the conclusion that 'cane-sugar' is the first sugar to be synthesised by the assimilatory processes seems hard to reconcile with its probable high chemical complexity, and with the fact that, botanically, it seems to stand at the end and not at the beginning of the series of metabolic change."

PROTOPLASMIC CHEMISTRY.

The synthesis of proteids is the problem which is second only in importance to that of carbohydrates. Loew's views of this deserve attentive study. Asparagin, as has long been suspected, plays an important part. It has, he says, two sources in the plant. "It may either be formed directly from glucose, ammonia (or nitrates) and sulphates, or it may be a transitory product between proteid decomposition and reconstruction from the [illegible] (*loc. cit.*, 64).

In some remarks I made to the Chemical Society I ventured to [illegible] suggestion that the chemical processes which took place under the influence of protoplasm were probably of a different [illegible] from those with which the chemist is ordinarily [illegible]. The plant produces a profusion of substances, [illegible] great facility, which the chemist can only build [illegible] way. As Victor Meyer (*Pharm. Journ.*, 18[illegible]) [illegible] "In order to isolate an organic substance we [illegible] the purely accidental properties of [illegible]. In other words, the chemist only [illegible] stability; while it can not [illegible] which plays a part in the processes of life [illegible] every respect. I am convinced that if the [illegible] field of protoplasmic activity he will [illegible] limitations, and be prepared to [illegible] algebra, there may be more than one chemistry. I am glad to see that a somewhat similar idea has been suggested by other fields of inquiry. Prof. Meldola (NATURE, xlii. 250) thinks that the investigation of photochemical processes "may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree." I am delighted to see that the ideas which were floating, I confess, in a very nebulous form in my brain are being clothed with greater precision by Loew.

In the paper which I have already quoted, he says of proteids (*loc. cit.*, 13): "They are *exceedingly labile compounds* that can be easily converted into relatively stable ones. A great lability is the indispensable and necessary foundation for the production of the various actions of the living protoplasm, for the mode of motions that move the life-machinery. There is a *source of motion* in the labile position of atoms in molecules, a source that has hitherto not been taken into consideration either by chemists or by physicists."

But I must say no more. The problems to which I might invite attention on an occasion like this are endless. I have not even attempted to do justice to the work that has been accomplished amongst ourselves, full of interest and novelty as it is. But I will venture to say this, that if capacity and earnestness afford an augury of success, the prospects of the future of our Section possess every element of promise.

PHYSICS AT THE BRITISH ASSOCIATION.

THE proceedings of this Section were commenced by the delivery of the presidential address by Prof. W. M. Hicks. In seconding the vote of thanks to the President, Prof. Fitzgerald referred to the possible change of mass with temperature, suggested in the address, and pointed out that such a phenomenon would show itself by a deviation of planetary motions from strict conformity to Kepler's laws, owing to their change of mass on cooling.

Sir Douglas Galton exhibited plans of the German Reichsanstalt, and of the new buildings in course of construction, and gave a more detailed account of the management of this institution than is contained in his presidential address to the Association. His object in reading the paper was to revive a movement set on foot at a previous meeting by Prof. Oliver Lodge. The Committee appointed at that time to consider the question of a National Physical Laboratory for the United Kingdom made but little progress, possibly because they did not propose to develop any existing institution. He suggested that the scope of the Kew Observatory should be extended so as to include research, and that it be made the starting-point for the national laboratory.

A discussion followed, in which several members took part. Prof. Rücker lamented the want of concentration and organisation in research work, and thought a national laboratory might remedy this. He regretted that the day was passing away when a man could undertake both teaching and research, because, in his opinion, teachers should not give up research. Prof. Oliver Lodge drew attention to the enormous advantages possessed by a national institution, for carrying on researches extending over a long period. In a university laboratory such research would possibly be discontinued with a change of professor. The universities would still do pioneer work, discovering new fields of research and obtaining preliminary results. Prof. Fitzgerald, on the other hand, did not think it advisable to hand over research to a national laboratory, whereas he strongly advocated an extension of the standardising work performed at Kew. He believed that the highest kind of instruction was training in research work, and it was the function of the universities to give this instruction. Instead of that, the professors are called upon to cram old knowledge into immature and stupid students. The Section has appointed a Committee to reconsider the question of a national laboratory.

Prof. Henrici read a paper on the teaching of geometrical drawing in schools, which was, he said, as a rule very bad. He pointed out that Euclid's constructions are generally followed, the use of the set square being discarded and only straight-edges and compasses used. He urged the desirability of discarding Euclid in the teaching of geometrical drawing, advocating the use of the set-square from the very commencement. The examples ought to be so arranged that a student can verify his constructions for himself; he therefore suggested the appointment of a Committee to report on the whole question and issue

a syllabus of examples. This suggestion was adopted by the Section.

The range of subjects included in the work of the Section was perhaps nowhere better exemplified than in the passage to the next paper, a report on cosmic dust, by Dr. J. Murray. An examination of the red clay from the bottom of the Pacific Ocean, in places 1000 miles from any coast, enables three classes of magnetic particles to be distinguished; these are—crystalline fragments of magnetic or titanic iron, dark shiny spherules containing metallic iron, and the brownish spherules known as chondres. The various layers of manganese nodules found surrounding nuclei of tertiary teeth or bones contain these black and brown spherules, and there is every indication that the brown ones are of extra-terrestrial origin. In this case they ought to occur at all, or at any rate many, points on the earth's surface; Dr. Murray has, however, looked for them in vain both in the dust of Greenland glaciers and on the summit of Ben Nevis. He is of opinion that the accumulation of meteoric dust takes place with exceeding slowness, say about 20 lbs. of dust per square mile per century, and that the bed of the Pacific Ocean has not received one foot of deposit since the tertiary period. Consequently any attempt to gather these particles will probably be fruitless, unless continued over a long period. He wished for suggestions as to the best method of procedure in the future. It was pointed out that a good opportunity for the collection of meteorites will be afforded by the meteor shower of November 1899.

The Committee on underground temperature have been fortunate this year in obtaining records from a bore-hole in New South Wales, the first observations made in the southern hemisphere. The bore-hole is situated near Port Jackson, close to Sydney Harbour; it is 2929 feet deep, and contains water. The gradient observed was a small one, being a rise of 1° F. in descending 80 feet vertically. The observers suspected that the temperature of the rock was influenced by the proximity of the water in the harbour, but an examination of the temperature distribution in the harbour did not confirm this. Lord Kelvin suggested the African mines as a new field for observations.

Prof. S. P. Thompson reported the recommendations of the Committee on the size of pages of scientific periodicals. It is considered advisable to retain quarto and octavo sizes, and certain limits for text and margin are given for each of these sizes. There appeared to be a strong feeling against any change in the sizes of the Royal Society's publications. During the year the Committee will endeavour to induce other scientific societies to adopt the standard sizes recommended.

Prof. Rucker communicated the results of a comparison of magnetic standard instruments, made by himself and Mr. W. Watson. In his presidential address to the Section last year he showed that it was useless to proceed further with a magnetic survey until a direct comparison of standards used in the various observatories had been made, because it was well known that instruments differed greatly. During the year he has visited the various magnetic observatories, carrying a portable declinometer of the Kew pattern, and with Mr. Watson's assistance has directly compared the simultaneous readings of his declinometer and that of the observatory. Errors are found in the latter, which are in every case traceable to magnetic material in or on the wooden box containing the suspended magnet. If this box be replaced by an ebonite one, the error disappears. It is, however, easier to allow for the error than to get rid of it; its amount is perfectly definite.

On Friday the Section sat jointly with Section B. Lord Rayleigh read a paper on the refractivity and viscosity of these gases. He described how, by means of an electric arc, kept up for several weeks in a mixture of oxygen and atmospheric nitrogen, he finally obtained more than a litre of argon at atmospheric pressure. This proved to have the same density as the specimen obtained by the magnesium method. The refractive index was measured by the interference method of Fizeau, the two beams being separated by slits in front of the lens nearest the eyepiece. The latter was constructed of cylindrical lenses. To avoid the use of cross-wires, the tubes containing the gases under comparison were arranged so as not to occupy the whole field of view, some light passing parallel to, and outside them; two sets of fringes were thus obtained, which could be brought to coincidence by varying the pressure of either gas. Adjustments were made for several pressures, one of the tubes always containing air. The values of the refractivity ($\mu - 1$) were, for argon 0·961, and for helium 0·146, that of air being taken as unity. The viscosity of each gas was measured by its rate of flow through a capillary tube, the results being (air = 1) argon 1·21, helium 0·96. Lord Rayleigh mentioned that a sample of nitrogen collected from a Bath spring, where it bubbles out along with the water, gave the D_3 line of helium. Dr. Gladstone showed that the results of these experiments assign to argon the atomic weight 20, its specific refractive energy being intermediate between those of fluorine and sodium, but not between those of potassium and calcium.

Prof. Schuster then opened a discussion on the evidence to be gathered as to the simple or compound character of a gas from the constitution of its spectrum. Recent spectroscopic work in connection with argon and cleveite gas has directed attention to the double spectra exhibited by these substances, and conjectures have been made that the two spectra indicate the gases to be mixtures. Prof. Schuster expressed strongly the view that gases with double spectra are not necessarily mixtures or compounds. He quoted in support of this the cases of sodium and mercury vapours, and oxygen, in all of which the absorption spectrum differs from that of the luminous vapour. The difficulty is not explained by assuming dissociation to occur, because some substances have three or more spectra. He thought mere examination of spectra would not suffice to determine whether an unknown substance is an element, mixture of elements, or compound.

The despondent view of Prof. Schuster was not shared by Prof. Runge, of Hanover, who at this point contributed an account of the researches of himself and Prof. Paschen on the spectrum of cleveite gas, showing that it is a mixture. (An account of this work by the authors themselves will be found on p. 520.)

Dr. G. J. Stoney contributed to the discussion by a paper on the interpretation of spectra.

On Saturday the Section was subdivided into two departments, mathematics and meteorology.

In the department of mathematics, Lord Kelvin read a paper on the translational and vibrational energies of vibrators after impacts on fixed walls, in which he sought to find an exception to the Maxwell-Boltzmann theorem relating to the average translational energy of the molecules of a gas. He calculated the time-average of the translational energy of a free particle after coming into contact with a vibrating particle, and found it always in excess of that which would be given by the Maxwell-Boltzmann law, though approximating more nearly to that average when the number of encounters was considerable; and that it seemed ultimately to give a total average out of accordance with the law. In the discussion which followed, Mr. G. H. Bryan pointed out that the Maxwell-Boltzmann law referred to the statistical average energy of a great number of particles, not to the time-average energy of a single particle.

Prof. Hicks, in his paper on a spherical vortex, stated that he had proved the possibility of building up a compound spherical vortex consisting of successive shells in which the rotation is oppositely directed, the vorticity and size of each shell satisfying a definite relation. In a paper on bicyclic vortex aggregates, he stated that it was possible, with given current and vortex-sheets, to have a steady *spiral* motion round an axis, compounded of motion in planes through the axis and motion in circles round the axis, the cyclic constants of the two component motions being independent of each other.

Mr. G. T. Walker showed an ingenious top in the shape of a flattened ellipsoid in which rotation could become converted into oscillations, and *vice versa*, by means of an adjustable piece which could be arranged unsymmetrically.

Dr. Burton made some suggestions as to matter and gravitation in the cellular vortex ether described in Prof. Hicks's presidential address.

Mr. P. H. Cowell read an important paper on recent developments of the lunar theory, chiefly by Dr. G. W. Hill, extended in the current number of the *American Journal* by an admirable paper by Prof. E. W. Brown. The order of work in attacking problems in the lunar theory is quite altered and much simplified in the new method. In a short discussion which followed, Mr. Cowell stated that Prof. Brown was engaged in bringing out a treatise on the lunar theory.

Prof. J. D. Everett read a paper on absolute and relative motion; and Mr. W. H. Everett made a communication on the calculation of the magnetic field due to a current in a solenoid.

In pure mathematics, Major MacMahon gave an interesting method of graphically representing partitions of numbers.

Colonel Cunningham read a paper on Mersenne's numbers, which are numbers of the form $2^q - 1$, where q is a prime, and which were first discussed by Mersenne about the year 1664. Colonel Cunningham also described a book of tables which he proposed to calculate, giving the solution of the congruence $2^x \equiv R$ (mod. p) for all moduli (p) which are primes, or powers of primes, up to 1000. There are to be two tables for each modulus, one giving the values of R for a series of values of x; and the other giving the smallest values of x for a series of values of R. He described some of the uses of such a table, and stated that the plan on which it would be drawn up would be precisely like a somewhat similar table by Jacobi, described in Prof. Cayley's report on mathematical tables in the British Association Report of 1876.

Prof. Alfred Lodge drew the attention of the Section to a multiplication table up to 1000 × 1000, drawn up by Mr. M. B. Cotsworth, of Holdgate, York, which was exhibited; it is similar to Crelle's table of the same extent, though in some respects more convenient.

Prof. M. J. M. Hill described two species of tetrahedron, the volume of any member of which can be determined without using the proposition that tetrahedrons on equal bases, and having equal altitudes, are equal.

In the department of meteorology, Mr. Eric S. Bruce put forward a new theory of lightning flashes, based on the principle of the pin-hole camera. The light from a concealed flash might, he supposed, pass through a small aperture in the concealing cloud and fall on another cloud, forming an inverted image of the flash. If there were several apertures we should have as many images. They would be faint, possibly too faint to affect a photographic plate. Moreover, if the receiving cloud were of irregular shape, an originally straight flash would appear distorted into a zig-zag line on the cloud. Mr. Symons thought a brighter patch of light ought to occur at the angles of the image thus distorted, and he scarcely thought the conditions imagined by Mr. Bruce corresponded with those of nature.

The report of the Committee on earth tremors was presented by Mr. Symons, who, in referring to the delicacy of the instruments used in their observations, said that an angle equal to that subtended by a chord 1 inch long at the centre of a circle 1000 miles in radius could be detected. Since last report two similar pendulums have been purchased, of the kind described in NATURE, vol. l. pp. 246-249 (1894); each possesses its own photographic recording apparatus. One of these has been recently erected in the cellar of Mr. Davison's house in Birmingham; the other should have been placed in a house three-quarters of a mile to the east, but this was found impracticable. It will be placed somewhere in the neighbourhood, and comparisons of the records of the instruments will be made during the year, after which the second one will be available for another station. An appendix to the report by Mr. Davison gives the bibliography and classification of horizontal pendulums.

Prof. John Milne gave an account of the long report of the Committee on seismological phenomena in Japan. This commences by a reference to the great loss caused by the recent fire at Prof. Milne's house and observatory, after which follows a description of the records of the Gray-Milne seismograph. Attached to the report is a catalogue of 8331 earthquake shocks recorded in Japan between 1885 and 1892, giving full particulars of the centre and area of disturbance. It enables the approximate *weight* of each to be found, and permits the division of Japan into fifteen distinct seismic districts. The next section of the report deals with the rate of propagation of earthquake disturbances from Japan to Europe. The small tremors which occur in the ten seconds or so before an earthquake shock are transmitted to Europe, but they are spread over half an hour; it appears, therefore, that the preliminary tremors either travel more quickly, or reach Europe by a shorter route than the main shock. The latter is known to travel along the surface at about 3000 metres per second. Do the tremors travel at 8000 to 11,000 metres per second, or do they pass through the earth, along a chord? If the latter, we may hope for some further knowledge concerning the interior of the globe. Prof. Milne has set up horizontal pendulums in nearly a score of places, and finds great differences in their behaviour. They all exhibit a general displacement, *i.e.* tilt, in the same direction, and similar long period movements. Examined from hour to hour, however, none of them show the existence of a diurnal wave. After a long and very laborious search, graphically described to the Section by Prof. Milne, he succeeded in tracing this diurnal effect to the local removal of load from the alluvium by greater evaporation from exposed areas. At night the movement is slight, and is probably accounted for by the condensation, at the cold surface, of aqueous vapour after rising through the warm earth. Some observations have been made on the disturbance of the pendulums by earth tremors. Their cause has not been ascertained, but they always occur with greatest intensity between 5 and 9 a.m. They are most marked with a steep barometer gradient and consequent wind, local or distant.

As Prof. Milne has now returned from Japan, and the earthquake catalogue is completed, the Committees on earth tremors and seismological phenomena have united under the latter name. The new Committee is a large one, and with Prof. Milne and Mr. Davison as joint secretaries, it ought to do good work.

A new theory of thunderstorms was advanced by Prof. Michie Smith in his paper on Indian thunderstorms. His observations, made at Madras, showed that sheet-lightning occurs there every evening during several months of the year, always in the south-west and near the horizon. Lightning phenomena in the morning occur, on the other hand, in the north-east. The phenomena consist of actual discharges between two clouds, or two portions of the same cloud, and are not reflections of distant lightning; they take place in the upper portions of low-lying cumulus clouds. Prof. Smith attributes them to the clouds formed in the regions of still air at the meeting of the land and sea breezes, and has observed in these regions the simultaneous rise of two close parallel clouds from the edge of the cumulus; such clouds are scarcely distinguishable except with oblique illumination, and it is within, or between, them that the discharges occur. The time of their formation depends on the hour at which the sea breeze sets in, being roughly three hours later. The land breeze being dry and dusty is negatively charged, while the sea breeze is known to carry a strong positive charge: equalisation of the electrical states of the clouds formed out of these will, therefore, give rise to lightning. Prof. Smith referred to the iridescence or nacreous appearance of the edges of the clouds when rapidly sinking, and considered this effect to be due to the dust left behind by them.

This paper gave rise to an interesting discussion, chiefly with reference to the origin of dust in clouds, and the source of their electricity. Mr. John Aitken pointed out that thunderstorms are most probably the effect, not the cause, of purifying the air. He gave instances of thunderstorms on several successive days, all of which left the air dusty and impure; eventually the air cleared, and no more thunder occurred. Prof. Schuster alluded to the fact that twenty-five theories of thunderstorms had been put forward in a dozen years, and in a single year five appeared. He attributed the positive charge of the sea breeze to the electrification of the air by the spray from the breaking waves; Lenard has shown that the spray of pure water gives a negative charge to the air, while that of salt water communicates a positive charge. He believed the dust of clouds to be acquired locally, except that at high altitudes, which we know to be carried long distances. A proof of this is to be found in the Himalayas where certain valleys are dusty and others fairly free from dust, although all receive the wind from the Indian plains. His observations of nacreous clouds in England had led him to connect them rather with the ice particles of cirrus clouds than with dust. To this latter point Prof. Michie Smith replied that the nacreous appearance fits the edge of the cumulus so closely that he believes the two to be connected.

The Committee on the application of photography to meteorology are proceeding with the photography of clouds near the sun by means of two cameras at a fixed distance apart, and exposed simultaneously by an electrical arrangement. In this way they hope to obtain absolute measurements of cloud altitudes. For purposes of measurement the sun's image appears in all the photographs. A photograph of the rainbow, by Mr. Andrews of Coventry, is the first of its kind received by the Committee. It shows the secondary bow, and the greater brightness of the region within the bow.

During a recent visit to the Engadine, Prof. Schuster has made observations on the atmospheric electricity near the ground at different heights above sea-level. The readings were taken with Lord Kelvin's portable electrometer, which worked very satisfactorily and seems well adapted for such purposes. In all cases positive charges were found, increasing with height but in an apparently erratic fashion. The normal positive charge at the foot of a glacier was found to be strengthened by a wind blowing down it, and Lenard's observations on the negative

electricity of waterfalls were all confirmed. The daily curve of atmospheric potential in the valley of Pontresina shows a maximum at 11 a.m., dipping a little and rising again to an afternoon maximum at 5 p.m., then rapidly descending as the evening breeze sets in. Discussion on the paper related chiefly to the behaviour and temperature errors of portable electrometers, the latter being somewhat large and quite unexplained. Prof. Ayrton suggested a crucial experiment to determine whether atmospheric electricity is due to an actual distribution in the air, or to induction from the earth's surface.

The report of the Ben Nevis Observatory for 1894 was presented. The mean hourly velocity of the wind at the top of the mountain, and the mean rainband, are included in the report for the first time. Dr. Buchan and Mr. Omond have made progress in collating the simultaneous records made at Fort William and the summit; the differences between them are to be examined especially with respect to their bearing on coming storms. Even at this stage the results indicate that the present theory of cyclones requires great modification.

The first part of Monday's sitting was devoted to a discussion on the nature of combination tones. Prof. Rücker gave an admirable account of the history of the subject, pointing out that Helmholtz originated both the theory that they are objective, and that which supposes them subjective. He reviewed the theories of Prior and others, according to which summation and difference tones are explained as beat tones of various kinds: and he called attention to Helmholtz's proof that an asymmetrical elastic body, such as the disc of a microphone or the drumskin of the ear, would resound to the difference tone between two notes. Prof. S. P. Thompson regretted that in his historical survey Prof. Rücker did not refer to his own work. He read communications from König and Hermann, defining their views. König distinguishes between beat tones, which can be resonated, and difference tones, to which the resonator does not respond; the latter are subjective. Hermann objects to Helmholtz's theory that it is inadequate to account for the loudness of the combinational tones. Prof. Thompson mentioned experiments to show that difference tones may be obtained by sending one sound to each ear, and in other cases where the drumskin does not receive the sounds. He described also the effect of periodically intermitting a single tone, or of suddenly and periodically changing its phase, in both of which cases a tone is heard the pitch of which is the frequency of phase-change or intermittence.

Prof. Everett sought for the cause of combination tones in the air itself, which would be disturbed unsymmetrically by two sounds of finite amplitude. He thought, however, that in the combined effect of two tones, the vibration corresponding to the fundamental Fourier term common to each would be louder than the difference tone, a view in which Lord Kelvin concurred. Dr. Burton pointed out that Prof. Everett's explanation of combination tones would apply also to phase tones and intermittence tones. Dr. G. J. Stoney thought resonance by the mouth-cavity was an important factor in hearing, and in the selection of separate sounds from among a number. There was a general agreement that summation tones have never been heard, and probably do not exist.

Mr. E. H. Griffiths opened a discussion on the desirability of a new Practical Heat Standard. He showed that the use of water as the standard substance in heat measurements had led to great confusion, on account of the various assumptions as to its variation of heat-capacity with temperature. The curves of heat-capacity of water and temperature, used by different experimenters, were exhibited; according to which the author's results furnished a value about the mean of those of recent observers. Mr. Griffiths suggests as a heat unit, absolute, independent of any one person's results, and convenient in magnitude, the heat energy of 42 million ergs. To interpret it as a water standard he proposes to take it as the thermal capacity of a gramme of water at 10° C., as measured by the hydrogen thermometer; and he gives a formula to find the heat-capacity at other temperatures than 10° C. Lord Kelvin said that Prof. Rankine had previously suggested the dynamical specific heat of water as a standard. Mr. W. N. Shaw thought it advisable to make a distinction between the numbers for the absolute thermal capacity and the specific heat of a substance. He believed this would be done most simply by taking the thermal unit as the heat energy of a million ergs; the specific heat of water at 10 C. would then be unity, and its thermal capacity 42 units. The choice of a thermal unit has been referred to the Electrical Standards Committee.

Dr. C. H. Lees gave an account of the method and results of experiments on the thermal conductivity of mixtures of liquids. The method used was that of Christiansen, in which the heat is conducted through the liquid enclosed between two copper discs, and confined by an ebonite ring if necessary. The results show that the conductivity of mixtures of two liquids is less than the value calculated by the ordinary law of mixture, at any rate for water, ethyl alcohol, methyl alcohol and glycerine. Dr. Lees undertook the experiments to verify certain relations suggested by Prof. H. F. Weber between molecular weight, density, specific heat and thermal conductivity.

A paper by Prof. Ramsay and Miss Dorothy Marshall was read by the latter, the subject being a method of comparing heats of evaporation of liquids at their boiling-points. After remarking that the data of heats of evaporation are very scanty and discrepant, Miss Marshall described a method by which two liquids, kept at their boiling points by jackets of their vapour surrounding them, are boiled by means of equal bare platinum wires heated by an electric current. A comparison of the amounts of the liquids evaporated in a given time gives the ratio of heats of evaporation. For absolute values a special determination was made on benzene by Mr. Griffiths and Miss Marshall. Alcohol was carefully compared with benzene, and all other liquids were then compared with alcohol. Water was very erratic in its behaviour, probably because of its greater electric conductivity.

Mr. G. U. Yule exhibited a harmonic analyser.

At the meeting on Tuesday, Lord Kelvin described the results of experiments for the electrification and diselectrification of air and other gases, made by Messrs. Maclean and Galt, and himself. In the earlier experiments the air inside a metal can was electrified by points, the can being put to earth: on insulating the can and blowing out the air, the charge acquired by the can was equal and opposite to that of the air. Electrification of air and other gases in gas-holders over water, by points and flames, was also tried, greater electric densities being thus obtained than by the previous method. The maximum effects were $1\cdot5 \times 10^{-4}$ electrostatic units per c.c. for air, and $2\cdot2 \times 10^{-4}$ for CO_2. The gases were diselectrified by "filtering" them through metal tubes containing conducting wire gauze and cotton wool. Very little electrifying effect was found when uncharged air passed through a platinum tube 100 cm. long and 1 mm. diameter, until the tube was made red-hot, in which case the air acquired a strong positive charge. Prof. Oliver Lodge suggested the use of a filter consisting of a metal tube, highly polished inside and illuminated by an electric beam shining into its interior. Lord Kelvin said that in all Hertz's or Elster and Geitel's experiments on diselectrification by light, the charge of the air round the illuminated body should be examined.

Prof. Rücker made a communication on vertical (earth-air) electric currents. At the meeting of the Association last year, Dr. Adolph Schmidt accounted for a portion of the earth's magnetism by assuming electric currrents to pass vertically between earth and air. Such currents would be shown by the non-vanishing of the line-integral of magnetic force when taken round a closed circuit on the earth's surface. The matter was tested in this way by Messrs. Kay and Whalley, using four independent circuits, three in Great Britain and one in Ireland, and obtaining the data of magnetic force from the surveys of 1886 and 1891. The results do not decide the general question, but they show that in the United Kingdom the upward current has certainly not more than one-tenth of the value required in Dr. Schmidt's theory. Lord Kelvin calculated that the current assumed by Dr. Schmidt (0·1 ampere per square kilometre of surface) amounts to a removal of the fine-weather charge of the air near the earth 36 times per second. Dr. Rijchevorsel said he understood that magnetic observations were about to be made in Switzerland, which would furnish data for similar calculations there.

Mrs. Ayrton made a communication on the connection between potential difference, current and length of arc, in the electric arc. The results of carefully-performed experiments, verified also by recalculation from the data of other observers, show that the following relations hold:—(1) For constant length of arc the power (number of watts used in the arc) is a linear function of the current; (2) for constant currents the power is a linear function of the length of arc: (3) for constant length of arc the curve of potential difference and current is a rectangular hyperbola. All these laws are included in the President's statement that the surface with potential difference, current and arc length as coordinates, is a hyperbolic paraboloid.

Prof. Ayrton read a paper by Mr. Mather and himself, in which arguments were advanced against the existence of a back electromotive force in the electric arc. The authors describe a method of measuring the true resistance of the arc, namely the ratio of a small increase of potential difference to the corresponding increase in the current; this, of course, is a negative quantity. The same authors described a magnetic field tester, an application of the ordinary exploring coil and ballistic galvanometer method, with a spiral spring to effect rapid rotation of the exploring coil, and a modified D'Arsonval galvanometer with shuttle-wound coil capable of rotating through several turns without losing the proportionality of angular displacement and restoring force.

The velocity of light in vacuum tubes conveying an electric discharge formed the subject of a paper by Messrs. Edser and Starling. Vacuum tubes were placed in the path of the two beams of a Fizeau interference apparatus, and the position of the bands observed. No appreciable shift of the bands was obtained either by setting up an induction-coil discharge, or by the discharge of [illegible] through the tubes when placed in series with a piece of wet string. The discharge in the latter case lasted one-thirtieth of a second, and the authors show that a disturbance of the bands of so long duration would have been observed.

Mr. F. G. Baily read a paper on hysteresis of iron in an alternating magnetic field, in which he showed that the hysteresis of iron increases with the field up to a maximum value, in accordance with Ewing's theory. The experiments were made by the [illegible] method, using a small laminated armature consisting of thin discs of charcoal iron; the most intense magnetic field used was 22,000 C.G.S. units, and the hysteresis was measured by the rise of temperature of the armature.

On Wednesday, Dr. Gladstone and Mr. W. Hibbert made a communication on the change of molecular refraction in salts or acids dissolved in water. The molecular refraction of a substance [illegible] when the substance changes its state, and a further slight alteration takes place on diluting its solution; the authors have obtained some evidence of a close connection between these changes and the variations of electric conductivity [illegible] its solutions. Such a connection would have an important bearing on the theory of solution.

The report of the Electrical Standards Committee was read. The Committee [illegible] during the year to institute a comparison between the British and German standards of resistance, and have prepared coils for this purpose, which have already been tested at the Reichsanstalt. The Committee, recognising the need for practical units of magnetic field and magnetic potential, recommend the tentative adoption (1) of a unit equal to 10^8 C.G.S. lines, to be called a *weber*, (2) the C.G.S. unit of magnetic potential, to be called a *gauss*. They also recommend that the termination [illegible] be used in describing the properties of a piece of matter, e.g. the resistance of a copper wire, and the termination [illegible] for the specific properties of the material, *e.g.* [illegible] of copper would mean the resistance of a centimetre [illegible]. Prof. Oliver Lodge explained, and advocated [illegible] the proposed units. Prof. S. P. Thompson, while agreeing with the Committee as to the desirability of having [illegible] field and magnetic potential, thought the choice [illegible] should be left with the practical men who use them. He considered the proposed *weber* was too large, and advocated the retention of the C.G.S. "line," using the kilo- and [illegible] multiples; further, he did not see any necessity for [illegible] the ampere turn in order to replace it by the [illegible]. Prof. Thompson pointed out a more formidable objection, [illegible] the American Institute of Electrical Engineers have [illegible] *weber* to a different unit, and have suggested [illegible] for the gauss. Several members continued the [illegible] and Prof. Perry expressed his opinion that the [illegible] ought to be settled by a general congress.

[illegible] apparatus for tracing the form of the wave of [illegible] alternate current circuit were exhibited and described [illegible] Messrs. Barr, Burnie and Rodgers, the other [illegible] Mr. Mather.

[illegible] the apparatus designed for the [illegible] temperature thermometers at Kew Observatory [illegible] Callendar and Griffiths platinum [illegible] glass or porcelain tube, and can be [illegible] thermometer to be calibrated, in [illegible] sulphur vapour, according to the [illegible] measured by a Wheatstone [illegible] are enclosed in a copper box, five sides of which are immersed in a water-bath of constant temperature, while the top is surmounted by a case similar to that of a chemical balance. The coils of the bridge are of platinum-silver, wound double, and are not embedded in paraffin, the object being to allow them to assume the temperature of the box and surrounding water as quickly as possible.

A vote of thanks to the Chairman and Secretaries terminated the proceedings.

CHEMISTRY AT THE BRITISH ASSOCIATION.

With the exception of Prof. Runge's announcement of the undoubtedly compound nature of helium, few of the communications laid before Section B at Ipswich are likely to awaken great interest outside chemical circles. The discussions, however, which are now a recognised feature of these meetings, were especially successful, and it is not too much to hope that the joint meeting with the newly-formed Botanical Section may be the means, if only indirectly, of bringing about results of great importance to the agricultural community.

Following the President's valuable address, Sir Henry Roscoe and Dr. A. Harden communicated to the Section an interesting discovery in historical chemistry. It has been generally assumed that Dalton arrived at the idea of atoms with definite weights from a consideration of the proportions in which certain elements combined. From the examination of a number of manuscript volumes of Dalton's own laboratory notes, which they have recently discovered in the library of the Manchester Literary and Philosophical Society, Sir Henry Roscoe and Dr. Harden conclude that Dalton worked out his theory solely from physical considerations as to the constitution of gases. His mind being saturated with Newton's ideas concerning atoms, it was from these that his own atomic theory was developed.

Later on, quoting not only his own results but those of other chemists, he seems to have been led to the law of multiple proportions as the only conceivable mode of combination between atoms. Extracts were given from his notes showing that certain numbers, usually quoted as having led him to his atomic theory, *e.g.* the analyses of marsh gas and olefiant gas, were only inserted in his tables some time after the publication of his ideas.

Prof. Armstrong said it was satisfactory to learn that Dalton had really arrived at his conclusions from truly philosophical considerations, without reference to the very crude numbers, usually quoted as sufficient basis for the laws that he worked out.

The report of the Committee on the teaching of science in elementary schools was read by Dr. J. H. Gladstone. During past years there has been an increase in the number of subjects taught, and in the number of pupils receiving instruction. The alteration in the system of inspection will have an especially useful effect in the teaching of science. The question of the training of teachers is discussed in the report. A course for mistresses on domestic science, dealing as far as possible with the nature of the processes and materials employed in the household, has been found successful. The great obstacles to good science teaching at the present time in elementary schools are: (1) Large classes; (2) multitude of subjects; (3) insufficiency of the training course for teachers in science subjects; (4) effects of the old science and art system, which is clearly far too formal, and pays far too little attention to ordinary requirements.

The courses on elementary physics and chemistry, and the science of common things are found to be more attractive than pure chemistry.

Other subjects dealt with in the report are school visits to museums; the right method of giving object lessons; and the teaching of the metric system. Finally it is suggested as a question worth consideration, whether the recognised school age should not be raised from thirteen to fourteen.

In the discussion which followed the reading of the report, the relation of County Councils to elementary schools was debated, and it was contended that these are helped indirectly by the Councils providing facilities for the training of teachers.

Mr. G. J. Fowler read a paper on the action of nitric oxide on certain salts, by H. A. Auden and G. J. Fowler, in which the action of nitric oxide on different salts at various tempera-

tures is described. Oxy-salts have been chiefly examined, the most interesting results being obtained with the chlorates and iodates of potassium and silver. With potassium chlorate action takes place at the ordinary temperature, chlorine being evolved, but no potassium chlorate being formed. With silver chlorate, chlorine is also evolved, but some chloride is obtained. Potassium iodate yields iodine but no potassium iodide at a low temperature, while silver iodate is completely converted into iodide, no iodine being liberated, or silver nitrate formed. It is suggested that these results tend to show a difference in constitution between the silver and potassium salts.

Prof. Clowes gave an account of further experiments on the respirability of air, in which a candle flame has burnt till it is extinguished. He finds that an atmosphere, which contains oxygen 16·4 per cent., nitrogen 80·5 per cent., carbon dioxide 3·1 per cent., will extinguish a candle flame, but is still, according to the experiments of Haldane, not only respirable, but would be breathed by a healthy person for some time without injury. An atmosphere which extinguishes a coal-gas flame, however, appears to approach closely to the limits of respirability, as far as the proportion of oxygen which it contains is concerned. The candle and lamp flames should be discarded as tests of the respirability of air in favour of the coal-gas flame.

A paper was read by Mr. D. J. P. Berridge, on the action of light upon the soluble metallic iodides in presence of cellulose, in which it was shown that the amount of iodine liberated from potassium iodide by the combined action of light, air and moisture, is greatly increased by the presence of cellulose, this substance probably combining with the potassium hydrate liberated in the reaction. By investigating the conditions of formation of the chocolate stain obtained when note-paper containing starch, and soaked in potassium iodide solution, is exposed to light, evidence is obtained of the formation of a tri-iodide of potassium. The iodides of sodium, calcium, strontium, barium, iron, and zinc, all behave like the potassium salt; cadmium seems alone unable to form a higher iodide.

Dr. C. A. Kohn read the second report of the Committee on quantitative analysis by means of electrolysis. The bibliography of the subject has been completed. The experimental work has been carefully organised, and the results on the determination of bismuth and of tin are nearly complete.

Sir H. E. Roscoe presented the report of the Committee appointed to prepare a new series of wave-length tables of the spectra of the elements.

Some interesting communications were made to a joint sitting of Sections A and B; and the account of these, which we give in our report of the work of the former Section, is supplemented by the following notes on Dr. Gladstone's and Prof. Schuster's communications.

Dr. Gladstone's paper was on specific refraction and the periodic law, with special reference to argon and other elements. In former years he had shown that the specific refractive energies of the elements in general were, to a certain extent, a periodic function of their atomic weights. With regard to argon, the specific refractive energy of argon gas as reckoned by Lord Rayleigh's data is 0·159. At the suggestion of Deeley, the bearing of this result on the atomic weight of argon was considered. If the atomic weight be 19·94, the molecular refraction will be 3·15. This figure is almost identical with that belonging to oxygen and nitrogen gas, and differs considerably from that of calcium, which has a molecular refraction of 10·0 and a specific refractive energy of 0·248. These facts tend to suggest an atomic weight of 20 for argon, and to place it in the vicinity of the alkali metals.

The discussion, which was opened by Prof. Schuster, on the evidence to be gathered as to the simple or compound nature of a gas from the constitution of its spectrum, dealt with matters of rather more physical than chemical bearing. Of special interest to chemists, however, was the evidence cited by Prof. Schuster for considering that the variations noticed in the spectra of sodium, nitrogen, and mercury under different conditions were due to differences in atomic aggregation.

Monday's sitting was devoted to a discussion, held in conjunction with Section K (Botany), on the relation of agriculture to science. It was introduced by Prof. R. Warington in a paper entitled, "How shall agriculture best obtain the help of science?" This was devoted to a consideration of the best means for diffusing a knowledge of the scientific principles of agriculture. Certain things could be usefully done by a Board of Agriculture, and others by County Councils. The formation of a really complete agricultural and horticultural library, freely open to the public, and the maintenance of an English agricultural journal, are matters which might fall to the Board of Agriculture. The advantages to be derived from a Government laboratory and experimental station were dwelt upon. Local stations and secondary agricultural schools should be maintained by the County Councils, who also should inspect the technical instruction in their locality. The foundation of habits of observation and logical reasoning must be laid in the elementary school if higher instruction is afterwards to be given. Higher qualifications should be required for agricultural lecturers than is at present the case.

Mr. T. Hendrick contributed a second paper. He spoke of the apathy and even hostility to science shown by the practical agriculturist, and considered the reasons for this attitude.

In other countries national systems of agricultural education and research have been founded by the State. It is hopeless to look to local effort and support, because the practical man expects immediate results, and results out of all proportion to the time and money expended in obtaining them. The time has come when the State must take part in the work and devote to it much larger sums than at present.

Mr. Thiselton-Dyer said that the matter had been carefully considered by the last Government. It was difficult, however, to persuade the Treasury that agriculture was entitled to receive special aid of a kind not given to any of our other great industries, such as iron and textiles. Personally he looked to individual effort and munificence to supply what was needed.

Prof. Marshall Ward pointed out that it was of extreme importance that the results of any investigations should be made known at once and accurately to the practical man, and this was work which might very well be undertaken by Government, but he deprecated any direction or control from a Government department in any matters of original research.

Prof. J. R. Green pointed out the necessity for investigations on vegetable physiology, as bearing on the growth of crops.

Sir Douglas Galton agreed with Mr. Dyer that agriculturists must look to themselves for help, rather than to the Government. The obtaining of really good teachers was the great difficulty.

Lord Walsingham spoke of the difficulty in producing crops which would realise a profit. Wheat-growing was unprofitable in England, and his own attempts to grow tobacco were frustrated by the heavy duty.

Sir J. Evans and Sir H. Roscoe spoke of the work of the County Councils, and Prof. Perceval gave an account of the courses at Wye College.

Mr. J. Long considered that schools and colleges for boys and youths and demonstration plots for adult farmers were the best means of bringing home the benefits arising from the application of science to agriculture.

Mr. J. R. Dunstan, in a paper on the subject under discussion, contended that courses of lectures were necessary as pioneer work. Unless farmers have a general knowledge of the principles of science, they cannot really understand the results of experiments.

Prof. Liveing advised the co-operation of County Councils in maintaining a central experimental station. He described the system of agricultural teaching adopted at Cambridge.

Mr. Avery gave some account of the agricultural side attached to the Ashburton School in Devon, and spoke of the difficulty of obtaining pupils.

Mr. T. S. Dymond emphasised the necessity of a knowledge of scientific principles, if farmers were to properly understand experimental results.

Mr. C. H. Bothamley considered agricultural sides to secondary schools much better than schools restricted to farmers' sons. The value of demonstration plots, as distinguished from experimental plots, was very great.

Prof. Warington, in reply, remarked that the whole agricultural position was such that if anything was to be done, it must be done at once, they could not afford to wait.

Mr. T. B. Wood gave an account of work at the experimental plots in Suffolk and Norfolk. The experiments in Suffolk are conducted at two stations with soils typical of large areas in the neighbourhood, viz. at Higham, where the soil is thin and light with a chalk sub-soil, and at Lavenham, where it is a much deeper loam. The experiments at both stations consist in the growth of various crops in rotation with various manures. Each year a report of these experiments is printed and circulated,

during the summer, lectures and demonstrations are given on the plots. In Norfolk there are no definite fixed stations, but the use of land has been granted by farmers for experiments on the effect of manures on crops grown in the ordinary course of farming. Feeding experiments have also been conducted.

A paper from Prof. H. W. Vogel was read, in his absence, by the Secretary, dealing with the history of the development of orthochromatic photography. Photographs were shown illustrating the advantages of the use of eosin-silver as a sensitiser, the plates being more sensitive to the yellow rays than plates prepared with ordinary eosin.

Mr. C. H. Bothamley read a paper, illustrated by lantern slides and specimens, on the sensitising action of dyes on gelatino-bromide plates. The manner in which the dye acts was discussed, experimental evidence being given against Abney's view that an oxidation product, formed by the action of light on the dye, is the active agent in assisting the reduction of the silver bromide by the developer. The probabilities appear more in favour of Eder's view that the dye or sensitiser absorbs the energy of the light waves, and passes that energy on to the silver bromide with which it is associated, the silver bromide being thereby decomposed, and the so-called latent image being formed.

In reply to questions by Lord Rayleigh, Dr. Kohn, and Dr. Harden, Mr. Bothamley said that, so far as he was aware, photo-chemical action is always preceded by the absorption of light-waves, and in the case of colourless substances it is the ultra-violet rays that are absorbed and do the chemical work. Although the quantitative composition of the latent image is not known, we have, as a matter of fact, considerable knowledge as to its properties. There is no difficulty in determining the absorbing action and the sensitising effect on two contiguous strips of the same plate, and therefore under strictly comparable conditions. No relation can be traced between the fluorescence of a dye and its sensitising action.

The report of the Committee for investigating the action of light upon dyed colours was read by the President. With some few exceptions, all the available red, orange, and yellow colours, as applied to wool and silk, have now been exposed. (Tables are appended giving the general result of the exposure.) As before, it is found that many natural dye-stuffs are by no means so fast as is generally supposed, and are exceeded in this respect by artificial colouring matters.

Two papers on organic chemistry were contributed by Dr. J. J. Sudborough. In the first paper, the author describes the preparation of a monochloro-stilbene from deoxy-benzoin, differing from that described by Limin, as it is a solid, crystallising from alcohol in large colourless plates. An oily compound, corresponding to that of Limin, has been prepared, and is being further investigated. Other stilbene derivatives are described.

In a note on the constitution of camphoric acid, the author draws attention to the fact that, as regards its etherification, camphoric acid shows a marked resemblance to some of the polycarboxylic acids investigated by Victor Meyer and Sudborough, and to hemi-mellitic acid. The formulæ of Armstrong and of Bredt are regarded as best agreeing with the behaviour of camphoric acid in this respect.

Mr. H. J. H. Fenton gave an account of the preparation and properties of a new organic acid obtained by oxidising tartaric acid under certain conditions in presence of a ferrous salt. It can be obtained by the oxidation of moist ferrous tartrate in the air, and it is found that this reaction is much accelerated by light. The acid has been isolated, and proves to be a dibasic acid having the formula $C_4H_4O_6 + 2H_2O$. It gives a beautiful violet colour with ferric salts in presence of alkali. The constitution of the acid is under investigation. Heated with water it is resolved into carbon dioxide and glycollic aldehyde, the latter substance polymerising to form a sweet-tasting solid having the formula $C_6H_{12}O_6$.

The Committee for investigating isomeric naphthalene derivatives report that the fourteen isomeric tri-chlor derivatives have been obtained.

Dr. H. Wakemann read two papers on physical chemistry. In the first, experimental evidence was quoted, showing the validity of Van 't Hoff's constant, Dalton's law, &c., for very dilute solutions. In the second paper, on the velocity of reactions before perfect equilibrium takes place, an attempt was made to calculate constants of equilibrium from experiments made by others on the rate of acidification of phosphorus and other substances.

Messrs. C. F. Cross and C. Smith contributed a paper on the chemical history of the barley plant. The work had been carried out during the two years 1894 and 1895 on the experimental plots at Woburn, and the general conclusions drawn were that the conditions of soil nutrition had very little influence upon the composition of the plant; that the straw grown in wet seasons had a high feeding value and conversely a low paper-making value; and that the compounds known as furfuroids were continuously assimilated to permanent tissue in a normal season, but in a very dry season the permanent tissue is drawn upon by the growing plant for nutrient material which is ordinarily drawn from the cell contents.

THE RETIREMENT OF PROFESSORS.

THE report of the Committee appointed by the Treasury to consider the question of the desirability of a fixed age for the compulsory retirement of professors serving under the Crown has been recently published as a Parliamentary paper. The Committee consisted of Lord Playfair, Lord Welby, and Sir M. W. Ridley, M.P. Mr. C. L. Davies was secretary. The report, which is addressed to the Lords Commissioners of her Majesty's Treasury, is in the following terms:—

We have taken the evidence of presidents and professors of the Queen's Colleges in relation to their retirement upon superannuation at fixed ages, as determined by the Order in Council of August 15, 1890. We are of opinion that the Commission of 1888, upon the report of which, to some extent, that Order in Council was based, did not intend that the limitations of age applied to Civil servants generally should be deemed applicable to presidents and professors of colleges, who are appointed and serve under different conditions from those which prevail in the Civil Service.

These presidents and professors are appointed at a maturer age, and have, by the nature of their employment at seats of learning, less tendency than Civil servants to become inefficient at the age of sixty-five. Indeed, up to that age it is often found that their efficiency increases, by experience in teaching, as their age progresses, though undoubtedly a time does arrive when advancing age weakens the receptivity of the professor to new discoveries in science, and diminishes the inclination to alter his instruction in order to adapt it to these changes. When this occurs the students are the sufferers. In the German Universities this well-known degeneration of intellectual activity among the aged is partly compensated by the appointment of active young "extraordinary professors," who, though not on the ordinary staff of the colleges, are allowed to give competing lectures within their walls. In Edinburgh an extra-mural competition is encouraged, and in each Scotch University, when professors show diminished efficiency through age, it is the duty of the University court to superannuate the professor under a pension scheme, which is charged upon a fixed Parliamentary vote for all the Scotch Universities. The Queen's Colleges in Ireland are in a different position, for they are only to a small extent dependent upon votes in Parliament, being mainly supported out of the Consolidated Fund. They are, in consequence of this peculiarity, in more intimate connection with the executive Government, with which the presidents are in frequent communication as to the working of the college and the efficiency of the professors, who are appointed by the Crown and can be dismissed by the Crown. The statutes which govern the Colleges also emanate from the Crown, and are not, like those of other colleges, the product of academic autonomy.

Under these circumstances, we are of opinion that there should be fixed rules as to superannuation of presidents and professors, and that they should be made by college statutes and not by an Order in Council.

We are of opinion that when a professor reaches sixty-five years of age the president of the college should be bound to report to the Government the condition and efficiency of the teaching. If these are and continue to be satisfactory, the professor need not be superannuated till seventy, but at this age his retirement should be absolute.

In regard to presidents, we are of opinion that the age of seventy should be the period of retirement, but, should the visitors of the college formally report that the college would suffer by the loss of the experience which the president has acquired, we think that the Treasury, and not the Irish Office, should have power to continue him as president for a certain

number of years not exceeding five, so that at the age of seventy-five the retirement of a professor should be absolute.

We are quite aware that there are cases where professors at seventy and presidents at seventy-five are fully competent to discharge their duties, but the advantages derived from superannuation would be seriously diminished if, to meet these rare cases, there were uncertainty in regard to the application of a general rule. We have observed with regret that the *alumni* of the Queen's Colleges do not seek to go back to them as professors, and it was explained to us that one reason for this is that it is useless for them to prepare for a professorial career in these colleges while so much uncertainty prevails as to when the chairs will become vacant.

We also took the evidence of Profs. Lockyer and Rücker as to the conditions which prevail in the Government School of Science at South Kensington, and we found that the age of seventy for professors was considered a proper age for retirement under ordinary circumstances.

In our opinion, as the professors are not appointed till middle life, the addition of seven years to their period of service in calculating the amount of their superannuation obviously tends to secure eminent specialists as candidates for office. The power of voluntary retirement at the age of sixty has also much to commend it in this sense.

We have the honour to be
Your Lordships' obedient servants,
PLAYFAIR.
WELBY.
August 5, 1895. M. W. RIDLEY.

The report is followed by the minutes of evidence taken on June 17, 18, and 19, during which nine witnesses were examined.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

WE learn from *Science*, that Prof. Strahl, of Marburg, has been called to the chair of Anatomy in the University of Giessen, Prof. Hans Lenk, of Leipzig, to that of Geology in the University of Erlangen, and that Dr. Haecker, of Freiburg i.B., and Dr. v. Dalla-Torre, of the University of Innsbruck, have been made assistant Professors of Zoology.

PROF. CHAPMAN having resigned the professorship of Geology and Mineralogy in the University of Toronto, that chair is now vacant.

ACCORDING to *Science*, the conditions attached to the bequest made by the late Sir William Macleay to the Sydney University, to found a chair of Bacteriology, are such that the University has decided to decline the bequest. The money will therefore revert to the Linnean Society of New South Wales, to maintain a bacteriologist, who will carry on bacteriological investigations and also take pupils.

THE Examinations for the Royal Agricultural Society's Junior Scholarships have been fixed to take place on November 12 and 13 next, at the schools of candidates and at the Society's house, 13 Hanover Square. Ten scholarships of £20 each are thrown open for competition by candidates between the ages of fourteen and eighteen, and the subjects of examination comprise: (1) The Principles of Agriculture, especially with reference to the rotation of crops, the nutrition of plants and animals, and the mechanical cultivation of the soil; (2) Chemistry as applied to Agriculture; (3) Elementary Mechanics as applied to Agriculture; (4) Land Surveying. The latest date for receiving entries is October 15.

THE following courses of Gresham Science lectures are announced: "Physic," by Dr. Symes Thompson, on October 8 to 11; "Astronomy," by Rev. E. Ledger, on October 22 to 25; "Geometry," by Mr. W. H. Wagstaff, on November 19 to 22. The lectures will all be delivered at six o'clock in the theatre of Gresham College, E.C.

THE London Society for the Extension of University Teaching announces that, in co-operation with the Royal Geographical Society, arrangements have been made for the delivery at Gresham College of a course of twenty-five lectures by Mr. H. J. Mackinder, on "The Principles of Geography." The course is specially arranged for pupil teachers, and the Sessional Certificate, granted in connection therewith, will carry marks at the Queen's Scholarship Examination. The lectures will be given on Monday evenings at six o'clock, beginning October 7.

AT the City of London College, Moorfields, a course of twenty-five lectures on "The History of Chemical Discovery" will be delivered, under the auspices of the London Society for the Extension of University Teaching, by Prof. W. Ramsay, F.R.S. The course will be begun on Tuesday evening, October 8, at eight o'clock, and be continued weekly.

ON Tuesday evening, October 1, Sir Henry E. Roscoe will preside at a meeting at the Royal Victoria Hall, Waterloo Bridge Road, when the presentation of certificates to students of the Morley Memorial College will take place. The lecture arrangements at the Royal Victoria Hall for the month of October are as follow: On the 8th, Mr. W. P. Bloxam will lecture on "Combustion"; on the 15th, Dr. W. D. Halliburton will lecture on the "Human Brain"; and on the 22nd, Mr. P. J. Hartog will lecture on "Lavoisier."

SCIENTIFIC SERIALS.

American Journal of Science, September.—Distribution and secular variation of terrestrial magnetism, by L. A. Bauer. Starting from the supposition that the earth is magnetised symmetrically to its axis of rotation, the author shows that the chief cause of distortion of this primary field can be represented as due to a secondary polarisation approximately equatorial in direction. Of these two systems, the polar systems would have to be five or six times stronger than the equatorial. Since, in going round the earth along a geographical parallel of latitude, the deflections due to the secondary system almost balance each other, the inference might be drawn that the secondary field is in some way connected with the earth's rotation.—Relations of the diurnal rise and fall of the wind in the United States, by Frank Waldo. For January the rise of wind towards the mid-day maximum is followed by a more rapid fall over nearly the whole of the United States. For July the same law holds, except in the Western States, where the morning rise is more rapid. As regards the time during which the wind rises, this is about seven hours in the Mississippi valley. On the Atlantic coast there is a decrease from ten hours in the north to five hours on the coast of Florida.—The rate of increase varies from 0·4 to 0·6 miles per hour.—Native sulphur in Michigan, by W. H. Sherzer. During the past year interesting deposits of sulphur have been discovered in the Upper Helderberg limestone, of Monroe County, Michigan. The sulphur bed lies from sixteen to eighteen feet below the surface between a compact, dolomitic limestone and a calcareous sand rock. The sulphur generally occurs in bright lustrous masses towards the centre of the cavity, intermatted frequently with the above minerals. Fragments as large as a fist are readily removed. Some of the smaller cavities contain nothing but sulphur, and one was found filled with selenite crystals. About an acre of this bed had been removed when the locality was visited, and from this the superintendent estimated that one hundred barrels of pure sulphur had been obtained.

Wiedemann's Annalen der Physik und Chemie, No. 8.—Simple objective presentation of the Hertzian reflection experiments, by Victor Biernacki. The author places one of Lodge's "coherers" in the focal line of the secondary mirror. Under these conditions, mirrors with a length as small as 45 cm. and an aperture of 30 cm., with a focal length of 3 cm., exhibit the reflection phenomena well. The coherer employed is a horizontal glass tube filled with copper filings, whose resistance is reduced as soon as electric oscillations impinge upon it. The polarisation experiment is easily performed with a tiled wall, which behaves as a transparent solid to the electric rays. A striking experiment analogous to the introduction of a doubly-refracting crystal between two crossed nicolls is the introduction of a thick slab of good ice between the two crossed mirrors, with its axis of 45° to both the focal lines. The galvanometer connected with the coherer, which before was motionless, now gives a distinct reflection, thus showing the doubly-refracting nature of ice.—A convenient method for showing the electric refractive powers of liquids, by P. Drude. For this purpose, strong oscillations are necessary. These may be produced by a modification of Blondlot's arrangement, using an exciter without a condenser, whose total length is slightly smaller than half the wave-length required. The wave-lengths in water and other

...s are obtained by conducting the parallel wires through a long trough filled with the liquid. A bridge is put across them where they enter the water. Another bridge is placed on the wires in air on the other side of a Zehnder tube connected with a gold-leaf electroscope. This is shifted until the gold-leaves collapse. The distance between the two bridges is then, say, 30 cm. The bridge on the water's edge is then gradually shifted along the immersed wires, and the points at which the gold-leaves diverge and collapse are noted. The distance between two successive nodes is 4 cm., so that the refractive index of water for electric waves is 9, and the specific inductive capacity 81. Alcohol, glycerine, and other not very highly conducting liquids may be similarly investigated.—Inconstancy of spark potential, by G. Jaumann. The author shows that the potential which leads to a spark discharge depends upon several elements besides the thickness and nature of the dielectric, the chief one being the presence of variations of electric force, which hasten the discharge and lower the necessary difference of potential. When these variations are avoided, differences of potential amounting to several times the ordinary ones may become necessary for discharge. The spark gap is also affected by previous sparks and by a delay in discharging.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 16.—M. A. Cornu in the chair.—A memoir by M. F. V. Maquaire, on protection against naval collisions, was referred to a Committee.—The perpetual Secretary, in presenting vol. vi. of "Œuvres de Christiaan Huygens," reminded the Academy of the loss of M. Bierens de Hahn. The Haarlem Society will continue the publication of this work, so ably edited by M. Hahn.—On the "équilatères" included in the equations $O = \Sigma_1^{2n} - {}^2Z_1T_1^n \equiv H_n$, $O = \Sigma_1^{2n} - {}^1Z_1T_1^n \equiv H_n + \lambda H'_1$, by M. Paul Serret.—Researches on Algerian phosphates. The case of a phosphatic rock from Bougie, having the composition of a superphosphate, by MM. H. and A. Malbot. The results of a number of analyses of rocks from various sources are tabulated. The Bougie rock is described in detail, as it presents several peculiarities. With regard to the method of analysis, the conclusions are drawn: (1) The presence of organic matter may produce a loss when the phosphoric acid is estimated by direct precipitation as magnesium ammonium phosphate in citric liquor, and this error is not always diminished by a preliminary evaporation with nitric acid on the sand bath. (2) The same error does not occur if the phosphoric acid be first precipitated as ammonium phosphomolybdate. (3) The agreement between the two methods is exact when the organic matter is first destroyed by calcination at a red heat.—The neoformation of nerve cells in the brain of the monkey, following the complete ablation of the occipital lobes, by M. Alex. N. Vitzou, of Bucharest. A detailed account is given of the gradual recovery of the power of perceiving external objects by a monkey after complete ablation of the occipital lobes. An examination revealed the fact that the space formerly filled by the occipital lobes had been filled up by new tissue which was found to consist throughout of pyramidal nerve cells and nerve fibres, the cells being less numerous than in the ordinary occipital lobes of the adult. The new tissue was not due to hypertrophy of the anterior lobes. On repeating the ablation the monkey again lost the power of perceiving external objects, and is still under observation.—M. Ch. V. Zenger records in a note the occurrence of atmospheric disturbances at certain points in Central Europe on September 10 and 11, as predicted by him.

NEW SOUTH WALES.

Linnean Society, July 31.—Mr. Henry Deane, President, in the chair.—Catalogue of the described Coleoptera of Australia. Supplement, part i. *Cicindelidæ* and *Carabidæ*, by George Masters. It is proposed to give as far as possible a complete list of all the Australian Coleoptera described since the year 1886, also to fill in the omissions previous to that date. The present part contains references to 429 species, besides many corrections, and additional localities.—Australian *Termitidæ*, part i., by W. W. Froggatt. The author gives an account of the distribution of *Termites* in general and of the damage done by them, and passes on to a consideration of the habits and range of Australian forms, concluding with a general account of the structure of the termitaria of both the common mound-building species, and of those of *Eutermes* which form arboreal nests as well as on the ground. (*a*) Report on a fungus (*Meliola amphitricha*, Fries.) on *Dysoxylon*. The fungus is found on the leaves of *Dysoxylon rufum*, Benth., on the Richmond River, N.S.W., and has not previously been recorded for this colony. (*b*) Notes on *Uromyces amygdali*, Cooke—a synonym of *Puccinia pruni*, Pers.—Prune rust. This leaf rust is of great economic importance, since it attacks such valuable fruit trees as peach and nectarine, plum and apricot, cherry and almond, causing them prematurely to shed their leaves, and as a consequence, either to bear no fruit, or only small quantities of an inferior kind. Though sometimes called "Peach Yellows," it must not be confounded with the dreaded disease, due to bacteria, known by that name in the United States. Specimens of affected peach leaves, forwarded by Mr. Tryon from Queensland, yielded both uredospores and two-celled teleutospores. In Victoria in the summer season, even as late as July, only the uredospores are at all common. (*c*) Groundsel rust, *Puccinia erechtitis*, McAlp., with trimorphic teleutospores. The æcidial stage is common on groundsel; but this is the first record for teleutospores in Australia. The rust is identical with that on *Erechtites*, described last year. The specimens were procured at Hobart, Tasmania.—By D. McAlpine, Government Pathologist, Melbourne. (Communicated by J. H. Maiden). On a new species of *Elæocarpus* from Northern New South Wales, by J. H. Maiden and R. T. Baker. A large tree (height 80–100 feet, and a trunk diameter of 2–3 feet as seen), found on the Brunswick River. The affinities of this species lie between *E. sericopetalus*, F.v.M., and *E. ruminatus*, F.v.M.; it differs from these two species in the number of stamens, lobed petals, bracts, and fruits. It is named in honour of Mr. William Baeuerlen, Botanical Collector to the Technological Museum, Sydney.—On a new cone from the Solomon Islands, by John Brazier.

BRISBANE.

Royal Geographical Society of Australasia. Annual meeting, July 22. Mr. J. P. Thomson, President, in the chair.—The Secretary, Mr. J. Fenwick, read the yearly report of the Council, which stated that during the year sixteen ordinary members had been added to the roll of the Society. The library had received some valuable donations and exchanges, and the finances of the Society were in a satisfactory condition. The President read an address on the subject of the physical geography of Australia, after which the election of officers took place.

CONTENTS.

THURSDAY, OCTOBER 3, 1895.

RITTER'S "ASIA": RUSSIAN ADDENDA.

Eastern Siberia, including Lake Baikal and the Mountains on its North-Western Shore. Vol. II. By P. P. Semenoff, I. D. Cherskiy, and G. G. von Petz. Pp. 630. (Russian: St. Petersburg, 1895.)

THIS new volume, edited by P. P. Semenoff, from the MSS. of I. D. Cherskiy, and containing 630 pages of text, in lieu of the three paragraphs of Ritter's work, is even more interesting than the preceding volume, which was noticed in these columns a short time ago (NATURE, vol. l. p. 471). It covers Lake Baikal and the mountains along its north-western shore, and embodies explorations either entirely new or quite unknown even in Russia itself. Moreover, all that has been said concerning the preceding volume, as regards the masterly treatment of the subject and a strict adherence to Ritter's excellent methods—a combination of a minute description of details with broad generalisations drawn out of them—fully applies to this new instalment of the great work undertaken by the Russian Geographical Society. A third volume, containing Transbaikalia and the Gobi, will soon follow—the invaluable collaboration of M. Obrucheff having been secured for this purpose by the editor.

When we cast a glance upon a good orographical map of Asia (*e.g.* Petermann's, in Stieler's "Hand Atlas," or even in the miniature "Taschen Atlas" of the same publishers), we see that the two great plateaus of West and East Asia are fringed along their north-western borders with a chain of great lakes: the Caspian Sea, Lake Balkhash, Ala-kul and Zaisan, Ulungur, Baikal, and Oron; while a succession of large post-Tertiary lakes, now desiccated, which formerly filled the valleys of the Tian-shan, the Altais, the Sayans, and the Muya ridges, complete this chain of depressions along the outer border of the plateaus. Lake Baikal is one of the lakes of this chain—a small remainder only of the great mass of water which formerly filled up the valley of the Irkut, and the lower parts of the eastern tributaries of the present lake, and discharged its waters, as we now learn from the volume under review, through the narrow gorge pierced by the Irkut through the Tunka Alps, by means of which it now joins the Angara at Irkutsk. At that time, *i.e.* during the post-Tertiary period, its level stood, as shown by the lake deposits and terraces explored by Cherskiy, at least 928 feet above the present level of Lake Baikal, which now lies 1561 feet above the sea level.[1]

However, even in its present limits, Lake Baikal occupies the sixth place among the largest lakes of the globe (after Lake Tanganika), and the first place among the Alpine lakes. Sufficient to say that it covers 15,300 square miles, and that the two extremities of the crescent which it makes on a map are 380 miles distant from each other. As for its depth it stands foremost. Already Kononoff's soundings, in 1859, indicated a depth of 5621 feet, and when the Polish exiles, Dr. Dybowski and Godlewski, made, in 1867 and 1871–76, a series of very accurate soundings, they revealed the existence of several valleys in its bottom, attaining depths of 2197, 4460, and 4503 feet, the greatest depths being located in the proximity of the north-western shore, so that a depth of 1935 feet (374 feet below the level of the ocean) was found within a thousand metres from the coast.

Both in its position at the foot of, and the manner it penetrates at its southern extremity into, the plateau, Lake Baikal offers a striking analogy with the Caspian Sea. The same analogy appears in its relations to the surrounding mountains. It is divided about its middle by a submerged ridge, which appears on the surface in the Olkhon Island, and in the promontory of Svyatoi Nos; and of the two basins thus formed, and named respectively the "Great Sea" and the "Small Sea," the southern, that is the one which lies nearest to the plateau, is the deepest. In older works, and in some recent ones as well, Lake Baikal used to be described as a longitudinal valley between two parallel chains of mountains; but it is evident, from what has just been said, how false this view is. The next step would be to consider it as originated from two lakes which once occupied two longitudinal valleys, and joined together after the dividing ridge had been partially destroyed by geological agencies; and this hypothesis, too, has been advocated. Things appear, however, to be much more complicated than that. When I was working out a general scheme of the orography of Siberia, I was compelled to recognise that even the two-valleys-hypothesis could not interpret the real features of the region, and although at that time (1872) we knew next to nothing about the geological structure of the Baikal mountains, I was induced, by considerations about the structure of the plateaus, their border-ridges, and the Alpine chains parallel to the latter, to draw two chains across the northern part of the lake. From the volume under review, we now learn the real state of affairs. In all his explorations in Siberia, Cherskiy used to pay a great deal of attention to the orographical features as they *now* appear to the explorer, and tried to discriminate in how far they were a result of structural features—foldings of the rocks and so on—and in how far they were derived from subsequent erosion which has been going on in these parts of Siberia since the Silurian and Cambrian periods, when the mountain ridges and plateaus received their first shape. As regards the Baikal mountains, it now appears that there is, on the north-western shore, a real ridge running parallel to the shore, and separated by a valley from the mountains lying further west; but that both this ridge and the deep hollow of the Baikal are due, not to structural, but to erosion processes. The ridge consists of slates and gneisses *crossing* it in a diagonal direction, and these strata cross also the northern part of the lake in the same direction—the direction I had indicated on the orographical map on merely theoretical grounds—so as to reappear in the same succession on the eastern shore. The foldings of the Baikal Mountains date from the Silurian, Cambrian, or perhaps even the Laurentian period (Devonian red sandstones lie undisturbed at the outer footings of the Baikal Mountains), but subsequent

[1] There is still a certain uncertainty, perhaps of over 100 feet, concerning the altitude of the level of Lake Baikal. A levelling across Siberia had been made a few years ago; but the death of the person who undertook the calculation of the results brought about some confusion, and Russian geographers suppose that some considerable error may have crept in in the levelling between the Yenisei and Irkutsk, and consequently in the above figure.

A A

erosion and denudation have modified the primary features on a gigantic scale; and a valley so deep as the northern part of Lake Baikal is, has been dug out *across* the former direction of the chains. The lake is thus an immense erosion valley which only partially has been determined by the structural valleys at the foot of the plateau, but has received its final shape through erosion, which made several parallel lakes coalesce as the mountains once separating them were pierced through and obliterated.

This instance will already give an idea of the interest which attaches to the volume now published, and the wealth of data which will be found in it. We sincerely desire, in the interests of geography, that at least these new volumes of the series should be rendered accessible to West European geographers.

The described region is very thinly populated, and contains but few explored remains of the past. As to its flora, it has been properly explored only on the Olkhon Island. The little, however, which is known in these two directions is well summed up, and will give a sound basis for ulterior exploration. We hope to find in the forthcoming volume a summary of all that is known about the fauna of the lake. P. K.

APPLICATIONS OF BESSEL FUNCTIONS.

A Treatise on Bessel Functions and their Applications to Physics. By Andrew Gray, M.A., and G. B. Mathews, M.A. (London: Macmillan and Co., 1895.)

THIS book, like the kindred work of Prof. Byerly on "Fourier's Series and Spherical Harmonics," marks the modern system of mathematical treatment, and may be contrasted with Dr. Todhunter's "Functions of Laplace, Lamé, and Bessel," of twenty years ago. At that time it was considered desirable to develop the purely mathematical analysis quite apart from the physical considerations to which it owed its life and interest; keeping the pure and the mixed mathematics in separate water-tight compartments, so to speak, with an impenetrable bulkhead between.

But as the Bessel function, like every other function, first presented itself in connection with physical investigations, the authors have done well to begin, on p. 1, with a brief account of three independent problems which lead to its introduction into analysis, before entering upon the discussion of the properties of the Bessel functions.

These three problems are: the small oscillations of a vertical chain, the conduction of heat in a solid cylinder, and the complete solution of Kepler's problem by expressing radius vector, true and excentric anomaly in terms of the mean anomaly.

It is very extraordinary that Kepler's problem should, as a general rule, be still left unfinished in the ordinary treatises, considering that the Bessel function is implicitly defined in the equation; but we need go back only twenty-five years, and we find Boole's "Differential Equations" ignoring the Bessel Function and the solution of the general Riccati equation which it provides. In those days it was customary to speak of any solution, not immediately expressible by algebraical or trigonometrical functions, as "not integrable in finite terms"; an elliptic integral was skirted round with the remark that it was "reducible to a matter of mere quadrature," and even the homely hyberbolic functions were tabooed.

String is the favourite material of the mathematician for illustrating catenary properties; but it is a relief to find that the authors have provided a *chain* for the discussion of the oscillations when suspended in a vertical line. The banal word *string* turns up accidentally two or three lines lower down (line 10, p. 1), but if a piece of string is used by the side of a length of fine chain, such as is now purchasable, the unsuitability of the string, by reason of its lack of flexibilty and its kinkiness, for the representation of catenaries and their oscillations, is at once manifest.

The small plane oscillations of the chain about its mean vertical position are of exactly the same character as the slight deviations from the straight line due to spinning the chain from its highest point of suspension; and this procedure has the advantage of showing a permanent figure, similar to that given for $J_0(\sqrt{x})$ on p. 295 of Lamb's "Hydrodynamics"; with a little practice the knack of producing one, two, three or more nodes at will is easily attained. Thus with a piece of chain 4 feet long, the number of revolutions per second should be 0·54, 1·24, 1·95, 2·65, &c.

The Bessel function was first introduced by the inventor for the complete solution of Kepler's problem, namely, to express the variable quantities in undisturbed planetary motion in terms of the time or mean anomaly $\mu = nt + \epsilon - \varpi$.

The authors avoid the awkward integration by parts employed by Todhunter in determining the excentric anomaly ϕ by means of a differentiation. Another procedure will give a/r, where a denotes the mean distance and r the radius vector, more directly, from the relation

$$\phi = \mu + e \sin \phi.$$

For differentiation with respect to μ gives

$$\frac{d\phi}{d\mu} = \frac{1}{1 - e\cos\phi} = \frac{1 + e\cos\theta}{1 - e^2} = \frac{a}{r} = 1 + \Sigma \mathrm{B}_r \cos r\mu,$$

suppose, when expressed in a Fourier series, and then

$$\mathrm{B}_r = \frac{2}{\pi}\int_0^\pi \cos r\mu \frac{d\phi}{d\mu} d\mu = \frac{2}{\pi}\int_0^\pi \cos r(\phi - e\sin\phi) d\phi = 2\mathrm{J}_r(re),$$

according to Bessel's definition.

An integration now gives

$$\phi = \mu + 2\Sigma \frac{\mathrm{J}_r(re)}{r} \sin r\mu$$

and

$$\sin\phi = \frac{\phi - \mu}{e} = 2\Sigma \frac{\mathrm{J}_r(re)}{re} \sin r\mu ; \text{ \&c.}$$

Chapters ii.-ix. are devoted to the purely analytical development of the Bessel function, considered as the solution of a differential equation, as an algebraical or trigonometrical series, or as a definite integral; these are the earlier chapters for which the authors apologise in the preface as appearing to contain a needless amount of tedious analysis. In Prof. Byerly's treatise the requisite analysis is introduced in small doses, and only as required; but the ordinary mathematician loves to strew the path at the outside with difficulties best kept out of sight; thus, as Heaviside remarks, the too rigorous mathematician tends to become obstructive. It is of

course reassuring to know that the functions employed in the physical applications, rest on a sound analytical basis, and that the convergency of the series has been carefully examined. But there is no compulsion to follow these demonstrations, tedious to all but pure mathematicians; so we can pass on direct to Chapter x., where the physical interest is resumed, under the head of "Vibrations of Membranes," for instance the notes produced on a circular drum-head. Lord Kelvin's oscillations of a columnar vortex, Lord Rayleigh's waves in a circular tank, and Sir George Stokes's investigation of the drag of the air in pendulum vibrations, make up an interesting Chapter xi. on Hydrodynamics.

Chapter xii. deals with the steady flow of electricity or of heat, and Chapter xiii. with the fascinating and novel phenomenon of Hertz's electromagnetic waves, when propagated along wires, in which problem the Bessel function assumes an essential importance.

The Diffraction of Light, considered in Chapter xiv., contains important applications of the Bessel functions; the hydrodynamical analogue would be the investigation of the effect of a breakwater in smoothing the waves which bend round behind into its shelter; for instance, the effect of the Goodwin Sands on the safe anchorage in the Downs.

Newton rejected the Undulatory Theory of Light, partly because he could not understand the existence of shadows on this hypothesis, a curious effect of Newton's early ideas as a country boy; had he been brought up on the sea coast, this apparent difficulty could not have troubled him.

It would be a needless complication to consider any but straight waves in the case of the breakwater; and similarly in the Diffraction problem, the authors might have made a simplification by parallelising the incident light by passing it through a lens; or at least this special case, which is the one of practical importance in the subsequent discussion of the resolving power of a telescope, might receive separate treatment as the analysis now becomes almost self-evident. This chapter concludes with a discussion of Fresnel's integrals, required in the diffraction through a narrow slit; the integrals are expressed by a series of Bessel Functions of fractional order, half an odd integer, and are represented graphically by Cornu's spirals.

The problem of the stability of a vertical mast or tree, considered under the head of Miscellaneous Application in the last chapter, may well be amplified by examining the effect of centrifugal whirling on the stability, as in the case of the chain on p. 1; for the number of revolutions required to start instability is exactly equal to the number of vibrations which the mast or tree will make when swaying from side to side. A differential equation of the fourth order, with a variable coefficient, now makes its appearance, the solution of which will express the oscillations of the bullrushes in a stream, or the waving of cornstalks in a field. The curious appearance of permanence in the waves on a cornfield gives an illustration, analogous to Prof. Osborne Reynolds's disconnected pendulum, of a case of zero group-velocity; and by some intuitive deductions from the appearance of these waves the farmer can judge the time suitable for harvest.

The authors have been fortunate in securing an original collection of numerical tables, including those of Dr Meissel, who did not live quite long enough to see his valuable calculations published in this book.

A collection of examples adds greatly to the interest of the treatise, and will probably form the nucleus of a still larger list in the future.

Altogether the authors are to be congratulated in bringing their task to such a successful conclusion; and they deserve the gratitude of the mathematical and physical student for their lucid and interesting mode of presentment.

A. G. Greenhill.

OUR BOOK SHELF.

Protoplasme et Noyau. Par J. Pérez, Professeur à la Faculté des Sciences de Bordeaux. (Bordeaux: Imprimerie G. Gounouilhou, 1894.)

Experimental work in recent years has repeatedly shown that in plants as well as in animals the physiological *rôle* of the nucleus in the cell is one of great importance. It has been demonstrated that non-nucleated fragments of protoplasm, whether of a *Spirogyra* or an Infusorian, are incapable of growth and reproduction; while, on the other hand, fragments containing a portion of nuclear material are capable of complete recrescence. Impressed by these facts the writer of the essay before us has been led to doubt whether protoplasm can be properly regarded as the "physical basis of life," since it cannot retain its life when removed from the influence of the nucleus. Consistently with this position the writer throws doubt upon the existence of non-nucleate organisms in general. The presence of nuclei has been demonstrated in many forms once believed to be destitute of them—*e.g.* Mushrooms, marine Rhizopods, and plasmodia. There remains only Haeckel's group of Monera in which the presence of a nucleus may still be disputed. M. Pérez considers in turn each of Haeckel's subdivisions of this most artificial group. In the Lobomonera (*e.g. Protamœba*) he believes that the nucleus has been overlooked. In the Rhizomonera the nucleus has been observed in various species of *Vampyrella*; and it probably exists also in *Protomyxa*, since this form produces zoospores; the zoospores of those Myxomycetes which most resemble *Protomyxa* have been shown by Zopf to be nucleated. In the Tachymonera (Schizomycetes) the greater part of the body seems to consist of nucleoplasm, while the zooglœa may perhaps be compared with the undivided protoplasm of a plasmodium.

M. Pérez concludes that non-nucleated organisms or cytodes are creations of the imagination; that protoplasm, by which our author means cytoplasm, is not the primitive living matter, but a product of nucleoplasm; and that nucleoplasm, and not protoplasm, is the most primitive living substance known to us.

Analytical Key to the Natural Orders of Flowering Plants. By Franz Thonner. Small 8vo. pp. 151. (London: Swan Sonnenschein and Co., 1895.)

The author's apology for his little book is that few "Exotic Floras" contain artificial keys to the natural orders, even such as contain keys to the genera and species. But we imagine few persons would attempt working with a flora, exotic or native, without some preliminary knowledge of botany, and especially of the natural orders. Indeed a considerable acquaintance with the subject would be necessary to enable a person to use the present key to advantage. For example, the author begins with "ovules naked," and "ovules enclosed in an ovary," &c. Now, to be able to decide this point means a great deal, for a person who could do it would most likely know his gymnosperm without looking at the ovule

—even better without, perhaps. The next alternative is between isolated vascular bundles, and vascular bundles in a cylinder, connected with other characters, entailing previous teaching and study, which should largely consist of acquiring a knowledge of natural orders. Nevertheless this book may prove useful, especially to the collector desirous of determining the natural orders of his plants in the field or at home. So far as we have tested it, it is carefully compiled and edited, and we can conscientiously recommend it to those who know the characters of many natural orders in advance.

W. B. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Attempt to Liquefy Helium.

I HAVE received a letter from Prof. Olszewski, of Krakau, in which he informs me that having exposed a sample of helium which I sent him to the same treatment as was successful in liquefying hydrogen—namely, compressing with a pressure of 140 atmospheres, cooling to the temperature of air boiling at low pressure, and then expanding suddenly—he has been unable to detect any sign of liquefaction.

The density of helium being, roughly speaking, twice that of hydrogen, it is very striking that its liquefying point should lie below that of hydrogen. It may be remembered that argon, which has a higher density than oxygen, liquefies at a lower temperature than oxygen; and it was pointed out by Prof. Olszewski that this behaviour was not improbably connected with its apparently simple molecular constitution. The similar fact now recorded for helium may therefore be regarded as evidence of its simple molecular constitution. I use the word "its" instead of "their," although further research may corroborate Prof. Runge's contention that what is termed helium may in reality be a mixture of two, if not more than two elements. If this contention is true, both, or all, must have extraordinarily low boiling-points.

WILLIAM RAMSAY.

September 23.

Helium and the Spectrum of Nova Aurigæ.

IN the paper on the constituents of the gas in cleveite that we read before the British Association, we said that in the first spectrum of Nova Aurigæ the principal lines 5016 and 4922 of the lighter constituent were far more intense than those of the other constituent. But we were puzzled at the line 6678 not having been observed, as it is also a strong line in the spectrum of the lighter constituent. On inquiry, Dr. and Mrs. Huggins were kind enough to give us better information. Dr. Huggins writes:

"I think there is no doubt that we did see the red line at 6678 in Nova Aurigæ. We were unable to measure in that part of the spectrum, but on three nights we saw a bright line a little below C. This was a pure estimation under difficult circumstances. In the map we put the line, as a mere guess, at a little over 6700. On the first night we put the line in a rough diagram, made at the time, a little nearer C, almost exactly at 6678. On a subsequent night, we made the estimation a little below 6700, but the line was not then so bright."

London, September 27. C. RUNGE AND F. PASCHEN.

Latent Vitality in Seeds.

THERE is no doubt, as M. Casimir de Candolle has recently shown in his paper on latent life in seeds, that all the functions of seeds may remain completely quiescent for a long period; probably in some cases this period may be indefinitely long. In 1878 I published a paper[1] on the resistance of seeds, especially of *Medicago sativa*, or lucerne, to the action of gaseous and liquid chemical reagents. An abstract of my experiments was published in NATURE, vol. xxv., 1882, p. 328.

Recently I have examined portions of the seeds used in the experiments of 1877 and 1878, to see if after the lapse of so many years, during which the seeds have remained constantly surrounded by special gases, or immersed in different solutions, they had retained their vitality. The results have been remarkable, for in some cases a large proportion of the seeds have maintained their vitality after a lapse of 15, 16, and nearly 17 years of special external chemical conditions. I summarise the results of some of my experiments.

(*a*) *Experiments in Gases.*

In all these experiments the gases were *dry*, for in these conditions moisture is rapidly fatal to the seeds. The seeds were introduced into small bulbed tubes, into which the dry gas was made to pass for some time, after which the tubes were rapidly sealed at a spirit-lamp flame. The tubes were then kept in the dark.

In the following summary I give the dates of the sealing and opening of the tubes:—

Hydrogen.—Lucerne seeds, from September 15, 1877, to August 5, 1894, a period of 16 years, 10 months, and 20 days. Out of 51 seeds sown, none germinated. Seeds of wheat, vetch, *Cynara cardunculus* and coriander, kept in hydrogen, gave the same negative results. There is some suspicion that the hydrogen had not been originally well dried.

Oxygen.—Lucerne, from May 19, 1878, to August 4, 1894, 16 years, 2 months, and 15 days. Out of 293 seeds sown, 2 germinated, or 0·68 per cent. The seeds were not thoroughly dry.

Nitrogen.—Lucerne, from April 12, 1878, to August 21, 1894, 16 years, 3 months, and 22 days. Out of 320 seeds, 181 germinated, or 56·56 per cent.

Chlorine and Hydrochloric Acid Gas.—Lucerne, from April 28, 1878, to August 3, 1894, 16 years, 3 months, and 5 days. Out of 342 seeds, 23 germinated, or 6·72 per cent. Originally these seeds had been put into pure chlorine; but the gas had acted on the seeds, carbonising a portion of them, so that at the end of the experiment the seeds were in an atmosphere composed chiefly of hydrochloric acid gas, mixed with carbon dioxide.

In a second experiment with lucerne seed, kept in chlorine, and then hydrochloric acid, during the same period, out of 167 sown, 10 germinated, or 5·98 per cent. In this experiment the tube was carefully opened *in vacuo*, to protect the seeds from the moisture condensed by the hydrochloric acid gas at the moment when it is brought into contact with common air.

Sulphuretted Hydrogen.—From October 14, 1877, to August 5, 1894, 16 years, 9 months, and 22 days. After the opening of the tube, filled with the strongly smelling gas, the seeds were left in contact with the air for 24 hours, before sowing them in the moist sand of the germinator. Out of 101 lucerne seeds, one germinated, or 0·99 per cent. Out of 50 seeds of wheat, none germinated.

Arseniuretted Hydrogen.—From April 4, 1878, to August 4, 1894, 16 years and 4 months. On opening the tube the garlic smell of AsH_3 was strongly evident. Out of 255 lucerne seeds sown, 181 germinated, or 70·98 per cent. In a second experiment with seeds kept in arseniuretted hydrogen, out of 247 lucerne seeds 170 germinated, or 68·82 per cent.

Carbon Monoxide.—From April 3, 1878, to August 4, 1894, or 16 years and 4 months. Out of 266 lucerne seeds, 224 germinated, or 84·2 per cent.

Carbon Dioxide.—From September 8, 1877, to August 5, 1894, or 16 years, 11 months, and 27 days. The same tube contained seeds of lucerne, wheat, vetch, *Cynara*, and coriander. None germinated. Perhaps the large number of seeds contained in a relatively small tube rendered the carbon dioxide damp, and therefore noxious.

Nitric Oxide.—From May 2, 1878, to August 4, 1894, or 16 years, 3 months, and 2 days. On opening the tube, abundant red fumes were produced by contact with air. Before sowing, the seeds were left dry for 24 hours. Some of the seeds were brownish, the rest retained their natural colour. Out of 309 lucerne seeds, 3 germinated, or 0·97 per cent. In a second experiment, the tube containing the lucerne seeds was opened *in vacuo*: out of 320 seeds, 2 germinated, or 0·62 per cent.

[1] [illegible] "Resistenza di alcuni semi all' azione prolungata di agenti [illegible] liquidi," *Gazzetta Chimica italiana*, ix., 1879, p. 199; and [illegible] *ital.* [illegible], 1879, p. [illegible]

(*b*) *Experiments with Liquids and Solutions.*

I give only the results obtained with alcohol and alcoholic solutions. In other liquids, such as ether and amyl alcohol, the liquids had gradually evaporated, so that the exact period of their action could not be ascertained, and the seeds, covered with a moist oily varnish, had lost all vitality. Lucerne seeds kept in chloroform for 16 years and 4 months, were completely lifeless. In all the recorded experiments the seeds were completely immersed in a relatively large volume of liquid.

Strong Alcohol.—From March 26, 1878, to August 6, 1894, or 16 years, 4 months, and 13 days. The alcohol was originally absolute, but in contact with the seeds, and during so many years must have absorbed a small proportion of water. Before being sown, the lucerne seeds were carefully air-dried on a filter for 12 hours. Out of 60 seeds sown, 40 germinated, or 66·6 per cent.

Concentrated Alcoholic Solution of Corrosive Sublimate.—The alcoholic solution was originally prepared with alcohol nearly absolute, and saturated with mercuric chloride. From May 23, 1878, to August 17, 1894, or 16 years, 2 months, and 25 days. On taking the seed from the mercuric solution, they were very carefully washed with alcohol at 97 per cent. until every trace of the mercuric compound was washed away. The seeds were dried at the ordinary temperature, and then sown. Out of 79 lucerne seeds, 16 germinated, or 20·2 per cent.

Alcoholic Solution of Sulphur Dioxide.—From November 10, 1878, to August 24, 1894, or 15 years, 9 months, and 14 days. Originally the alcohol was of 93 per cent. strength; the solution preserved a suffocating odour of sulphurous acid. The lucerne seeds were mixed with minute sulphur crystals; the seeds were well washed with strong alcohol, dried and sown. Out of 645 lucerne seeds, one alone germinated, or 0·15 per cent.

Alcoholic Solution of Sulphuretted Hydrogen.—From November 10, 1878, to September 4, 1894, or 15 years, 9 months, and 15 days. The alcohol, originally 93 per cent. strength, had been repeatedly saturated with sulphuretted hydrogen gas. The liquid emitted a marked mercaptanic smell. Sulphur crystals were formed, and sedimented with the lucerne seeds. The latter were washed with 97 per cent. alcohol, and then air-dried. Out of 583 seeds, 41 germinated, or 7·03 per cent.

Alcoholic Solution of Nitric Oxide.—From November 10, 1878, to September 4, 1894, a period equal to that of the last described experiment. The alcohol, 93 per cent. strength, had been repeatedly saturated with NO. Before sowing, the seeds were washed with alcohol and dried. Out of 288 seeds, 12 germinated, or 4·16 per cent.

Alcoholic Solution of Phenol.—The lucerne seeds preserved in the solution for over 15 years, showed no signs of vitality. In washing the seeds, previous to sowing, with alcohol, they could not be completely purified from the phenol.

Many of the germinating lucerne plants developed from the seeds used in these experiments, were transplanted from the germinator into flower-pots. The plants grew well, and have flowered and seeded normally.

At the beginning of these experiments, in 1877 and 1878, I was not aware of the noxious action of even small proportions of moisture. It is probable that if in all these experiments special care had been taken at the beginning to exclude as much as possible moisture, both from the seeds and from the gases or liquids, a much larger proportion of seeds would have retained their vitality. The difficulty of preserving the vitality of large seeds must be chiefly caused, in all probability, by the difficulty of thoroughly drying them.

These experiments are of interest in showing that seeds may retain their vitality in conditions when all respiratory exchange is completely prevented for a long series of years. They fully confirm the results of the late G. J. Romanes, who proved that seeds may preserve their vitality for 15 months when kept *in vacuo*, or when transferred from the vacuum tubes to other tubes, charged with sundry gases or vapours.[1]

My experiments encourage, moreover, the suspicion that latent vitality may last indefinitely when sufficient care is taken to prevent all exchange with the surrounding medium. There is no reason for denying the possibility of the retention of vitality in seeds preserved during many centuries, such as the mummy-wheat, and seeds from Pompei and Herculaneum, provided that these seeds have been preserved from the beginning in conditions unfavourable to chemical change. The original dryness of the seeds, and their preservation from soil moisture or moist air, must be the very first conditions for a latent secular vitality.

In experimenting with seeds from Pompei and Herculaneum, I have not as yet been able to find among them any living grain. The greater part of these seeds are too much carbonised and changed to permit the entertaining of much hope as to their possible vitality. Especially among the seeds of Pompei, the carbonisation must have been caused by the slow action of moisture, which would speedily destroy all life in the seeds. Among the Pompeian wheat the destruction of organic matter has been so great as to leave in the seed, in its present condition, a proportion of ash as high, in some cases, as 4·2 per cent., and even 8·4 per cent.

On the other hand, some of these seeds, as those found in the granaries of the *Casa dell' Argo*, at Herculaneum, in 1828, seem to have been in conditions favourable to a prolonged preservation of latent vitality; the millet seeds, especially, were found unchanged in outer aspect. Unfortunately, no test was made at the time of their discovery, and since then the action of moist air, and exposure to changes of temperature and to light, must have impaired fatally any remnant of vitality still lurking amongst the seeds.

All researches on latent life are of great interest in ascertaining the nature of living matter. The present researches have established that, for some seeds at least, respiration, or exchange with the surrounding medium, is not necessary for the preservation of germ-life. It is a common notion that life, or capacity for life, is always connected with continuous chemical and physical change. The very existence of living matter is supposed to imply change. There is now reason for believing that living matter may exist, in a completely passive state, without any chemical change whatever, and may therefore maintain its special properties for an indefinite time, as is the case with mineral and all lifeless matter. Chemical change in living matter means active life, the wear and tear of which necessarily leads to death. Latent life, when completely passive, in a chemical sense, ought to be life without death.

It may be finally remarked that the proof of the resistance of seeds to vacuum, of the non-necessity of a respiratory exchange with outer air, together with the proof of the resistance in some seeds to very low temperatures, are facts encouraging the belief that the origin of life on our globe may be due to the introduction of germs that have travelled, embedded in aerolites, from other planets where life is older than upon the earth.

ITALO GIGLIOLI.

Regia Scuola Superiore d'Agricoltura,
Portici, near Naples.

To Friends and Fellow Workers in Quaternions.

SINCE the publication of Hamilton's "Elements of Quaternions," in which the great mathematician developed his new calculus with admirable skill and clearness, more than thirty years have passed away, without it finding the adequate recognition which it so highly deserves. The circumstance is still the more deplorable as the calculus has since been further developed by Prof. Tait and others.

There is, in truth, no question as to the importance of the use of vectorial quantities in physics, but on account of their apparently preponderating importance, various physicists have been led to invent new forms of vector-theory excluding the idea of quaternions. But, as far as we see, they are founded on definitions which are established by quaternions, and are systems of notation rather than logical developments of a mathematical idea.

On the other hand, many who are prejudiced against the calculus of quaternions maintain the opinion that it is hard to understand, and that it contains a great deal which is useless in addition to things immediately applicable. To the latter charge there need be no answer, since all forms of mathematics are exactly alike in this respect, and since in the very combination of the pure and the applied lies the potentiality of further development. In regard to the former objection, quaternionists need only say that if the objectors approach the calculus of quaternions with proper care and meekness, they will ere long assuredly rejoice in having at their disposal an instrument of research mightier far than they had the slightest notion of so long as they were in the domain of cartesian coordinates. Certainly it would be a blessing to science if they could accept these assertions, and their endeavours would find a

[1] NATURE, December 7, 1893, p. 140.

... in the advancement wherever this method might be applied. So much for these objections.

New notations in the calculus of quaternions must needs be invented from time to time. But since they are becoming complex (though far simpler than in cartesian coordinates) as the problems are getting more complicated, it is highly desirable already at this stage of development, to exchange opinions on the selection or adoption of new symbols.

By these and other considerations we have been led to believe that the time has come for those who are interested in vector analysis to come to the fore and join hands. In order to further this purpose, we venture to suggest the establishing of something like an "International Association for Promoting the Calculus of Quaternions." The following would be amongst its principal objects:

(1) That the members should be informed of the publications of all important papers and works respecting either the theory of quaternions or its applications; and if possible to have these made accessible to them.

(2) That the members should be afforded the means of exchanging opinions on the introduction and adoption of new notations.

In these few lines we have tried to point out the important tasks of the Association, but shall be obliged for any suggestion or improvement. All we desire is to assure to the calculus the place it deserves, and consequently to see it fully developed in its various aspects by the combined efforts of able mathematicians and physicists. It is almost needless to say that we are only preparing the way: and once the Association has been started, we shall be ready to place it in the hands of persons much more competent than ourselves to further its best interests.

We earnestly hope that all friends will appreciate our endeavours and show us at once some token of approval. We would ask those who are in Europe to communicate with the first of the names below, and those in America with the second.

P. MOLENBROEK, The Hague, Holland.

SHUNKICHI KIMURA, Yale University, U.S.A.

August 7.

P.S.—It has been suggested by friends interested in this matter to enlarge the scope of the proposed Association so as to include all systems allied to quaternions and to Grassmann's "Ausdehnungslehre." This suggestion we are in full sympathy with. The name of the Association might then be "The International Association for Promoting the Study of Quaternions and Allied Systems of Mathematics." P. M.

September 17. S. K.

Artificial Human Milk.

It is stated in NATURE of September 19, that "so far, according to Dr. Backhaus, no satisfactory substitute has been produced in the place of human milk"; and a method is then described by which he has "quite recently" succeeded in supplying the deficiency. It appears to differ little from the process first employed and made known by me in 1854, and afterwards published in my "Experimental Researches" in 1877; except that, in omitting to add the necessary amount of milk-sugar to make up for the deficiency in the cow's milk, Dr. Backhaus fails to obtain an artificial milk closely resembling the human in chemical composition.

My recipe has, since its first publication, been advantageously used in private and hospital practice by the late Prof. W. C. Williamson, by Dr. W. Playfair, and others, but it has probably not come under the notice of Dr. Backhaus.

My process is based on the fact that by the removal of one-third of the casein from cow's milk, and the addition of one-third more milk-sugar, a liquid is obtained which closely approaches human milk in composition. The following is the mode of preparing the milk, and it is so simple that any intelligent mother or nurse can easily carry it out.

"Allow one-third of a pint of new milk to stand for about twelve hours, remove the cream, and add it to two-thirds of a pint of new milk, as fresh from the cow as possible. Into the one-third of a pint of blue milk left after the abstraction of the cream, put a piece of rennet about one inch square. Set the vessel in warm water until the milk is fully curdled, an operation requiring from five to fifteen minutes, according to the activity of the rennet, which should be removed as soon as the curdling commences, and put into an egg-cup for use on subsequent occasions, as it may be employed daily for a week or two. Break up the curd repeatedly and carefully separate the whole of the whey, which should then be rapidly heated to boiling in a small tin pan placed over a spirit- or gas-lamp. During the heating, a further quantity of casein separates, and must be removed by straining through muslin. Now dissolve 110 grains of powdered milk-sugar in the hot whey, and mix it with two-thirds of a pint of new milk to which the cream from the other third of a pint was added, as already described. The artificial milk should be used within twelve hours of its preparation: and it is almost needless to add, that all the vessels employed in its manufacture and administration should be kept scrupulously clean."

In this process only one-third of the milk was sterilised; but, in the light of modern bacteriology, it is desirable to sterilise the whole by finally heating it to boiling.

The Yews, Reigate, September 20. E. FRANKLAND.

The Elements of Architecture.

HAVING been for some weeks out of the way of seeing papers, I have only just seen the review of "Architecture for General Readers" in NATURE of August 15. I ought to thank you for devoting so much space to a book which deals rather with art than "nature," and there are one or two criticisms on special points which I think are just, and which will have attention in the second edition of the book. But there are three remarks of the reviewer's on which I should like to have a word.

(1) He refers the reader to Perrot and Chipiez' work on "The Arts of Primitive Greece" for proof of the derivation of the Greek entablature from a wooden origin. In my opinion, Messrs. Perrot and Chipiez prove nothing whatever but their own ingenuity. They argue in a circle. Assuming the probability of a wooden origin for the Greek entablature, they proceed to construct out of their own inner consciousness a series of wooden structures, quite possible but entirely imaginary, in which the origin of all the features of the stone entablature is carefully provided for, and then produce an engraving of the stone (or, rather, marble) entablature to show triumphantly the result which they have been consciously leading up to all the way. You may prove anything on that kind of principle. I do not deny that the Greek entablature appears to be of timber origin. I only say it has not been proved to be so, and I am sure Messrs. Perrot and Chipiez have not proved it.

(2) The reviewer thinks I am captious in objecting to Wren's double cupola at St. Paul's as a sham, and that I might as well object to the vault which hides the interior of the tower over the crossing in a mediæval cathedral. But he misses the main point of my objection, which is that the exterior timber dome of St. Paul's is made to appear, to the eye, to carry a ponderous stone lantern which would, in fact, crush it at once, and which is really the termination of a concealed masonic construction thrusting itself through the timber dome. At Florence and St. Peter's the stone lantern is really carried by the visible dome which appears to carry it: at St. Paul's it is not, and could not be. I consider St. Paul's by far the more beautiful design of the three, but it cannot be denied that it is a constructional falsehood in that respect. (See the block section of it given on p. 99 of the book.)

(3) The reviewer objects that I have denied to Italy any specimen of true Gothic, and yet that Milan is one of the most impressive Gothic interiors in existence. This may be true as to general effect; but the detail of Milan is wretched; and it is by detail that purity of architectural style is chiefly to be judged.

H. HEATHCOTE STATHAM.

(1) MR. STATHAM objects to Perrot and Chipiez' work, on primitive Greece being cited for proof of the derivation of the Greek entablature from a wooden origin.

It seems to me that in this matter possibly the main difference between Mr. Statham and the reviewer lies in the meaning to be attached to the word *proof*. Absolute mathematical proof is seldom to be looked for in archæological or historical descriptions, and we must be often contented with a sufficiently high probability. Taking the word in that sense, it seems to me that the circle in which Perrot and Chipiez are said to argue, cannot be made to re-enter into itself.

Mr. Statham allows that the Greek entablature "appears to be of timber origin." Vitruvius (iv. cap. 2) says distinctly that it

was so. The remains of primitive architecture in Greece—particularly at Tiryns—show that wood must have entered largely into architectural constructions; amongst other evidences, the traces of wooden door-cases cannot be explained away. Perrot and Chipiez, with whatever amount of fancifulness there may be (and there is no doubt much which is altogether hypothetical) in their restorations, do come legitimately to an explanation of the Doric guttæ both under the triglyphs and beneath the mutules, as typical of the ends of wooden pegs or trenails in timber construction, which is sufficient for the argument in the review, in which there was no intention to approve Perrot and Chipiez' restorations and deductions any further than that.

(2) As to the second objection taken to the review—the remark respecting the cupola of St. Paul's. The remark in the review had reference to the objection that the external outline of the dome was distinct from the internal, and not to the question of support of the lantern; but with reference to the latter point, when the lantern of St. Peter's is quoted as supported by a more legitimate construction than that of St. Paul's, it may be asked: Why the construction of St. Peter's dome, which is absolutely dependent for its safety on the iron chains by which it is hooped together, is preferable to that of St. Paul's, where the lantern has a much securer, and therefore not less legitimate, support in Sir Christopher Wren's cone?

(3) One remark only on the objection raised to the style of Milan Cathedral. The detail is said to be wretched. That it does not conform to the canon of Northern Gothic can be readily conceded, but that the shafts of the magnificent forest of pillars which support the interior are wretchedly designed, and unsuitable to the intended effect, is not so easy to admit.

THE REVIEWER.

Do the Components of Compound Colours in Nature follow a Law of Multiple Proportions?

THIS question, put by Mr. F. Howard Collins in NATURE (p. 438), may be answered in the negative.

In practical work there is no indication of such a law. It is found that the two rays, which together produce a compound natural colour, may be in any proportions; when there is a multiple proportion, and in some cases there must be, it is only as forming part of a series of variations, such as are frequently found within the limits of a single popular colour term. How wide these proportions may be, can be illustrated by comparing them to the varying proportions of two irregular curves towards each other.

The examples of foliage quoted can only be taken as representing individual instances. Variations of climate, age, cultivation, and aspect alter the colour proportions of a given variety of leaf; indeed, such variations are sometimes found in the same leaf. JOSEPH W. LOVIBOND.

Salisbury, September 23.

IN view of the letters, recently printed in NATURE, by Mr. H. H. Pillsbury and Mr. Herbert Spencer, it may be well to state that Chevreul published an "Exposé d'un moyen de définir et de nommer les couleur d'après une méthode précise et expérimentale" (Paris, 1861, also *Mém. de l'Acad.* xxxiii.), in which elaborate charts are given showing the colours defined by a decimal system and in ten degrees of saturation.

Recently Prof. W. Hallock, of this College, has painted discs with standard colours, and determined their wave-lengths with the spectroscope. These discs were then used to study 6000 samples of coloured objects, and formulæ were determined for some 500 named colours. These formulæ have been used for defining the names of colours in the new "Standard Dictionary" (Funk and Wagnall's, New York).

J. MCKEEN CATTELL.

Columbia College, New York, September 20.

A Problem in Thermodynamics.

IT may interest some of your readers to know that the problem in thermodynamics, propounded by Mr. Blass in your number of August 29, has actually been put to the test. I pointed out Mr. Blass's letter to my brother, who is a freezing engineer, and he showed me a copy of the *Zeitschrift für die Gesammelte Kälte-Industrie* (Munich) for August, in which an account is given of a machine on exactly the principle Mr. Blass suggests, by which Herr Linde has succeeded in liquefying air. It would appear, therefore, that the "theoretical minimum of temperature produced at C" would be determined by the point of liquefaction of the gas employed: with a perfect unliquefiable gas it would, I suppose, theoretically, be absolute zero. EDWARD T. DIXON.

Cambridge, September 22.

THE NEW MINERAL GASES.

OUR knowledge of the spectra and other conditionings of the new mineral gases has received an important addition in the communication from Drs. Runge and Paschen which appeared in last week's NATURE. The employment of exposures extending over seven hours has given a considerable extension in the number of lines, and the bolometer has been called in to investigate lines in the infra-red; better still, they have employed well-practised hands in searching for series of lines. Operating, by chemical means, upon a crystal of clèveite free from any other mineral, they have obtained a product so pure that from these series there are no outstanding lines. Very great weight, therefore, must be attached to their conclusions, and there are several points of contact with the work upon which I have been engaged from a slightly different stand-point since last April, when Prof. Ramsay made his fortunate discovery of a terrestrial source of helium.

I will touch upon some of these points *seriatim*.

In the first place, there has never been the slightest doubt in my mind that it was a question of gases and not of a gas. The spectroscopic evidence in the laboratory alone was complete, and the case was greatly strengthened when the behaviour of the various lines in the sun and stars was also brought into evidence. Drs. Runge and Paschen also declare that the gas given off even by a pure crystal of clèveite is not simple, but consists of two constituents. To the one containing the line D_3, which I discovered in 1868, the name helium remains; the other for the present, we may call "gas X." The chief lines of these two constituents are as follows, according to Runge and Paschen:

Helium.	Gas X.
5876	6678
4713	5048
4472	5016
4026	4922
3889	

Last May I wrote as follows[1]:—

"The preliminary reconnaissance suggests that the gas obtained from bröggerite, by my method, is one of complex origin.

"I now proceed to show that the same conclusion holds good for the gases obtained by Profs. Ramsay and Clève from clèveite.

"For this purpose, as the final measures of the lines of the gas as obtained from clèveite by Profs. Ramsay and Clève have not yet been published, I take those given by Crookes, and Clève, as observed by Thalén.

"The most definite and striking result so far obtained is that in the spectra of the minerals giving the yellow line I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of clèveite by chemical methods is vastly different from that obtained by my method from certain specimens of bröggerite, and since from the point of view of the blue lines, the spectrum of the gas obtained from clèveite is more complex than that of bröggerite, the gas itself cannot be more simple.

"Even the blue lines themselves, instead of appearing

[1] *Proc.* Roy. Soc., vol. lviii. p. 114.

en bloc, vary enormously in the sun, the appearances being—

4922 (4921·3) = 30 times.
4713 (4712·5) = twice.

"These are not the only facts which can be adduced to suggest that the gas from clèveite is as complex as that from bröggerite, but while, on the one hand, the simple nature of the gases obtained by Profs. Ramsay and Clève, and by myself, must be given up, reasoning on spectroscopic lines, the observations I have already made on several minerals indicate that the gases composing the mixtures are by no means the only ones we may hope to obtain."

It will be seen that the laboratory separation of D_3 from the lines 5048, 5016, and 4922 was complete, and we now know that they belong to different series.

These lines have now been differentiated by Runge and Paschen by a different but equally satisfactory method.

Nor is this all. The difference between the results obtained by Thalén and myself seemed susceptible of explanation by admitting a fractional distillation, according to which D_3 and 447 came off first, and 4922, 5016, and 667 later on (Fig. 2).

Here also I got the same result as in the diffusion experiment referred to by Drs. Runge and Paschen. They found similarly—

Less bright.	More bright.
D_3	5016
	6678

All these various lines of evidence tend therefore to complexity, and there can be little doubt from the convergence of all these lines of work, the results of which agree among themselves, that we are in presence of at least two distinct gases, the complete spectra of which are those given by Drs. Runge and Paschen.

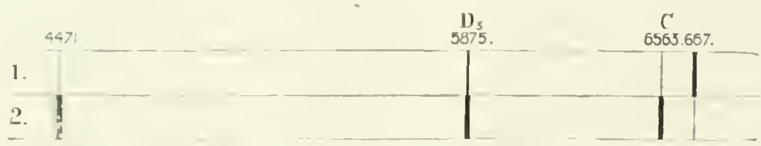


Fig. 1.—Diagram showing changes in intensities of lines brought about by varying the tension of the spark. (1) Without air-break. (2) With air-break.

Later on, in the same month, I returned to this subject, and showed that the lines at D_3 and 447 behaved in one way, and that at 667 behaved in another.

I wrote as follows[1]:—

"(1) In a simple gas like hydrogen, when the tension of the electric current given by an induction coil is increased, by inserting first a jar, and then an air-break into the circuit, the effect is to increase the brilliancy and the breadth of all the lines, the brilliancy and breadth being greatest when the longest air-break is used.

"(2) Contrariwise, when we are dealing with a known compound gas; at the lowest tension we may get the complete spectrum of the compound without any trace of its constituents, and we may then, by increasing the tension, gradually bring in the lines of the constituents, until, when complete dissociation is finally reached, the spectrum of the compound itself disappears.

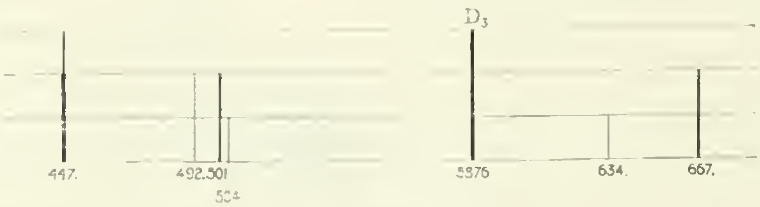


Fig. 2.—Diagram showing the order in which the lines appear in spectrum when bröggerite is heated.

"Working on these lines, the spectrum of the spark at atmospheric pressure, passing through the gas, or gases, distilled from broggerite, has been studied with reference to the special lines C (hydrogen), D_3, 667, and 447.

"The first result is that all the lines do not vary equally, as they should do if we were dealing with a simple gas.

"The second result is that at the lowest tension 667 is relatively more brilliant than the other lines; on increasing the tension, C and D_3 considerably increase their brilliancy, 667 relatively and absolutely becoming more feeble, while 447, seen easily as a narrow line at low tension, is almost broadened out into invisibility as the tension is increased in some of the tubes, or is greatly brightened as well as broadened in others (Fig. 1).

[1] Proc. Roy. Soc., vol. lviii. p. [illegible]

The second point is that there is no connection whatever between either of these gases and argon. Argon is of the earth, earthy, but helium and gas X are distinctly celestial, even more celestial than I thought when I claimed for them last May[1] the dignity of "a new order of gases of the highest importance to celestial chemistry." It was supposed at first that the spectra contained any number of common lines, next that there were two coincidences in the red between the new gases and argon; one I found broke down with moderate dispersion, the other has yielded to the still greater dispersion employed by Drs. Runge and Paschen; and, more than this, I have not found a single coincidence between argon and any line in the spectrum of any celestial body whatever. This happens, as everybody knows, also in the case of oxygen, nitrogen, chlorine, and the like.

The third point is as follows. So far I have worked upon some eighty minerals, and I have found the yellow line in sixteen; among the lines which I have already reported to the Royal Society are included all the stronger ones in the various series determined by the German physicists, but I can now add that in the region over which my work has extended, there is scarcely a single line in their series which I have not either seen or photographed in the spectrum of some celestial body or another. The following tables will show the results I have already obtained with all the six series of lines indicated by Drs. Runge and Paschen.

[1] *Proc.* Roy. Soc., vol. lviii. p. 117.

HELIUM.

11220	Sun.	Star or Nebula.
3889	C E	N. III. γ
3188		
2945		
2829		
2764 *		
2723		
2696		
2677		
5876	C 100 E	
4472	C 100 E	
4026	C 25 E	
3820	E	α Cygni
3705		
3634		
3587		
3555		
3513		
3499 *		
3488		
3479		
3472		
3466		
3461		
7066	C 100	
4713	C 2 E	
4121	E	N. α Cygni
3868	?	
3777	E	Bellatrix
3652		
3599		
3567		
3537 *		
3517		
3503		
3491		
3482		

GAS X.

	Sun.	Star or Nebula.
5016	C 30 E	
3965	?	III. γ
3614	E	
3448		
3355		
3297 *		
3258		
3231		
3213		
6678	C 25	
4922	C 30 E	
4388	E	N. III. γ
4144	E	III. γ
4009		III. γ
3927		Bellatrix
3872		Bellatrix
3833	E	Hid by H line
3806		Bellatrix
3785 *		
7282		
5048	C 2	
4438		Bellatrix
4169		Bellatrix
4024	?	N. III. γ
3936	Hid in K.	
3878	C E	α Cygni
3838	C E	α Cygni
3803 *		

* Means that these lines are out of the range of my observations.

In the tables, under "Sun," C, followed by a number, indicates the frequency as given by Young; E indicates the lines photographed during the eclipse of 1893. Under "star or nebula" the references are to the tables given in my memoir on the nebula of Orion (*Phil. Trans.* vol. clxxxvi. (1895), p. 86 *et seq.* N = Nebula of Orion).

Hydrogen, helium, and gas X are thus proved to be those elements which are, we may say, completely represented in the hottest stars and in the hottest part of the sun that we can get at. Here then, in 1895, we have abundant confirmation of the views I put forward in 1868 as to the close connection between helium and hydrogen.

J. NORMAN LOCKYER.

RESEARCH IN ZOOLOGY AT OXFORD.[1]

THE second volume of the Linacre Reports, which has lately been printed, shows that the zoological laboratory at Oxford continues to be a source of production of many interesting and valuable contributions to knowledge.

In the course of a little more than one year the colleagues and pupils of Prof. Lankester have published a number of memoirs and essays, which, when collected together, form a bulky octavo volume, illustrated by numerous lithographs and woodcuts.

There is, as might be expected, considerable range in the interest and importance of the several items composing the volume, but not one of them could have been omitted without lessening its value to the zoologist. At least four of the memoirs are of such importance that they may be considered to be standard works to which reference must be frequently made in future by naturalists of all nationalities. Of these, perhaps, the most important is Prof. Poulton's memoir on the structure of the hair and bill of the duck-billed Platypus, which contains not only an excellent account of certain histological features of this rare animal, but some extremely suggestive remarks, derived from this research, on the relations of hairs and scales.

Dr. Benham's beautifully illustrated essay on the brain of the interesting Chimpanzee "Sally," which recently lived and died in the Zoological Gardens in London, forms an important chapter in "Man's place in Nature." The careful comparison which Dr. Benham gives of the large and valuable series of anthropoid and human brains which he has examined, makes this memoir one of special interest and importance.

Mr. Bourne's monograph on the post-embryonic development of Fungia gives us, at last, detailed information and good illustrations of a subject which has long interested zoologists.

The description of Prof. Lankester's collection of the species of Amphioxus and the genera allied to it, which has been carefully and ably written by Miss Kirkaldy, forms a memoir which will be welcomed heartily by zoologists in all civilised countries.

The other contributions to this volume are of less importance, perhaps, than those referred to above, but they are all useful additions to our knowledge of many widely separated branches of zoology, and being carefully written, and the result of work done under excellent advice and guidance, cannot be neglected by those who are specially interested in the branches of zoology of which they treat.

With such a volume of good useful work before us, it is truly lamentable to read in Prof. Lankester's editorial preface of the general indifference prevailing in the governing bodies of the Oxford colleges towards the progress of natural knowledge. The University of Oxford and the colleges together are the possessors of very large endowments for the cultivation of learning in all its branches. No university in the empire is so fortunately

[1] "The Linacre Reports." Vol. ii.

situated, as regards funds, as Oxford is at the present day, and yet the just claims of the most progressive sciences upon her vast resources are persistently neglected, and she remains in the position of a follower rather than a leader in most of the scientific movements of the day.

The efforts that Prof. Lankester has so successfully made to stimulate his pupils to investigate natural things, have been made in spite of, and not as they should have been with the warm support and sympathy of the collegiate systems that prevail in Oxford.

During the past ten years only four fellowships have been awarded to young zoologists of promise by the Oxford colleges. The recipients of this support have each produced valuable work, which has reflected great credit upon themselves and the enlightened action of the colleges to which they belong. Not one of them has joined the ranks of the idle fellows which abound in the old universities of this country. The experiment cannot, therefore, be said to be a failure. It is as a fact the most conspicuous success of any of the college enterprises of the present day. Why then, it may be asked, have not other colleges followed the example that has been set? The answer to this question is to be found in the fact that, in consequence of the unfortunate competition that exists between colleges to swell the ranks of their undergraduates, the income of the endowments is frittered away in the salaries of the heads, the stewards, the bursars, and the tutors of the pass-men. Whether the time will soon come when a radical alteration will be made in the administration of the college endowments it is difficult to say, but there can be no doubt that the present state of affairs as regards the support of natural science in Oxford is little short of scandalous, and should call for the serious attention of men of influence who have her interests at heart.

Prof. Lankester is to be congratulated on the efforts he has personally made, as shown by the two volumes of "The Linacre Reports," to stimulate research in his own branch of science at Oxford; and it is to be most sincerely hoped that, in a little while, his enterprise will meet with the recognition from the colleges that it deserves.

SYDNEY J. HICKSON.

DEEP SOUNDING IN THE PACIFIC.

A DEEPER spot in the ocean than any yet known has been recently found by H.M. surveying ship *Penguin*. Unfortunately the observation was not complete, as a fault in the wire caused it to break when 4900 fathoms had run out without bottom having been reached.

Commander Balfour reports that this occurred in lat. 23° 40' S., long. 175° 10' W., about 60 miles north of a sounding of 4428 fathoms obtained by Captain Aldrich in 1888. A previous attempt to reach bottom had been foiled by a similar accident to the wire when 4300 fathoms had passed out, and the rising wind and sea prevented any further attempt at the time. As the deepest cast hitherto obtained is one of 4655 fathoms near Japan, it is at any rate certain that the depth at the position named is at least 245 fathoms greater.

It is hoped that before long a more successful attempt to find the actual depth will be made.

September 28. W. J. L. WHARTON.

LOUIS PASTEUR.

ON Saturday afternoon, M. Pasteur died at Garches, near St. Cloud, where he had gone for the summer in order to be near Paris, and at the same time to be near the large establishment for the preparation of antitoxic serum.

In 1868, Pasteur suffered from an attack of paralysis, the result apparently of a cerebral hæmorrhage; but although traces of this paralysis remained, he enjoyed fairly good health until 1887, when he developed symptoms of heart and kidney disease, probably a recrudescence of the diseases associated with his earlier paralysis. Four years ago he suffered from influenza, which appears to have left further weakness of the heart. Last winter he was unable to do any work, and in fact was confined to bed for several months; but when summer came, he was able to go to his country house at Villeneuve l'Etang, near St. Cloud, where he remained in comparatively good health, though easily fatigued, until about three weeks ago, when he seems to have felt that the end was approaching. It is stated that "about three weeks ago he kissed his grandchildren fondly, and pressed each for some time to his breast, sobbing as he did so. On being asked what was the matter, he said 'The matter is that I must so soon leave them.'" He appeared to be no worse at this time, but about a week later symptoms of uræmia began to develop, he became comatose, and on Wednesday last the uræmic poisoning became more marked, and by Friday it was evident that there could be only one termination to the illness.

In 1891 (NATURE, March 26) we gave a sketch of his life from the pen of Sir James Paget, some features of which may now be repeated. "Louis Pasteur was born on December 27, 1822, at Dôle, in the Jura, where his father, an old soldier who had been decorated on the field of battle, worked hard as a tanner." Father and mother alike seem to have been earnest, thoughtful people, whose one ambition seems to have been to "make a man" of their son.

"In 1825 they removed to Arbois, and as soon as he was old enough to be admitted as a day boy, Pasteur began his studies in the Communal College, and there, after the first year or two, he worked hard and gained distinction." He then, in turn, studied for a year at the college of Besançon and at the École Normale. He was only fourteen when he first applied for admission, but it was not until he had studied for a year that he went in for the examination; and in 1843 it is recorded that he was fourth on the list of successful competitors. At a very early period he devoted special attention to chemistry under Darlay at Besançon, and then under Dumas at the Sorbonne, and Balard at the École Normale. Here, too, in the École Normale, he commenced that study of molecular physics, especially in relation to the formation of crystals, which led up to his now classical investigation on the isomeric crystals of the tartrates and paratartrates of soda and ammonia. In 1847 he took his degree of Doctor of Science, after which he was appointed Assistant and then Professor of Chemical Physics in the University of Strassburg. In 1854 he was appointed Dean of the Faculty of Sciences at Lille, where he spent three years in organising the new school, and commenced those experiments on fermentation which seemed to follow naturally on his researches on the tartaric acids. He found that certain processes of fermentation were set up by distinct micro-organisms, under the action of which organic salts and even inorganic substances were broken down, and others were formed in their place. Three years later he was appointed Director of Studies in the École Normale in Paris, which office he retained until 1867. During this same period he was Professor, first of Geology, then of Physics, and latterly of Chemistry in the École des Beaux Arts. He also held the position of Professor of Chemistry at the Sorbonne.

As early as 1856, before his recall to Paris, the Royal Society of London awarded to him the Rumford Medal for his researches on the polarisation of light. In 1869 he was made a foreign member of the Royal Society, and in 1874 the Copley Medal was given to him. It is interesting to note in connection with his recent action as regards the Order offered to him by the Emperor William, that, during the bitterness caused by

the war, M. Pasteur sent back the Diploma of Doctor given to him by the University of Bonn in 1868, and subsequently received a message from the students calling him an impostor and a quack. In 1881 Pasteur was elected a member of the French Academy, succeeding to the seat of M. Littré. About the same time he was made an honorary Doctor of Science of the University of Oxford. In 1887 he was appointed perpetual secretary of the Academy of Sciences, but in 1889, owing to the failure of his health, he was compelled to hand over the duties of this position to M. Bertholet.

At the conclusion of his researches on crystals and wine fermentation, Pasteur commenced an inquiry into the diseases of the silkworm, and in no investigation that he undertook were his method and thoroughness more fully exemplified than in this. When he commenced his inquiry he had never even seen a silkworm, but for four years he spent several months of each year in tracing the germs of the "pebrine" disease through the various stages of development of the worm, egg, larva, chrysalis, and moth. He found what he described as "corpuscles," which he indicated were the contagious elements of the disease. These were taken up from the mulberry-leaves on which they had been previously deposited by diseased moths: some of the worms died, but others went on to the chrysalis and even to the moth stage, still affected by these "corpuscles," and the eggs laid by these moths were also found to contain them. He was convinced that the only way was to breed from moths not affected by the disease, and "to this end he invented the plan which has been universally adopted, and has restored a source of wealth to the silk districts: each female moth, when ready to lay eggs, is placed on a separate piece of linen, on which it may lay them all; after it has laid them and has died, it is dried, and then pounded in water, and, the water is then examined microscopically. If "corpuscles" are found in it, the whole of the eggs of this moth, and the linen on which they are laid, are burnt; if no corpuscles are found, the eggs are kept, to be, in due time, hatched, and yield healthy silkworms."

Pasteur's experiments on fermentation began to have a more direct bearing on disease when Sir Joseph Lister, applying the principles to the changes that occur in wounds, was able by his antiseptic practice to exclude putrefactive and septic germs from wounds, and so to prevent those terrible sequelæ which were the terror of surgeons of the past generation.

Then came Pasteur's great work in bacteriology, his attenuation of the anthrax bacillus and of other pathogenic organisms by which he procured a vaccinating virus, capable of producing a mild form of the disease: as a result of this attack vaccinated animals were protected against the attacks of the non-attenuated organism. This was first proved in connection with fowl-cholera, then in connection with swine erysipelas; but the most important application at that time was in connection with anthrax. His work on hydrophobia is still fresh in the minds of all. Pasteur's work does not end with his death. He had collected in the Institut Pasteur, which was raised as a memorial to his life's work, a band of able and well-trained investigators, who are imbued with the spirit that animated his mind and soul—men who, under his advice and encouragement, are working out the details of the great works that he initiated, who are endowed with some of his great mental power, and who have been fully trained under his eye in the methods of direct experiment and accurate observation, men who have been taught by him "n'avancez rien qui ne puisse être prouvé d'une façon simple et décisive," a rule always practised by himself.

France may well offer a public funeral. Louis Pasteur was one of her noblest sons—an honoured one during his life, and deeply lamented now that he is dead.

In Pasteur not only has France lost the greatest Frenchman, but the world has lost one of its greatest benefactors, not only of this age but of all time. Letters and telegrams of condolence have been sent by men of light and leading in many nations, and they indicate the sorrow felt unto the ends of the earth. No greater testimony than this could be given of the esteem in which the memory of the great investigator is held. The blessings which the human race owes to Pasteur have been recognised for some time, and now that the mind which gave them birth is at rest, one great outburst of grief arises. The expression of sorrow in France is full and sincere. At the funeral, which is arranged to take place next Saturday, the President of the Republic will be present, and other representatives of the French Government, together with a multitude of fellow-workers and friends who revere Pasteur's memory. The funeral procession will first proceed to Notre Dame, where a solemn requiem will be chanted in presence of the Archbishop of Paris. The body will afterwards be placed in one of the vaults of the cathedral until the celebration of the Centenary of the Institute of France, in three weeks' time, when it will be removed to its final resting-place. It has been arranged that the body of the great investigator shall be finally interred at the Institute which bears his name, and which will form a fitting monument to him. The representatives of science who will be assembled in Paris for the Centenary will accompany the transfer of the mortal remains of their foremost fellow-worker: so that while they unite to celebrate the foundation of the Institute of France, they will join together in sorrow for the deep loss which science has sustained.

NOTES.

THE eleventh International Geodetic Conference was opened at Berlin on Tuesday. Representatives were present from Austria, Belgium, France, Italy, Japan, Norway, Servia, Spain, Sweden, Switzerland, and the United States. The proceedings were opened by Dr. Bosse, the Prussian Minister of Public Education.

A NEW meteorological observatory is reported to have been opened on the Brocken, in the Harz Mountains, on Tuesday. The observations obtained there will be useful for discussion in connection with those made at the observatory on Ben Nevis.

SIR DAVID SALOMONS has arranged for an exhibition of horseless carriages on Tuesday, October 15, at the Tunbridge Wells Agricultural Show Ground, which has been lent to him for the occasion. The carriages will enter the ring at three o'clock p.m. The entrance money received will be used for prizes to be awarded at the show of the Tunbridge Wells and South Eastern Counties Agricultural Society next year, for the best horseless carriages intended to be used for agricultural, trade, and private purposes. Invitation tickets for the exhibition may be secured in order of application by Fellows and Members of the following Institutions sending an addressed envelope to one of the Secretaries—the Institution of Civil Engineers, the Institute of Electrical Engineers, the Institute of Mechanical Engineers, the Royal College of Physicians, and the Royal College of Surgeons.

THE Medical Schools attached to London and provincial hospitals commenced a new session on Tuesday with the customary introductory addresses. Prof. J. R. Bradford, at University College, discussed the positions occupied by biology, anatomy, and physiology in the medical curriculum. Dr. A. P. Laurie addressed the students at St. Mary's Hospital on the medical profession and unhealthy trades. At the London Hospital, Dr. J. Hughlings-Jackson was presented with his portrait and a piece of plate, in recognition of his great services to the London Hospital and Medical College, of his distinguished position in the profession, and of the advance he has effected in medical science by his laborious investigations and profound

insight into the diseases of the nervous system. The presentation was made by Sir James Paget, who also presented the prizes to the students. Mr. G. D. Pollock advised the students at St. George's Hospital as to their methods and aims of work. A valuable address on the more important developments of modern medicine, especially in the department of bacteriology, was given at Westminster Hospital by Dr. S. M. Copeman. Dr. W. J. Mickle discoursed on psychological medicine at Middlesex Hospital, and Dr. G. D'Ath read a paper at Guy's Hospital on "Our Profession, our Patients, our Public, and our Press." The introductory address to the students of the London School of Medicine for Women was given by Miss Ellaby.

The annual exhibition of natural scientific specimens of the South London Natural History Society will be held at the St. Martin's Town Hall, Charing Cross, on the evening of October 17.

A portrait bust in bronze of the late Dr. Robert Brown, the botanist, has been presented to the Montrose Town Council by Miss Paton, a kinswoman of the botanist; it has been placed in a niche in the house where Dr. Brown was born in 1773.

The *Lancet* announces that a subscription has been opened in Bristol to provide for the purchase and retention in that city of the celebrated collection of relics belonging to Jenner in connection with his introduction of vaccination. The collection is at present the property of Mr. Frederick Nockler, of Wotton-under-Edge, and was exhibited by him at the Bristol Exhibition in 1893, and since then in London, at each of which places it attracted a considerable amount of attention.

Was any record obtained of an earthquake in England on September 13? A correspondent informs us that at 12.25 a.m. on that day, four slight but very distinct shocks were felt two miles north-west of Southampton. The shocks caused the room to shake, and a deep grinding noise was heard; they occurred a few seconds after each other, but the interval between the third and fourth was a little longer than that between the previous tremors. The last shock appears to have been the most intense.

On Saturday, September 14, the ceremony of breaking the soil preparatory to the erection of the new building of the Brooklyn Institute, was performed in that city. The estimated cost of the new building is several millions of dollars, as its projectors intend it to be one of the finest and most complete of its kind erected. The Institute, which has a membership approaching 4000, has never yet had a suitable home, and it is confidently anticipated that rapid strides in membership and usefulness will be made when the present scheme has been carried to a conclusion.

We much regret to have to record the death, from injuries received whilst riding his bicycle, of Prof. C. V. Riley, of Washington. Prof. Riley, who was fifty-two years old and a native of England, died on September 14. He was for many years State entomologist of Missouri, and from 1878 till 1894 was Government entomologist of the United States, and as such did very much in devising and applying means to destroy noxious insects. His successful experiment in checking the ravages of the white scale in California, a few years ago, by introducing the parasitic lady bug, *Vedalia cardinalis*, was among the most brilliant triumphs of economic entomology. Prof. Riley has written and published much. He was one of the original Fellows of the American Association for the Advancement of Science, and President of the Zoological Section in 1888, when he delivered an address on the causes of variation in organic forms.

Authorities have differed much as to the character of crystallised bromine. Gmelin-Kraut's Hand-book describes the solid substance as steel grey and similar to iodine, whereas Schutzenberger says "solid bromine is a crystalline, brown-red mass, and not grey-blue, as it is often described." The *Zeitschrift für Anorganische Chemie* (x. 1 and 2) gives a short account of its preparation by Henryk Arctowski by a new method. A very concentrated solution of bromine in carbon bisulphide, when cooled to $-90°$, deposits the halogen in the crystalline form and free from the solvent. When thus obtained, bromine forms a mass of fine needles of some millimetres length, which have a fine dark carmine-red colour like that of chromium trioxide. Solid bromine, obtained in mass, has a crystalline fracture, and has no well-defined metallic lustre like iodine; at the best, it has a dull black metallic appearance.

The boiling point and the critical temperature of hydrogen, concerning which Prof. K. Olszewski made a preliminary statement in Nature some little time ago, have since been determined by him with every precaution against error, with the result that his first estimate is proved to have been very near the truth. In the current number of *Wiedemann's Annalen* the process is described in detail. The "expansion method," which had already been successfully employed to determine the critical pressure, was again utilised, the critical temperature being the temperature at which liquid hydrogen, when slowly released from pressure, first boils up, and the boiling point being the temperature attained when the pressure is reduced to that of one atmosphere. The chief difficulty was, as usual, that of determining the temperature accurately. Prof. Olszewski succeeded here by using a coil of thin platinum wire immersed in the hydrogen, whose varying resistance indicated the amount by which it was cooled. This coil was placed in a cast-iron cylinder into which hydrogen was conducted from a reservoir under 180 atmospheres pressure. The cast-iron cylinder could be brought down to a temperature of $-210°$ C., not far from the absolute zero, by means of liquid oxygen. But the critical temperature of hydrogen was found to be still lower, viz. $-234·5°$ C., and had to be found by extrapolation. The boiling point was $-243·5°$ C., or $-406·3°$ F.

In a report on the Coosa coal-field, published by the Geological Survey of Alabama, Mr. A. M. Gibson describes some remarkable effects of the great "cloud-bursts" which devastated that region in 1872, and are still conspicuous after a lapse of over twenty years. Clean-cut channels, in one case sixty feet wide and three or four feet deep, are described as extending down the mountain sides. They were formed by the direct force of the downpour of water, and along them were carried great masses of rock—one weighing a hundred tons—earth, trees, &c., which formed moraine-like masses at the base, or were scattered far over the lower ground.

Vol. vi. of the new series of Reports of the Geological Survey of Canada has recently been published, and contains the annual reports for the years 1892 and 1893, two special preliminary reports on particular districts (namely, parts of Ontario and Nova Scotia), and chemical and mining reports illustrated by numerous statistical diagrams. Among the matters of general interest, we may note the results of Mr. Low's exploration of Labrador. He finds that the interior of Labrador is well-wooded, instead of being a treeless wilderness as generally supposed, and finds evidence that the continental ice-cap took its rise in the interior of that country. In the chemical report, Mr. G. C. Hoffmann records a remarkable mineralogical discovery. In the kaolinized perthite from a pegmatite vein are found spherules of metallic iron, mostly minute but at times measuring as much as a millimetre in diameter, and having a siliceous nucleus. Mr. Hoffmann refers to similar spherules described by him some years ago (*Trans. Roy. Soc. Canada*, vol. viii. sec. iii. p. 39), on the joint-surfaces of a quartzite, and considers that the explanation suggested in that case applies here again—that the iron has been reduced from limonite by the action of organic matter.

THE Canadian Geological Survey has published the second part of vol. iii. of its monographs on "Palæozoic Fossils," in which Mr. Whiteaves describes and figures fossils—chiefly Gastropods and Brachiopods—from the Guelph and Hudson River formations.

WE have received from Mr. J. H. Knowles, of Lavender Hill, S.W., a catalogue of various books of science which he has for sale. Many interesting and valuable works on Ornithology, Botany, Astronomy, and other sciences are included.

MESSRS. JARROLD AND SONS have just published an abridged edition of "The Official Guide to the Norwich Castle Museum," at the small price of sixpence. The chief author of the book is Mr. T. Southwell, who has produced a work that should be in the hands of all visitors to the museum, which it so well describes. The little work is admirably compiled, and is illustrated by numerous figures in the text.

THE valuable series of reprints now being published by Mr. Engelmann, of Leipsig, under the title of Ostwald's "Klassiker der Exakten Wissenschaften" has recently had four more volumes added to it. These, numbered 63 to 66, contain respectively the following papers:—"Zur Entdeckung des Elektromagnetismus," by H. C. Oersted and T. J. Seebeck; "Über die Vierfach Periodischen Functionen Zweier Variabeln," by C. G. J. Jacobi; "Abhandlung ueber die Functionen Zweier Variabler mit vier Perioden," by G. Rosenhain; and "Die Anfänge des Natürlichen Systemes der Chemischen Elemente," by J. W. Doebereiner and Max Pettenkofer.

WE have received part i. vol. vi. of the *Transactions* of the Norfolk and Norwich Naturalists' Society, by which it appears that the Society has just completed its twenty-sixth year, and to be financially in a prosperous condition; now numbering 275 members, amongst whom we recognise many well-known names. The presidential address, by Dr. Plowright, was mainly devoted to the consideration of some obscure points in the life-history and development of the various forms of *Puccinia*, which he showed had by no means been worked out, and indicated the direction in which further investigations should be pursued. Amongst the papers read before the Society, and published in their *Transactions*, is a very interesting one on "Neolithic Man in Thetford District," with illustrations of the various types of flint implements found in the river-gravels of that neighbourhood. The usual "Report on the Herring Fishery of Yarmouth and Lowestoft" is also published, which having been continued for fourteen consecutive years, in the absence of trustworthy statistics on the subject elsewhere, should be possessed of value; and the same may be said of the very full meteorological notes by Mr. A. W. Preston. A chatty paper on "Old-time Yarmouth Naturalists," by Mr. F. Danby-Palmer, should also be mentioned as giving some particulars of the more noticeable of the old-time naturalists, for which that ornithologically rich section of the east coast has always been remarkable. There are fifteen published papers in all, each of which speaks well for the vitality and usefulness of the Society.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus sinicus*, ♀ ♀) from India, presented respectively by Mr. Thomas Mackenzie and Messrs. Davies and Sons; a Chimpanzee (*Anthropopithecus troglodytes*, ♂) from West Africa, presented by Captain G. C. Denton; a Piping Guan (*Pipile cumanensis*) from Uruguay, presented by Mr. P. du Pré Grenfell; four Green Lizards (*Lacerta viridis*), three Wall Lizards (*Lacerta muralis*), European, presented by Mr. C. W. Tytheridge; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, deposited; a Common Seal (*Phoca vitulina*) from Scotland, purchased.

OUR ASTRONOMICAL COLUMN.

RETURN OF FAYE'S COMET.—A telegram from Kiel, received on September 28, announces that Faye's comet was observed by Javelle at Nice on the 26th. At 12h. 34·8m., Nice time, it was in R.A. 21h. 8m. 11s., and Decl. 1° 54′ S. It is accordingly well situated in the north-western part of the constellation Aquarius, crossing the meridian a little before 9 p.m. At the time of observation it was noted as "feeble."

ELEMENTS AND EPHEMERIS OF COMET *a*, 1895 (SWIFT).—Dr. Berberich has computed the following new elements of Swift's comet, from observations made at Mount Hamilton, August 21; Nice, August 31; and Strassburg, September 16.

These elements represent the comet's orbit with a greater degree of accuracy than those previously deduced, and the ephemeris determined from them closely represents observations made at Paris. In continuation of the ephemeris given in NATURE of September 5, we print the following, from *Edinburgh Circular* No. 46:—

T = 1895, *Aug.* 20, 88480 *M. T. Berlin*

$$\left.\begin{aligned} \pi - \Omega &= 167^\circ\ 47'\ 7''\cdot8 \\ \Omega &= 170\ 16\ 17\cdot3 \\ i &= 2\ 59\ 24\cdot9 \end{aligned}\right\} 1895\cdot0 \qquad \begin{aligned} \mu &= 502''\cdot654 \\ \log a &= 0\cdot565825 \\ \log q &= 0\cdot112686 \end{aligned}$$

$$\phi = 40\ 22\ 17\cdot6 \qquad \text{Period} = 7\cdot059 \text{ years.}$$

Ephemeris for Berlin Midnight.

1895.		α app. h. m. s.		δ app. ° ′		Brightness.
Oct. 2	...	1 24 7	...	+4 32·9		
4	...	1 24 41	...	4 23·9	...	0·62
6	...	1 25 9	...	4 15·3		
8	...	1 25 31	...	+4 7·1	...	0·56
10	...	1 25 50	...	+3 59·5		
12	...	1 26 6	...	3 52·5	...	0·50
14	..	1 26 19	...	3 46·2		
16	...	1 26 31	...	3 40·6	...	0·44
18	...	1 26 42	...	3 35·8		
20	...	1 26 53	...	3 31·9	...	0·38
22	...	1 27 5	...	3 29·0		
24	...	1 27 19	...	3 27·0	...	0·33
26	...	1 27 35	...	3 25·9		
28	...	1 27 53	...	+3 25·7	...	0·29

It will be noticed that the comet is diminishing in brightness, and on October 12 will only be half the brightness at the time of discovery, August 20.

γ VIRGINIS.—Of the many double-star orbits which have recently been computed by Dr. See, of Chicago, none presents more features of interest than that of γ Virginis. This famous double star has been very persistently observed since its discovery in 1718, but none of the orbits previously determined are consistent with the most recent observations. Including some of his own measures, Dr. See finds the following elements (*Astronomical Journal*, No. 352):—

$$\begin{aligned} P &= 194\cdot0 \text{ years} & \ldots\quad \Omega &= 50\cdot4 \\ T &= 1836\cdot53\ \ ,, & \ldots\quad i &= 31\cdot0 \\ e &= 0\cdot8974\ \ ,, & \ldots\quad \lambda &= 270\cdot0 \\ a &= 3''\cdot989 & \ldots\quad n &= 1\cdot8557 \end{aligned}$$

Apparent orbit:

Length of major axis = 6″·824
,, ,, minor axis = 3″·530
Angle of major axis = 140°·4
,, ,, periastron = 140°·4
Distance of star from centre = 3″·062

A comparison of computed and observed places shows, according to Dr. See, that these elements are probably the most exact yet found for any binary star. It will be seen from the figures given that the line of nodes coincides with the minor axis of the real ellipse, which is also the minor axis of its projection; and, owing to the small inclination, the apparent ellipse is only slightly less eccentric than the real ellipse, so that the foci of the two ellipses nearly coincide. Dr. See points out that one of the consequences of this disposition of the orbit is to make the movement of the radius vector in the apparent orbit very little different from that in the real orbit, so that γ Virginis furnishes the best test we have for the exactness of the law of gravitation in stellar systems. "If there is any deviation from the Keplerian law of areas, it must be extremely slight. There-

re the force is nearly central, and if it differs at all from the law of Newton, the deviation must be relatively unimportant."

The orbit is also remarkable for its great eccentricity, which surpasses that of any known stellar orbit.

For many years to come the angular motion will be very slow, and Dr. See draws attention to the fact that observations of distance will be more valuable than angular measures in effecting further improvement in the elements.

THE THIRD INTERNATIONAL ZOOLOGICAL CONGRESS AT LEYDEN.

FROM first to last this Congress, the Session of which lasted from September 15 to 21, was favoured by exceptionally fine autumn weather, and the quaint old town of Leyden, where the meeting was held, as well as the island of Marken, the Zoological Park at Graveland (where apteryx thrives and gnus are kept in free pastures), the Zoological Station at Helder, the sea-beach of Katwijk and Scheveningen, and the port of Rotterdam, to all of which places excursions were organised, were under these circumstances seen at their very best.

The character of the meeting was eminently international. The daily bulletin, although edited in French, contained announcements of lectures to be held and of papers to be read in English and in German, and in the Sections these three different tongues often succeeded one another rapidly and fraternally.

On the Sunday evening preceding the official opening there had been an informal mustering of the forces then already assembled, and Prof. Hubrecht, of Utrecht, who, as President of the Netherlands Zoological Society, gave a hearty welcome to those present, hinted at the inadvisability of allowing the use of more than these three languages.

Still, besides forty-two representatives from Great Britain and the United States, sixty-three from France and Belgium, and twenty from Germany and Austria, there were no less than eleven Russians, eight Scandinavians, and sixty-four Dutchmen inscribed as members, who had to restrict the use of their native language to conversation among themselves.

The total number of members inscribed was 232, and not only the number but also the quality of the zoologists assembled was such as to make this international gathering really a very representative one, which served to bring together some of the veterans of the old guard, and a great number of the younger generation of zoologists.

A glance along the list of those that were present will show this at once. There we find Jul. Vict. Carus, Th. Eimer, V. Hensen, A. Metzger, F. E. Schulze, R. Semon, J. W. Spengel, R. Virchow, Aug. Weismann, K. Grobben, Ch. Julin, E. de Selys Longchamps, C. Lutken, H. Field, O. C. Marsh, W. B. Scott, C. W. Stiles, R. Blanchard, E. Bouvier, A. Certes, A. Milne-Edwards, E. Perrier, L. Vaillant, J. Anderson, Sir W. H. Flower, S. J. Hickson, John Murray, Adam Sedgwick, R. B. Sharpe, R. Trimen, d'Arcy Thompson, S. Apathy, S. Brusina, C. Emery, R. Collett, A. Kowalevsky, W. Schimkevitch, W. Salensky, W. Blasius, N. Zograff, W. Leche, F. A. Smitt, Th. Studer, van Wyhe, Max Weber, Vosmaer, Sluiter, van Lees, Lidth de Jeude, Kerbert Jentink, Hubrecht, Hoffmann, Hoek, Horst, Everts, Büttikofer, M. C. Dekhuyzen, E. Rosenberg, and van Bemmelen. Very many of these read papers in the Sections, three of them (John Murray, A. Milne-Edwards and Weismann) addressed general meetings; whereas on the Tuesday evening a lecture on the curiosities of bird life, illustrated by coloured lantern-slides, was given by Dr. Bowdler Sharpe, of the British Museum, and was attended by the young Queen and the Queen-Regent.

The Committee of Organisation, to whose excellent arrangements much of the success of the meeting was due, were Prof. Hubrecht of Utrecht, Dr. Jentink, Director of the Natural History Museum, Leyden (President of the Congress), Dr. Hoek (Secretary), and Dr. Horst (Treasurer).

There were six different Sections, a new feature of which was the inclusion of palæontology with recent zoology. There was no separate Palæontological Section.

In the first Section (general zoology, geographical distribution, with the inclusion of fossil faunas and evolution theory), Mr. A. Sedgwick, of Cambridge, gave an exposition of his views concerning direct cellular continuity in the living organism. In a later meeting of the same Section, Prof. Apathy, of Klausenburg (Hungary), demonstrated a series of the most beautiful and delicate microscopical preparations, which, already at an earlier date, have led him to conclusions very similar to those of Sedgwick just referred to.

Prof. Hensen, of Kiel, gave an interesting account of the Plankton expedition, its aims and its results.

Prof. Eimer, of Tubingen, spoke in this Section on the subject of orthogenesis, and on the impotence of natural selection for the production of new species.

In the second and third Sections, devoted to living and extinct vertebrates, their anatomy and embryology, papers were read by Profs. Zograff of Moscow, Vaillant, O. C. Marsh, Büttikofer, Lutken, Leche, Semon, Hubrecht, and van Bemmelen.

The fourth, fifth and sixth Sections embraced the invertebrates, one of them being specially devoted to entomology. Messrs. Wardell Stiles (from the United States), Hickson, Blanchard, Goto (from Tokyo), Perrier, Kowalevsky, Schimkevitch, Gilson, Salensky, and Julin were among the principal speakers in these Sections.

The sectional meeting which proved to be the most attractive was the one that was held on the last day of the meeting, when in the second Section, temporarily presided over by Rudolf Virchow, Dr. E. Dubois, the indefatigable naturalist, who has devoted the last six years to the collection of palæontological specimens in Sumatra and Java, gave a full account of the finding of the remnants of his *Pithecanthropus erectus*.

The four fragments (a femur, the upper part of a skull, and two teeth) upon which this new species, looked upon by its author as an intermediate stage between the anthropoids and man, was founded, were laid before the Section, together with a good many pieces intended for comparison. A most interesting discussion followed, in which Virchow, O. C. Marsh, Emil Rosenberg, Sir William Flower and Prof. Martin took a prominent part. Virchow's contention was that the four fragments did not belong to the same animal. He attempted to derive arguments from pathological anatomy, which would show that the osteophytic outgrowths of the femur described by Dubois were indications tending rather towards the human than towards the simian origin of the femur. Nevertheless, he spoke in a very appreciative tone, telling the Section that he had only wished to put in a point of interrogation where Dubois' affirmations did not appear to him to be as yet fully justified.

Prof. Marsh was inclined, on grounds derived from his vast experience in palæontological excavations, to support many of Dubois' conclusions. He had noticed exostoses of a similar nature as those of Pithecanthropus in fossil animals of quite different orders. He pointed out the necessity of carefully comparing these remains with those from the Sivalik Hills.

Prof. Rosenberg, considering more especially the femur and the cranium from the point of view of the anatomist, tried to show that the four characteristics, by which Dubois separates the femur of Pithecanthropus from that of man, are found also in human femora, in some few cases even all of them combined. The skull, on the contrary, is more that of a primate; but he did not agree with Dubois' argument that certain peculiarities of its *planum nuchale* tended to show that the animal had assumed a more erect gait. Very similar peculiarities are found in the New-World Cebus, which moves on all fours. Rosenberg acknowledged, however, that the high intrinsic value of the fragments was in no way diminished by the doubts expressed by him, because the femur, even if human, would prove Tertiary man to have existed in Java, the origin of man being thus pushed further back towards the earlier Tertiary period.

The results of this discussion, though not a decisive triumph for Pithecanthropus as a valid species, was a unanimous recognition of the great importance of Dr. Dubois' researches.

Another interesting afternoon lecture, which attracted numerous members of different Sections, was given by Prof. W. B. Scott, of Princeton, and was illustrated by lantern-slides. The wonderful continuity of the American tertiary formations, the vast geographical scale on which they are developed, and the excellent state of preservation of their fossils, was specially insisted upon. Skeletons of many members of the beautifully continuous phylogenetic series were projected on the screen.

Of the lectures held at the general meetings, those of Dr. John Murray and Prof. Milne-Edwards were most interesting to the audiences they addressed. Prof. Milne-Edwards spoke on the extinct avifauna of the Mascarene Islands in its relation to that of certain islands in the Pacific Ocean, and Dr. Murray gave an admirable survey of deep-sea exploration in general,

and of its principal results. In seconding a vote of thanks to Dr. Murray, Prof. Milne-Edwards availed himself of the opportunity of complimenting him, in terms of the highest eulogy, on the completion of the *Challenger* Reports, the cost of which has been so liberally met by the British Government, and the editorship of which has been in the hands of Dr. Murray since the death of the late Sir Wyville Thomson.

Prof. Weismann's lecture to the general meeting treated of a much more abstruse and complicated subject, viz. Germinal Selection. Under this name he introduced what he holds to be a supplementary hypothesis to that of Natural Selection, and by which he explains the fact that useful variations appear to be protected from their very first appearance, so that, when natural selection would require them, they are indeed always there. The simultaneous development of harmonious variations of different parts of the same organism was at the same time elucidated on similar principles.

It may be noticed in passing, that these theoretical views of Weismann's approach very closely to views expressed as the result of quite different series of palæontological observations by W. B. Scott and others. It is clear that an explanation of certain groups of facts is yet wanted. Weismann will have to show that his speculations do indeed bridge this gap.

In the three general meetings other questions of international significance were settled. In the first one, presided over by the Minister of the Interior, Mr. van Houten, the prize to which the name of the present Emperor of Russia is attached was awarded to Dr. R. T. Scharff, of Dublin. In the second one, of Wednesday, September 19, the conclusions of a report of M. Bouvier, on the question of bibliographical reform, were unanimously carried, and seven members of the Controlling Commission nominated, viz. Messrs. Spengel, Sidney Hickson, W. B. Scott, Blanchard, Hoek, Schimkevitch, and Lang.

Another commission for the definite codification of the rules of zoological nomenclature was appointed, and consists of Messrs. Blanchard, Victor Carus, Jentink, Sclater, and Wardell Stiles.

In the final meeting, Sir William Flower was nominated to the presidency of the next Zoological Congress, in 1898. Upon the proposition of the President of the Congress, in the name of the Committee of Organisation, it was decided to meet in England, the exact place of meeting being left to the consideration of the permanent Bureau.

During the Congress, as has already been announced in NATURE, three of its most eminent members—Messrs. Weismann, Milne-Edwards, and Sir Wm. Flower—received the honorary degree of Doctor in Science (Section of Zoology and Botany) from the Senate of the Utrecht University, upon the proposal of the Faculty of Natural Philosophy.

On Saturday the meeting closed, and the members united in a farewell banquet in the concert hall, where the Minister of the Interior was again present.

On Sunday, the 22nd, the Amsterdam Zoological Society *Natura Artis Magistra* invited the members to a luncheon party, and to a visit to its well-known gardens and aquarium.

After this the members of the Congress definitely separated. There is not one of them who has not extended the circle of his personal acquaintance amongst his fellow-workers in the field of zoology. And this extension of the feelings of international scientific fraternity is one of the great advantages of these cosmopolitan gatherings.

THE INTERNATIONAL CONGRESS OF PHYSIOLOGISTS AT BERN.

I.

MONDAY, September 9.—Presidents, Profs. Chauveau and Bowditch. Dr. Boruttau (Göttingen) demonstrated on a platinum wire contained in a glass tube filled with ·6 per cent. salt solution, electrical changes (negative variation), analogous to those occurring upon stimulation of a nerve trunk. The negative variation occurred not only upon electrical, but also upon chemical and mechanical stimulation of the wire. The apparatus used for the purpose were a Hermann's repeating rheotome and a Thomson's galvanometer.

Profs. Ewald and Goltz (Strassburg) showed a dog from which they had removed, at three successive stages, large portions of the spinal cord. In all 158 mm. had been removed: this involved all the spinal cord below the middle dorsal region, inclusive of the cauda equina. The dog had already survived the last operation two years. The condition was as follows:—(1) Entire muscular degeneration of hind limbs, and back muscles below mid-dorsal region; (2) evacuation of fæces, and sphincter ani ext., normal; (3) large quantities of urine collected in bladder, but were eventually evacuated; (4) vascular tone normal. Animal gave birth to young ones since last operation, and suckled them normally.

Prof. Fano (Florence) showed a special apparatus by which he could measure exactly motor reaction time. He worked on the dog, and found that it was 32·6 sec. for anterior paw, 27·32 for posterior. After removal of parts of cortical layer of frontal and occipital lobes this reaction time was diminished; upon stimulation of same regions it was increased. From this the author concluded that the cortical cells, especially of the frontal region, exert a constant inhibitory action on the spinal cord.

Prof. Langley (Cambridge) gave a demonstration on (1) the general anatomical relations of the sympathetic system; (2) connection of nerve cells and nerve fibres; (3) reflexes from the sympathetic system.

Dr. Mann (Edinburgh) read a paper, accompanied by lantern-slides, and gave a demonstration on the position of the psycho-motor areas in the rabbit, hedgehog, dog, and cat. The result of the author's researches was to show that (1) the same general scheme of arrangement of the psycho-motor areas holds good throughout the animal kingdom, and (2) that there exist in the above animals centres of varying psycho-motor value (higher and lower centres in the physiological sense).

Prof. Gaule (Zürich) discussed the growth of muscle, and came to the conclusion that it was periodic, and that there exist in muscular fibres changes corresponding to these periods. He further discussed and showed the effect of excision of the inferior cervical ganglion upon the biceps and psoas of the same side. Within twenty-four hours of this excision these muscles increase in weight, and have their resistance to mechanical strain greatly diminished (rupture easily).

Prof. Vitzou (Bucharest) produced blindness in a monkey by removal of the occipital region of the brain; two years after the operation the animal recovered, to a slight extent, its sight. Upon examining the brain at the seat of the lesion, he found a tissue of new formation; this tissue was very vascular, and its extirpation reproduced the blindness. Histological examination of this tissue showed the presence of nervous elements, which the author regarded as of true new formation.

Dr. Demoor (Brussels) stained the brains of animals to which he had given a strong dose of morphine or chloral hydrate by Golgi's method. Demoor found that the plotoplasm of the cell processes in these animals presented a characteristic mannilliform aspect, which was not to be observed in normal animals. The author showed his preparations

Monday Afternoon.—Presidents, Prof. Hensen and Mosso.—Prof. Herzen (Lausanne) described the characters of a gastric juice obtained by the author and Dr. Fremont (Vichy) from the isolated stomach of a dog. The œsophagus was sutured to the duodenum, and a fistula was made, from which the gastric juice was collected. The juice was without smell or colour, was highly acid, and could digest its own weight of coagulated albumin. The author further gave the result of his researches on the influence of the spleen on pancreatic digestion. He found a given quantity of blood from the splenic vein, added to a pancreatic infusion, greatly increased its digestive power, whereas the same quantity of ordinary arterial blood did not.

Prof. Schiff (Geneva) discussed the effects of an isolated lesion of one pyramid, and showed that it produced degeneration in the pyramid only, and not in the pyramidal tract.

Dr. Gurber (Würzburg) described the results of his researches on the crystallisation of serum albumin. He treated horse serum after Hofmeister's method (ammonium sulphate), and obtained four kinds of crystals. The author showed diagrams of these, and gave the results of their analysis.

Prof. Tigerstedt (Stockholm) described a new apparatus, on the principle of Pettenkofer and Voit, for respiration experiments on man. The author's apparatus is of such volume as to be able to contain several men at the same time.

Tuesday Morning.—Presidents, Prof. Rutherford and Hegir.—Dr. His, junr. (Leipsig) supported Engelmann's view that the propagation of the rhythmic cardiac wave takes place from fibre to fibre. He could not confirm Kent's results. He found in mammals, including man, a single muscular bundle which showed cross striation, going from the inter-auricular down into

the inter-ventricular septum, in the neighbourhood of a cusp of the mitral valve. The author divided this bundle by a transverse incision of 2 mm., and found that after this the auricle and ventricle beat each with its own rhythm. This bundle contained no nervous elements.

Dr. Kaiser (Heidelberg) showed that upon pinching off the lower two-thirds of the partially emptied frog's ventricle, this portion remained motionless; but on subsequently clamping the bulbus arteriosus, tension being produced in the ventricle, it recommenced to beat. Dr. Kaiser explains this result by supposing the existence in the frog's heart of a series of neurons which are discharged by an impulse which starts in the sinus, so that the mechanism is that of a reflex action: he believes the pinching destroyed the continuity of the nervous apparatus, while it left the muscle intact.

Prof. Kronecker (Bern) demonstrated in a most striking manner the effect of a sudden arrest of the coronary circulation, by injecting paraffin melting at 39° C. into the descending coronary artery. The heart at once stops and enters into marked fibrillar contraction, from which, except occasionally in young animals, it never recovers. This effect is not produced by ligature of the same artery; this, in Prof. Kronecker's opinion, is due to collateral circulation being at once established. From this experiment Kronecker infers that the cardiac rhythm is due to the activity of some structures which are exceedingly sensitive to sudden cessation of their blood supply; this is not true of muscles or nerve trunks, but is of a nerve plexus or a ganglion cell.

Dr. Magnus (Heidelburg) showed a sphygmograph for use on a dissected out artery.

Prof. Hurthle (Breslau) showed a new method of registering the arterial blood-pressure in man. The arm is made bloodless by means of an Esmark's bandage, and introduced into a phlethysmograph connected with a tambour. Hurthle also showed a method for simultaneously recording the heart movements and rendering audible the heart sounds. The latter was effected by a resonating box placed against the chest-wall; to the box a wooden tuning-fork was attached; the limbs of this vibrated in unison with the heart sounds, and varied the intensity of a current led through the primary coil of an induction apparatus, and through a microphone placed between the limbs of the fork; a telephone was connected with the secondary coil.

Dr. White (London) made experiments to show that perfect cardiac perfusion was not obtained with a Kronecker's canula as modified by Williams, the actual perfusion in a Williams' only extending to the top of the end-piece.

Dr. Zuntz (Berlin) described a new method for determining the velocity of the blood; it consisted in injecting into the carotid artery, during arrest of the heart produced by stimulation of the vagus, sufficient blood to bring the blood pressure back to the normal. Knowing the amount of blood which has to be injected, and the time this takes, the velocity of the blood, as well as the amount propelled by the heart, can be deduced.

Tuesday Afternoon.—Presidents, Profs. Wedensky and Tigerstedt. Dr. A. Dastre (Paris) showed that if fresh fibrin is placed in strong neutral saline solutions, one finds after a certain time two globulins in the solution. (1) Globulin coagulating at 54°; (2) Globulin having the properties of serum globulin; and further, that proteose and true peptone are also present. This action of saline solutions on fresh fibrin can be divided into different stages. If fresh fibrin be submitted in the same manner to the action of the digestive juices, the same results are produced; it is then quite justifiable to speak of a "saline digestion" of fibrin. Fibrin submitted to the action of oxygenated water, and to that of micro-organisms, gave the same results. When gelatin is similarly treated, gelatoses are produced, and the gelatin loses its power of coagulation.

Dr. A. Beck (Lemberg) discussed the velocity of blood in the portal vein. The author found that the normal velocity was from 2000 to 2800 cms. per second, and that 0·62-0·79 grm. of blood flowed through 1 grm. of liver tissue in twenty-four hours. The author used Cybulsky's method.

Prof. Berry Haycraft (Cardiff) read a paper on the change of shape of the heart during contraction. The author found it difficult to kill animals with the heart in systole: this he finally did by injections of $HgCl_2$. Killing animals with the heart in diastole presented no difficulty. The animals were frozen immediately after death, and sections of their hearts cut at different levels. The author's results confirmed those of Ludwig and Hesser.

Prof. F. Gotch (Oxford). The discharge of *Malapterurus electricus*. The electrical discharge of the organ evoked in the living fish by mechanical and electrical stimulation was investigated by the capillary electrometer, the frog nerve muscle galvanoscope, and the galvanometer. Each apparently single shock of the organ was found to be multiple in character, showing an initial primary, followed by several secondary electrical outbursts. The primary outburst had a latency of 4/1000" and a duration of 2/1000". The E.M.F. = 120 to 200 volts. The secondary effects which follow the primary are plainly perceptible in the isolated organ after killing the fish; hence the multiple character of the single *secousse* is due to the organ itself. Each of the secondary effects occurs at an interval of from 4/1000"-6/1000" after its predecessor. The primary as well as the secondary effects are monophasic in character; hence a very profound physiological effect is produced.

F. W. Tunnicliffe.

FORTHCOMING BOOKS OF SCIENCE.

Messrs. C. Griffin & Co., Ltd., will shortly issue:—An exhaustive treatise on "Petroleum: the Geographical Distribution, Geological Occurrence, Chemistry, Refining, and Testing, &c.," by Boverton Redwood and Geo. T. Holloway, in two volumes: "The Chemistry of Gas Manufacture: a Handbook on the Production, Purification, and Testing of Illuminating Gas, and the Assay of the Bye-Products of Gas Manufacture," by W. J. Atkinson Butterfield; "Chemistry for Engineers and Manufacturers," by Bloxam Blount and A. G. Bloxam, in two volumes: vol. i. "The Chemistry of Engineering, Building, and Metallurgy"; vol. ii. "The Chemistry of Manufacturing Processes"; "Electrical Measurements and Instruments: a Practical Handbook of Testing for the Electrical Engineer," by Charles H. Yeaman; "Textile Printing: a Practical Manual of the Processes used in the Printing of Cotton, Woollen, and Silk Fabrics," by C. S. Seymour Rothwell, with illustrative specimens (companion volume to the "Manual of Dyeing," by Messrs. Knecht and Rawson); "Bleaching and Calico-Printing: a Short Manual for Practical Men," by Geo. Duerr, with specimens designed specially to show various stages of the processes described; a third edition, revised and enlarged, of the "Outlines of Practical Physiology," by Dr. William Stirling; a fourth edition of "Foods: their Composition and Analysis," and a third edition of "Poisons: their Effects and Detection," both enlarged and revised, by A. Wynter Blyth; an eleventh edition, revised, of Munro and Jamieson's "Electrical Pocket-book"; a third edition, also revised, of Seaton and Rownthwaite's "Marine Engineering Pocket-book"; a new issue, revised, of "Griffin's Electrical Engineer's Price-book," brought up to date and edited by H. J. Dowsing; a second edition, revised, of "The Design of Structures," by S. Anglin; and the thirteenth annual issue of "The Year-book of Scientific and Learned Societies of Great Britain and Ireland," comprising Lists of the Papers read before Societies engaged in Fourteen Departments of Research during 1895.

In the Cambridge University Press's announcements we notice a series of volumes dealing with geographical and cognate subjects, which will be under the general editorship of Dr. F. H. H. Guillemard. The first volume will be by Prof. A. H. Keane, and will treat of Ethnology. This will be followed by "The Geographical Distribution of Mammals," by R. Lydekker. Mr. H. F. Tozer is to write on the "History of Ancient Geography"; and other volumes will deal with the "Renaissance Period of Geographical Discovery," by C. E. Ravenstein, and "Oceanography," by J. Y. Buchanan.; "The Scientific Papers of John Couch Adams," vol. i., edited by Prof. William Grylls Adams, with a memoir by Dr. J. W. L. Glaisher; "The Collected Mathematical Papers of the late Arthur Cayley," vol. ix.; "A Treatise on Spherical Astronomy," by Sir Robert S. Ball; "Catalogue of Scientific Papers compiled by the Royal Society of London," 1874–1883, vol. xi.; a second edition of Heath's "Treatise on Geometrical Optics; "A Treatise on Abel's Theorem," by H. F. Baker; "A Treatise on the Lunar Theory," by E. W. Brown; "An Elementary Treatise on Electricity and Magnetism," by Prof. J. J. Thomson; "A Treatise on Geometrical Optics," by R. A. Herman. In the Pitt Press Mathematical Series: "Euclid," books xi. and

xii., by H. M. Taylor; and in the "Cambridge Natural Science Manuals: "Mechanics and Hydrostatics: Part iii. Hydrostatics," by R. T. Glazebrook; "Electricity and Magnetism," by the same; "Solution and Electrolysis," by W. C. D. Whetham; "Sound," by J. W. Capstick; "Fossil Plants: a Manual for Students of Botany and Geology," by A. C. Seward; "The Vetebrate Skeleton," by S. H. Reynolds; "Text-Book of Physical Anthropology," by Prof. Macalister; and a second edition of "Practical Physiology of Plants," by F. Darwin and E. H. Acton.

Messrs. Swan Sonnenschein and Co.'s list includes:—"Text-Book of Embryology: Invertebrates," by Drs. Korschelt and Heider, part i., translated and edited by Dr. E. L. Mark and Dr. W. M. Woodworth, with additions by author and translators; parts ii. and iii. translated and edited by H. T. Campbell; "Text-book of Palæontology for Zoological Students," by Theodore T. Groom, illustrated; "The Indian Calendar," containing complete tables for the verification of Hindu and Muhammedan dates for a period of 1600 years (A.D. 300 to 1900) for the whole of India, by Robert Sewell, of the Madras Civil Service, in collaboration with Sankara Bâlkrishna Dikshit, with a table of eclipses by Dr. Schram; "Practical Plant Physiology," by Prof. Wilhelm Detmer, translated by S. A. Moor; "Introductory Science Text-books"—"Zoology," by B. Lindsay, with illustrations and diagrams; "Elementary Biology," by Dr. H. J. Campbell, second edition, with appendix; "Organic Chemistry," by J. Wade; Young Collector Series: "Fishes," by the Rev. H. A. Macpherson; "Mammalia," by the Rev. H. A. Macpherson; "Birds' Eggs and Nests," by W. C. J. Ruskin Butterfield.

Messrs. Longmans and Co.'s forthcoming books include:—"The Romance of the Woods: reprinted Articles and Sketches," by Fred. J. Whishaw; "The Life of Joseph Wolf, Artist and Naturalist," by A. H. Palmer; "The Life of Sir Henry Halford, Bart., F.R.S., President of the Royal College of Physicians, Physician to George III., George IV., William IV., and to Her Majesty Queen Victoria," by Dr. William Munk; "Darwin, and After Darwin: an Exposition of the Darwinian Theory, and a Discussion on Post-Darwinian Questions," by the late Dr. George John Romanes, F.R.S. Part ii. Post-Darwinian Questions: Heredity and Utility; "The Life and Letters of George John Romanes, F.R.S.," written and edited by his Wife; "Studies of Childhood," by Prof. James Sully; and in a new series of "Physical and Electrical Engineering Laboratory Manuals," "Elementary Physics," by John Henderson; "The Magnetic Circuit in Theory and Practice," by Dr. H. du Bois, translated from the German by Dr. E. Atkinson.

Messrs. George Philip and Son's announcements include:—*Maps*—Topographical Map of the Argentine Republic, in 10 sheets, scale 1 : 2,000,000, by H. D. Hoskold. Geological Map of the British Isles, forming part of the International Geological Map of Europe, scale 1 : 1,500,000. Philip's Topographical Map of England and Wales, in 40 sheets, scale 1 : 200,000. Philip's Library Map of India, scale 1 : 5,000,000. Philip's Large Map of Western Australia, scale 1 : 1,500,000. Philip's New Map of Liverpool, scale 6 inches to 1 mile. *Books*—"Telescopic Astronomy," by A. Fowler; "The Anatomy of the Human Head and Neck," illustrated by means of movable coloured plates, with description, by William S. Furneaux; "The Ox," its external and internal organisation, illustrated by means of movable coloured plates, with description, by Prof. G. T. Brown; "The Oarsman's Guide to the Navigable Rivers and Canals of the British Isles," by Members of the Cruising Club.

Messrs. Crosby Lockwood and Son hope to issue:—"Rural Water Supply: a Practical Hand-book on the Supply of Water and Construction of Water Works for Small Country Districts," by Allan Greenwell and W. T. Curry; "Dangerous Goods: their Sources and Properties, and Modes of Storage and Transport," by H. Joshua Phillips; "Practical Masonry: a Treatise on the Art of Stone-cutting," comprising the construction, setting out, and working of stairs, arches, niches, domes, &c., with fifty lithographic plates, by Wm. R. Purchase; "Refrigerating and Ice-Making Machinery," by A. J. Wallis-Tayler; and new editions of "The Metallurgy of Gold," by M. Eissler, with twenty-five additional plates and working drawings, and chapters on recent milling operations in the Transvaal, and the future outlook in the South African Gold-fields; and "Practical Tunnelling," by F. Simms, with large additions on recent tunnelling practice by D. K. Clark.

We find in the list of the S.P.C.K.:—"The Romance of Science" Series, "The Splash of a Drop," by Prof. Worthington, with numerous diagrams; "The Work of the Spectroscope," by Dr. Huggins; "Time," by Prof. Boys. Manuals of Science "Physiology," by Prof. Macalister; "Ancient History from the Monuments: Babylonia," by Prof. Sayce, a new edition, edited and brought up to date; "Simple Methods for Detecting Food Adulteration," by J. A. Bower, with diagrams; "Gosse's Evenings at the Microscope," a new edition, revised by Prof. F. Jeffrey Bell; "Iceberg, Prairie and Peak: some Gleanings from an Emigrant Chaplain's Log," by the Rev. Alexander A. Boddy; "The Zoo," vol. iv., by the Rev. T. Wood, with coloured illustrations.

Among Messrs. A. and C. Black's new books will be:—The last part of Prof. Newton's "Dictionary of Birds"; "Artistic and Scientific Taxidermy and Modelling," by Montagu Browne; Vol. i. of "Zoology," by Prof. Ray Lankester; "Text-book of General Pathology and Pathological Anatomy," by Prof. R. Thoma, translated by Dr. Alexander Bruce, two volumes, illustrated; "Introduction to the Study of Fungi," by Dr. M. C. Cooke, illustrated; "Dynamics," by Prof. P. G. Tait; "Milk: its Nature and Composition," by Dr. C. M. Aikman, illustrated; and a new edition of "Black's General Atlas of the World," with twenty-six additional maps of the North American States.

Messrs. Cassell and Co., Limited, promise:—The Century Science Series, edited by Sir Henry Roscoe, F.R.S.; "Charles Lyell: his Life and Work," by Prof. T. G. Bonney, F.R.S.; "British Birds' Nests: How, Where, and When to Find and Identify them," by R. Kearton, with illustrations of nests, eggs, young, &c., in their natural situations and surroundings; "Popular History of Animals for Young People," by Henry Scherren, with thirteen coloured plates and numerous illustrations in the text; "The Year-book of Treatment for 1896," twelfth year of publication, illustrated.

The Religious Tract Society promises: "Rambles in Japan: the Land of the Rising Sun," by Canon Tristram, with forty-five illustrations; "A Visit to Bashan and Argob," by Major Algernon Heber-Percy, illustrated; "Plants of the Bible," by Rev. George Henslow, illustrated; "A Primer of Hebrew Antiquities," by Rev. O. C. Whitehouse, illustrated; "Hidden Beauties of Nature," by Richard Kerr, illustrated; "Consider the Heavens: a Popular Introduction to Astronomy," by Mrs. William Steadman Aldis, illustrated; "A Popular Handbook to the Microscope," by Lewis Wright, illustrated, "Lighthouses: their History and Romance," by W. J. Hardy, illustrated.

Messrs. Sampson Low and Co. will publish by subscription "Twentieth Century Practice: an International Encyclopædia of Modern Medical Science," by leading authorities of Europe and America, edited by Dr. Thomas L. Stedman, in twenty volumes. Their other scientific publications include the second edition of "A History of Scandinavian Fishes"; "A Manual of Obstetric Nursing," by Marian Humfrey, vol. ii., and new editions of Hofmann's "Treatise on Paper-Making," and Davis's "Practical Treatise on the Manufacture of Brick, Tiles, and Terra-Cotta," both fully illustrated.

Messrs. Whittaker and Co. announce the following works:—"Transformers for Single and Polyphase Alternating Currents," by Gisbert Kapp, translated from the German; "The Inspection of Railway Material," by G. R. Bodmer; "The Chemist's Compendium, a Pocket-book for Pharmacists and Students," by C. J. S. Thompson; "Modern Printing, a complete handbook of printing," by J. Southward; a new and enlarged edition of "Coal-pits and Pitmen," by R. Nelson Boyd; "A Practical Trigonometry for the use of Engineers, Architects, and Surveyors," by Henry Adams.

Messrs. Rivington, Percival, and Co. will issue:—"Clinical Illustrations of the Diseases of the Fallopian Tubes and of Tubal Gestation," by Dr. C. J. Cullingworth; "Mensuration," by Rev. A. D. Clarke; "Beginner's Text-Books of Science" Series, "Chemistry," by G. Stallard; "Geology," by C. L. Barnes; "Electricity and Magnetism," by L. Cumming; "Heat," by G. Stallard; "Mechanics" (treated experimentally), by L. Cumming; "Physical Geography," by C. L. Barnes.

In Messrs. Putman's Son's list we find:—"Wild Flowers of the North-Eastern States," drawn and carefully described from life, by Margaret C. Whiting and Ellen Miller, with 308

...ustrations: "A Natural Method of Physical Training," by Edwin Checkley, illustrated from photographs, new edition; "The Law of Psychic Phenomena," a working hypothesis for the study of hypnotism, spiritism, mental therapeutics, &c., by T. J. Hudson.

The Clarendon Press has in active preparation:—"A New English Dictionary," portions of D, edited by Dr. J. A. H. Murray, and of F, edited by H. Bradley; "British Moralists of the Eighteenth Century," edited by L. A. Selby-Bigge, two vols.; "Index Kewensis," compiled at the expense of the late C. R. Darwin, under the direction of Sir Joseph D. Hooker, by B. Daydon Jackson, two vols.; and "An Introduction to the Algebra of Quantics," by E. B. Elliott.

Messrs. G. Bell and Sons have in the press:—"The Mechanism of Men-of-War," by Fleet-Engineer R. C. Oldknow, R.N.; "Torpedoes, Torpedo Boats, and Torpedo Warfare," by Lieut. J. Armstrong, R.N.; "Naval Gunnery," by Capt. H. G. Garbett, R.N.; "Naval Architecture, the Designing and Construction of a Warship," by J. J. Welch; "Logic: a Handbook for the Use of Students," by F. Ryland; "Gas Manufacture," by J. Hornby.

Messrs. James MacLehose and Sons, Glasgow, have in preparation a volume on "Deaf-mutism, including Chapters on the Education of Deaf Mutes," by Dr. J. K. Love and W. H. Addison; a new edition of a "Treatise on Diseases of the Ear," by Dr. Thos. Barr; "An Account of the Institution and Progress of the Faculty of Physicians and Surgeons of Glasgow," by Alexander Duncan.

Messrs. W. Blackwood and Sons' announcements include:—"Introductory Text-Books of Meteorology," by Dr. A. Buchan, new edition, with coloured charts and engravings; Page's "Advanced Text-Book of Geology," new edition, revised and enlarged by Prof. Lapworth; Dr. Mackay's "Elements of Physiography," rewritten and enlarged, "Elementary Algebra," "Mental Arithmetic."

The following additions will be made to Messrs. George Newnes' "Library of Useful Stories": "The Story of the Earth in Past Ages," by Prof. H. G. Seeley, with original illustrations from the author's collection; and "The Story of the Solar System," by George F. Chambers.

Mr. Edward Arnold will issue:—"The Exploration of the Caucasus," by D. W. Freshfield, in two volumes, illustrated; "Cycling for Health and Pleasure," by L. H. Porter, revised; and "Strength; or, the Development and Use of Muscle," by C. A. Sampson.

Messrs. W. and R. Chambers's list contains:—Eminent Engineers: "Lives of Watt, Stephenson, Telford, and Brindley"; "Thomas Alva Edison: the Story of his Life and Inventions"; a re-issue of Chambers's Encyclopædia, in ten monthly volumes.

Mr. Fisher Unwin gives notice of: "The Evergreen: a Northern Seasonal," part ii.; "Electricity for Everybody," illustrated. The Criminology Series. (2) "Criminal Sociology," by Prof. E. Ferri; (3) "Our Juvenile Offenders," by W. Douglas Morrison.

Messrs. A. D. Innes and Co. will publish: "A Naturalist in Mid Africa," by G. F. Scott Elliot, with numerous illustrations.

To Mr. Walter Scott's "Contemporary Science Series" will be added "Evolution in Art, as illustrated by the Life-Histories of Designs," by Prof. A. C. Haddon.

Messrs. W. H. Allen and Co. have in preparation:—"Ferns, British and Foreign," by John Smith, and a new edition of Herschel's "Popular Lectures on Scientific Subjects."

In Messrs. Macmillan and Co.'s announcements we find the following: "Sir Joseph Banks's Journal," selections edited by Sir Joseph Hooker, K.C.S.I., F.R.S.; "Sketches in Sport and Natural History," by the late Dr. George Kingsley, with memoir by his son Charles Kingsley; "A History of Mankind," by Prof. Friedrich Ratzel, translated from the second German edition by A. J. Butler, with preface by Dr. E. B. Tylor, with thirty coloured plates, maps, and numerous illustrations in the text, in thirty monthly parts, and in three vols.; "Studies in the Art Anatomy of Animals," by Ernest E. Thompson, illustrated; "The Cambridge Natural History," edited by S. F. Harmer and A. E. Shipley, vol. v., Peripatus, by A. Sedgwick, F.R.S., Centipedes, &c., by F. G. Sinclair, Insects, by D. Sharp, F.R.S.; "The Structure and Development of the Mosses and Ferns (Archegoniatæ)," by Dr. Douglas Houghton Campbell; "The Scenery of Switzerland," by Sir John Lubbock; "A Handbook of British Lepidoptera," by Edward Meyrick; "The Structure of Man," by Prof. Wiedersheim, translated by H. M. Bernard, and edited by Prof. G. B. Howes, illustrated; "A Text-book of Comparative Anatomy," by Dr. Arnold Lang, translated into English by Henry M. Bernard and Matilda Bernard, vol. ii.; "Dictionary of Chemical Solubilities," by Dr. Comey; "A System of Medicine," edited by Dr. T. Clifford Allbutt, F.R.S., five vols; "A System of Gynæcology," edited by Dr. William Playfair and Dr. T. Clifford Allbutt, F.R.S.; "Elements of Palæontology," by Prof. Karl A. von Zittel, translated and edited by Dr. Charles R. Eastman; "Principles of Mechanics," by the late Prof. H. Hertz, translated by D. E. Jones; "Evolution and Man's Place in Nature," by Rev. Dr. Henry Calderwood, second edition, in great part rewritten; "Miscellaneous Papers," by the late Prof. H. Hertz, translated by D. E. Jones; "Electro-Physiology," by Prof. W. Biedermann, translated by Miss F. A. Wells; "The Scientific Basis of Analytical Chemistry," by Prof. Wilhelm Ostwald, translated by Dr. George MacGowan; "Text-book of Botany," by Prof. Strasburger and others, translated by Dr. H. C. Porter; "The Life of Agassiz," by Jules Marcou, two vols.; "Columbia College, Contributions to Philosophy, Psychology, and Education"; Columbia University Biological Series: "Fishes, Living and Fossil," by Dr. Bashford Dean; Columbia University Press Publications: "Statistics and Sociology," by Prof. Richmond Mayo-Smith; "An Atlas of Fertilization," by Prof. Edmund B. Wilson; "Elements of Geometry," by George C. Edwards; "The Theory of Sociology," by F. H. Giddings; "Alternating Currents," by D. C. Jackson; "A Laboratory Course in Experimental Physics," by W. J. Loudon and J. C. McLennan; "An Exercise Book of Elementary Practical Physics," by R. A. Gregory; "Elementary Text-book of Physical Geography for High Schools," by R. S. Tarr.

GEOLOGY AT THE BRITISH ASSOCIATION.

AFTER the presidential address, which was of great local interest, and listened to with much attention by a large audience, Mr. Harmer read two papers bearing on the Coralline and Red Crags. This veteran geologist, who, with the late Mr. Searles Wood, jun., did so much to unravel the age of the various Tertiary deposits in East Anglia, rendered much service to the Section, not only by the contribution of papers and in the discussions, but by attending the numerous excursions, and placing his knowledge and experience at the service of those less acquainted with Pliocene and Pleistocene rocks.

Taking the 240 more abundant molluscan species found in the Coralline Crag apart from those which are represented by rare or even unique species, he finds that their assemblage points, more distinctly than the mere aggregate of fossils, to the Southern character of the fauna; 57 per cent. being extinct, only one species is not found south of Britain, and not less than 36 per cent. are characteristically Southern. The following summary gives the principal facts on which this conclusion is based.

Summary of the abundant and characteristic Species of Mollusca occurring in the Coralline Crag.

Not known as living (37 per cent.)	89
Living in distant seas	8
,, ,, the Mediterranean	133
,, ,, the West European area	9
,, not south of Britain	1
Total	240

Species of European Mollusca occurring abundantly in the Coralline Crag.

Southern and not British (28 per cent.) ...	42
British (rare) and Southern	9
(35 per cent.) ...	51
British (characteristic) and Southern ...	91
,, and not Southern	1
Total	143
Total number of species	436

In his second paper, Mr. Harmer acknowledged that the Eocene shells, and probably some others found in the nodule bed at Waldringfield, were undoubtedly derivative; but he contended that it was possible that others belonged to the period which elapsed between the deposition of the Red Crag at Walton and that at Butley. This conclusion was mainly based on the fact that many of them are found *in situ* in the Belgian Crags of this age.

Mr. Burrows followed with a paper on the distribution of Foraminifera in the Crags. In the Upper Crag, or Newer Pliocene, there are 29 species of common North Atlantic Foraminifera; in the Red Crag 20 species; and in the St. Erth beds 163, of which 66 occur also in the Coralline Crag. Some of the Coralline Crag Foraminifera appear to have been derived from older deposits. Notes were given on the age of the different portions of the Coralline Crag now or formerly exposed at several important localities.

Next came two papers on Southwold; the first by Mr. H. B. Woodward, on a section recently exposed by denudation at the North Cliff, and a second on recent coast erosion there, by Mr. Spiller. The Norwich Crag is succeeded by chalky boulder clay, and that by a fresh-water loam, peaty earth, and a recent beach deposit, in which a human skeleton was found this year. Mr. Spiller's paper gave an account of the erosion of the North Cliff during a storm in May last, and by measurements taken since, and comparison with a map previously made by Mr. Whitaker, he concluded that different points on the coast had been eroded at the following rate:—

		Feet.
Easton Bavents	Loss in 6 years ...	20
Easton High Cliff ...	,, 13 ,, ...	22
Covehithe Cliff	,, 6 ,, ...	84

In two short papers which followed, the Rev. E. Hill attributed the formation of some boulder clays to rapid deposit by the agency of water under the influence of floating ice and ice-rafts, a conclusion strongly controverted by several advocates of the land-ice theory who were present. A third paper, by the same author, described traces of an ancient watercourse seven miles long in Suffolk.

A paper, by Messrs. Reid and Ridley, described their recent researches by boring, and an examination of the deposits above the water-level, at Hoxne. The following is the section disclosed, revealing the apparent existence of a temperate flora between the morainic deposits and the Arctic plant bed. A grant was made by the Association to enable Mr. Reid to continue this work, with a view of determining the relation of the Palæolithic remains to the Glacial epoch.

		Feet.
Gravelly surface soil	about	2
Brick-earth; towards the base *Valvata piscinalis*, cyprids, bones of ox, horse, elephant (?), and Palæolithic implements	about	12
Sandy gravel, sometimes carbonaceous, with flint flakes	about	1
Peaty clay, with leaves of Arctic plants (?) ...	about	4
Lignite, with wood of yew, oak (?), white birch, and seeds of cornel, &c.	about	1
Green calcareous clay, with fish, *Valvata piscinalis*, *Bythinia tentaculata*, cyprids, *Ranunculus repens*, *Carex*	about	4
Boulder clay.		

The day's work was closed by a paper from the President, on some Suffolk wells, six of which penetrate some distance into the chalk.

Tuesday was devoted almost exclusively to papers on glacial subjects, opening with an interesting communication by Prof. Sollas on artificial glaciers, or "poissiers," made of pitch. This paper was illustrated by pitch models split longitudinally, lantern photographs, and models in Canada balsam, images of which could be thrown on the screen. The main point to which attention was directed was the power of the viscous substance to carry grains of rice, sand, or pigment uphill when confronted by a barrier, or when driven into a narrow gorge. The conclusion drawn was that ice and pitch conformed to the laws of fluid motion, and this was further illustrated by the flow of water over a raised model of Ireland, when the currents conformed to the directions of former ice movement. The pitch sometimes travelled over heaps of loose material without disturbing them.

Mr. Clement Reid followed with some illustrations of the glacial sections at Cromer, showing the great chalk boulders, the contortion of the chalk, and the contortion, crushing, brecciation, and shearing of the boulder clay at that locality. Prof. W. B. Scott gave an illustrated description of the "Bad Lands," and showed that this area was in Tertiary times the site of a succession of great lakes whose history extended from the beginning of the Eocene period up to Pleistocene times. Evidence of change in climate is given by the gradual disappearance of palms, and the diminution in numbers and variety of the reptiles. A paper by Mr. R. B. White described various deposits in Colombia (New Granada) to which he attributed a glacial origin; he recognises moraines, erratic blocks, breccias and conglomerates, in places mostly made up of volcanic materials, but elsewhere made of the débris of sedimentary rocks. The paper concluded with some novel speculations as to the cause of the Ice Age.

Mr. B. Thompson described a number of pre-glacial valleys Northamptonshire, belonging to the following chief types. New valleys without drift and having old filled-up valleys near at hand: (2) valleys with rock on one side and drift on the other; (3) streams re-excavating old, drift-filled, valleys; (4) re-excavated valleys with the drift only left in the form of river-gravel derived from it. In his account of some Snowdonian tarns, Mr. W. W. Watts concluded that one of the shallow lakes in Cwm Glas was in a very shallow rock-basin, and the other dammed by scree- and stream-detritus. Glaslyn and Llyn Llydaw, though finding exit over moraine, had rock-barriers at depths of from thirty to fifty feet below the lake surface, so that they are either confined in true rock-basins, or else are very much shallower than is generally supposed.

The Committee for exploring the supposed glacial shell-bed at Clava, hoped to bring important results out within the year, and that engaged in exploring the Calf Hole cave also hoped to finish its lists of fossils in the same period. In reporting on the high-level flint drift near Ightham, Mr. Harrison described excavations made into a gravel 658 feet above the sea on the face of the chalk escarpment: worked flints, chiefly scrapers and flakes, were found in great quantity. In the discussion Sir John Evans expressed scepticism as to the human origin of the supposed worked flints.

The Committee on Coast Erosion published a final report which contains an abstract of previous reports, and a considerable amount of new information from Kent, Suffolk, Sussex, Hampshire, Norfolk, Yorkshire, the Northern counties, Lancashire, and North Wales. The Committee concludes that the work of devastation is much aided by the abstraction of shingle and sand, and also by the erection of unsatisfactory sea-walls and groynes. They further recommend that the subject should become the work of a departmental Committee of the House of Commons. The twenty-first and final report of another long-standing Committee gives a useful summary of principles guiding underground water supply, and then resigns its task to the local scientific societies, which are urged to communicate all information received to the Geological Survey Office at Jermyn-street, where careful records are now kept. Such a course naturally will give increased value to the information daily supplied to inquirers from that office. In the last paper Mr. Holmes gave further information on an ancient silted-up stream course which flowed between the high ground of Warley, Billericay, and Maldon on the one hand, and that of Laindon, Rayleigh, and Althorne on the other, into the Blackwater. The deposits of this river were covered by the highest (oldest) gravel terrace of the Thames system. A paper by Messrs. Lomas and Kendall dealt with the striæ produced by modern glaciers.

The first paper on Saturday was that of Prof. Marsh on some European Dinosaurs. He exhibited a diagram placing American and European forms side by side, and showing that the European types filled up gaps in the American series. In many of his restorations he differed decidedly from those which have been previously published, some of which he characterised as being like nothing "in heaven above, or in the earth beneath, or in the waters under the earth." The Connecticut Triassic footprints he attributed to Dinosaurs and not birds. The Committee appointed to endeavour to recover the missing portions of the Cetiosaurus skeleton in the Oxford Museum had been unable to carry out their work within the year, but they had now determined on their course of action, and obtained the requisite permission, so that they hoped to complete the work before the Liverpool meeting. Mr. Montagu Browne communicated a description of a section

on the new Manchester, Sheffield, and Lincolnshire Railway, exposing Rhætic rocks in Nottinghamshire, and gave a list of fossils derived from these beds.

The first part of Monday's sitting was devoted to papers by authors from France and Belgium. M. G. F. Dollfus considered that in Upper Tertiary times there were two great seas in Western Europe; one was to the east, not very far from Eastern England, in Miocene times, and extended over the Netherlands and North Germany; the other, or old Atlantic, was to the west of England, and extended in gulfs into France and Portugal, probably communicating with the Mediterranean Sea along the Guadalquiver Valley. In Pliocene times the seas occupied similar positions, but the land was rather higher, and a gulf on the Atlantic side appears to have reached Cornwall. The English Channel was closed, and the Eastern Sea appears to have been open only towards the north. M. Van den Broeck's paper described the present state of knowledge of the Upper Tertiary strata of Belgium. He had determined that the Upper Oligocene strata did not exist in Belgium, but that the Upper Pliocene was probably present there. He concluded that the line of march of the Miocene fauna was from east to west, for Miocene forms present in Belgium were absent from England. That the Miocene formation had been once present in England he inferred from the fact that half the Belgian Miocene fauna was to be found in the Coralline Crag. A communication from M. M. Boule described the finding of remains of *Elephas meridionalis* and *E. antiquus* in association with worked flints, some of them of elaborate workmanship, but others of St. Acheul type, and mammoth tusks, one of which was 2·85 metres in length; one flint was found under a tusk of *E. meridionalis*.

Prof. John Milne's report on Japanese earthquakes was given in full to Section A, but a short account of his work was communicated to Section C. The author has prepared a catalogue of 8331 shocks recorded in Japan between 1885 and 1892. The instruments used have recorded earthquakes which must have travelled right through the earth with a velocity greater than if its interior were composed of glass or steel. They also indicate movements corresponding with variations in barometric pressure and strong winds, and even a diurnal variation possibly due to the evaporation of moisture and the condensation of dew.

Dr. H. J. Johnston-Lavis reported on the activity of Vesuvius during 1895. (The substance of his report has already appeared in NATURE for August 8). The Committee on coral reef exploration presented an interim report on the negotiations between the Royal Society and the Admiralty as to beginning the work of sounding and boring. Mr. Osmund Jeffs reported that a number of the geological photographs collected by his Committee had found a home at the Museum of Practical Geology in Jermyn-street, and that the rest would shortly be deposited there. Prints to the number of 1200 had been received and catalogued, but numerous localities, and particularly the Eastern Counties, were as yet poorly represented. The report contained some valuable recommendations for the apparatus suitable for continuing the work, and the Committee proposed to carry on its collection, and to make special efforts to induce local societies and individuals to fill up the blanks in the collection, and to make it a thorough photographic survey of geological phenomena throughout the United Kingdom. A valuable appendix to the report contained a list of such of the photographs as had been employed in illustrating geological works. Dr. Hatch's paper on the auriferous conglomerates of the Witwatersrand showed that gold occurred only in the matrix of these rocks, and not in the pebbles; it had probably been introduced by subsequent infiltration. Mr. E. A. Walford, in a report and paper, described the succession of limestones, clays, and sandstones which have been revealed by sinking between the Stonesfield slate and the Inferior Oolite in Oxfordshire, and traced these divisions north-west and south-east, correlating the upper calcareous division with the Fullonian, and the middle sandy division with the Northamptonshire Estuarine series.

The early part of Tuesday was devoted to papers on deep borings, and the later part to work chiefly on invertebrate palæontology. The President described the succession of rocks revealed by the experimental boring at Stutton. The section which heads the top of the next column gives that succession. The lowest rocks are likely to be of Carboniferous or Silurian age, but the absence of fossils renders it impossible to be sure which of these divisions they really belong to. The boring has now been carried down to a depth of 1356 feet, mostly in highly inclined and even vertical strata of the same doubtful character.

	Feet.
Drift (river gravel)	16
London clay and Reading beds	54
Upper and middle chalk	720
Lower chalk, with very glauconitic marl at the base (almost a green sandstone)	$154\frac{1}{2}$
Gault	$49\frac{1}{2}$
Palæozoic rock, with a high dip.	

Mr. J. Francis gave the methods and results, hitherto unpublished or incorrectly stated, of the attempt to determine the dip of strata met with in deep wells at Ware and Turnford. After rejecting various magnetic and mechanical appliances, the following device was hit upon. The boring tools were lowered with extreme precautions to prevent any torsion during the lowering, and by means of steel points connected with them the direction of a known diameter was marked by vertical chases on the circumference of the core while still *in situ*; during the raising of the tool no twisting occurred; a wax mould of the top of the core *in situ* was then taken, and again the lowering and raising were done without twisting. The core was then broken and lifted, and by means of the diameter marked on it *in situ*, confirmed by a known line on the wax mould, the direction and amount of dip was ascertained. To test the method the boring was continued, and after the top of the core had been ground to a flat surface, steel-punch marks along a known diameter, maintained by careful lowering and raising with the same precautions, were impressed on the surface, and again the core was broken and lifted. This observation was within a degree of the previous one; so that there is probably only a negligible error, or none, in the observations. The dip of the Silurian rock at Ware at 828 feet below the surface was 1° west of south, at an angle of 41°. Similar experiments at Turnford, carried out with rather less success, gave the dip of the Devonian rocks at 994 feet as 17° west of south at 25° from the horizon. These dips correspond with those of the Secondary rocks off the Wealden axis. The south-easterly dip which has been published for one of these instances is incorrect. Mr. Harmer, in a paper which followed, advocated that the survey of deep-seated rocks by borings should be systematically carried out by the Geological Survey, the expense being provided for indirectly by the appreciation of real property, and directly by royalty, wherever success attended the operations.

Prof. Claypole described some whole specimens of Cladodonts from the Devonian rocks of Ohio, which showed that many species hitherto defined from single and isolated teeth can no longer be maintained. The Upper Devonian shales of the same region have yielded many genera of large Placoderms; the head of *Dinichthys* measured from 2 to 3 feet in length; *Titanichthys* was still longer; and the jaws of *Gorgonichthys* alone measured 24 inches in length, ending in teeth or points from 6 to 9 inches in length. All these genera are closely allied to *Coccosteus*.

One of the most important papers of the meeting was that by Prof. Nicholson and Mr. Marr on the Phylogeny of the Graptolites. They are led to believe that a character of essential importance in dealing with the classification of the Graptolites, and one which, in all probability, indicates the true line of descent, is found in the shape and structure of the hydrothecæ, the point of next importance as indicating genetic relationship being the "angle of divergence"! These views are illustrated by reference to forms belonging to the "genera" *Bryograptus*, *Diplograptus*, *Tetragraptus*, and *Didymograptus*, which appear in turn in this sequence. Out of nine *Tetragrapti* (and the authors know of no other forms referred to this genus which are represented by well-preserved examples), eight are closely represented by forms of *Didymograptus*, which are closely comparable with them as regards characters of hydrothecæ and amount of "angle of divergence," whilst the ninth is comparable with a *Didymograptus* as regards "angle of divergence" only. Moreover, four of the *Tetragrapti* are comparable as regards the two above-named important characters with forms of *Dichograptus* and *Bryograptus* with eight or more branches, and the authors confidently predict the discovery of forms belonging to these or closely allied many-branched "genera," agreeing with the remaining *Tetragrapti* in what they regard as essential characters. They give details showing points of agreement of each group of the various series, including a two-branched, a four-branched, and a many-branched form, and point out how difficult it is to understand how the extraordinary resemblances between the various species of *Tetragraptus* and *Didymograptus* (to take one example) have

arisen, if, as usually supposed, all the species of a "genus" have descended from a common ancestral for each genus, in the one case four-branched, and in the other case two-branched. On the other hand, it is comparatively easy to explain the more or less simultaneous existence of forms possessing the same number of stipes, but otherwise only distantly related, if they are different ancestral types. Phenomena somewhat analogous have been detected amongst the species of Ammonites and Brachiopods. Following these inferences to their legitimate conclusion, the authors point out how "genera," like *Diplograptus* and *Monograptus*, may contain representatives of more than one "family" of graptolites according to the classification now in vogue, which would account for the great diversity in the characters in the monograptid hydrothecæ.

Messrs. Garwood and Muir followed with a paper on the zonal divisions of the Carboniferous system. The following zones are recognised by them :—

Zone of *Productus* c.f. *edelburgensis.*
,, ,, *latissimus.*
,, ,, *giganteus.*
,, *Chonetes papilionacea.*
,, *Spirifera octoplicata.*

Mr. Garwood has traced the zone of *P. latissimus* occupying the same position relative to that of *P. giganteus* from Settle, in Yorkshire, to the Northumbrian coast, near Howick Burn. In conclusion, the authors hope that their work may be continued by a Committee, and one was appointed by the Section and confirmed by the General Committee of the Association.

Prof. T. Rupert Jones, in the twelfth report on Palæozoic Phyllopoda, gave a *résumé* of these organisms referred to in previous reports, and appended some valuable notes and two tables by Prof. Lapworth, of which the first gives a general correlation table of the Lower Palæozoic rocks ; the second, the horizons of the chief species of Phyllopods. A third table gives a list of the geological order of species. After hearing interim reports from the Committees on Eurypterids, and on type specimens, the Section listened to a paper by Dr. Woodward on Decapod Crustaceans from the Cretaceous rocks of Vancouver, in which the following new species were described. *Callianassa Whiteavesii*, *Palæocorystes Harveyi*, *Plagiophthalmus(?) vancouverensis*, and *Homolopsis Richardsoni*. Many of these forms approach contemporaneous European types. The closing report was that on erratic blocks. The Yorkshire Boulder Committee and that of the Hull Geological Society are promoting a systematic survey of the ground. New work has also been done in Lincolnshire, Shropshire, Cheshire, South Wales, and Ireland.

A very pleasant feature of this year's meeting has been a series of afternoon walks or drives, carefully planned by the Local Secretary, Mr. Ridley ; in many of these the President took the leadership, and several members of the Section attended. The list of these included Bramford, Sproughton, Orford, Sudbourne, Butley and Chillesford, Woodbridge and Sutton, Tattingstone, Bawdsey, Foxhall, and Cromer. At several of these localities the sections had been freshly scarped or reopened by the Local Committee and by the landowners. It is much to be hoped that in future similar opportunities may be afforded of acquiring as full a knowledge of the geology of the neighbourhood in which the meeting is held.

ZOOLOGY AT THE BRITISH ASSOCIATION.

As this Section was occupied with dredging excursions on the Saturday and Wednesday, only four days were available for sectional meetings, and as the number of papers and reports to be discussed was large (nearly fifty), the sittings were continued late into the afternoon. The majority of the papers dealt with marine zoological subjects, and fishery questions received special attention.

After the President's address on Thursday, the following reports of Committees were taken :—

On the marine zoology, botany, and geology of the Irish Sea. The report deals with nine dredging expeditions held during the past year, and discusses the additions made to the known fauna. Statistics of the dredging results are given to show (1) the relative richness, per haul, of the shallower over the deeper waters, and (2) the relatively large number of genera represented by the species in one haul ; pointing to the conclusion that, as a rule, allied species are not found together. The submarine deposits round the Isle of Man, and the currents of the Irish Sea are also discussed.

On the migration of birds. The nine years' observations are now being tabulated for presentation at next meeting.

Investigation of the zoology of the Sandwich Islands. Valuable collections are being made and brought home, and unless these are made now they can never be done, as the extinction of much of the present fauna is not only inevitable, but will be immediate.

Research at the Zoological Station at Naples. The British Association table has been occupied by Mr. M. D. Hill, who has been investigating the maturation and fecundation of the ova of Echinodermata and Tunicata.

Research at the Marine Biological Laboratory at Plymouth. This Committee have enabled Miss Florence Buchanan to work out the blood-forming organ in the larva of Magelona ; Mr. E. J. Allen to work on the nervous system of the embryonic lobster ; and Mr. Sumner to work at the Echinoderm fauna of Plymouth.

Investigation of the fauna and flora of the West Indian Islands. The Committee reported upon the progress made in working up the collections.

On an Index Generum et Specierum Animalium. In Mr. Sherborn's hands the Index is making satisfactory progress.

On the physiological applications of the phonograph. The Committee are studying the marks on the cylinder of the phonograph by microphotographs and by recording curves, and they propose to make these available for philological purposes in the study of dialects.

The following papers were then taken :—

On the Stereornithes, by C. W. Andrews. They are a heterogeneous group of extinct birds, found in Patagonia, whose chief points of resemblance lie in their large size and reduced power of flight. Some of them, at least, have no special affinities with the living Ratitæ. They are not represented in European museums.

Facts and reflections on budding in compound Ascidians, by Prof. W. E. Ritter (California). The author argues for the polyphyletic origin of the compound Ascidians ; he considers that there is no homologue of the "epicardium" of *Clavelina* in either *Goodsiria* or *Botryllus ;* he suggests that budding has arisen in small Ascidians as a compensation for diminished power of sexual reproduction ; he believes that physiological necessities have modified the course of development by budding, so that the endoderm now produces some organs originally formed from ectoderm.

A new classification of the Tunicata, by W. Garstang. The author gave his reasons for proposing to modify the classifications given by Herdman and by Lahille, by adopting some of the features of each scheme. In the main he proposes to follow Herdman in the primary divisions, and Lahille in the subdivisions. He considers *Pyrosoma* to be related to the pelagic forms, such as *Salpa*, and not to the fixed Ascidians. He makes use of the branchial sac largely in classification. This paper gave rise to an interesting discussion.

On the presence of skeletal elements between the mandibular and hyoid arches of *Hexanchus* and *Læmargus ;* and on the presence of a sternum in *Hexanchus griseus*, by Dr. P. White.

On the Creodonta, by Prof. W. B. Scott. This and some of the other papers gave rise to considerable discussion, and the Section did not adjourn till about five o'clock.

In the course of the day's proceedings it was moved by Prof. W. A. Herdman (President of the Section), seconded by Dr. P. L. Sclater (past-President), and carried unanimously, that the zoologists of this Section desire to present to Dr. John Murray their congratulations on the completion of the *Challenger* publications, and their best thanks for his splendid services to science. This resolution was duly conveyed to Dr. Murray, and a letter of thanks from him was received by the Section later in the meeting.

Friday was devoted to papers and discussions on the marine fisheries. Prof. McIntosh led off with a paper on some of the results of scientific investigations as applied to the fisheries. He gave a useful summary of what had been effected by the Scottish Fishery Board : he showed that the three-mile limit was insufficient to protect the spawning fishes, and in conclusion urged that scientific investigations on the fisheries should be carried out by Government and not be left to Universities.

Prof. Haddon followed with a report on the Royal Dublin Society's Fishery Survey, and also gave an account of the Fishery School at Kingsend, near Dublin. He pointed out the special conditions of the Irish fishery grounds, the lack of access to markets and of fish-curing stations on the west.

Dr. Bashford Dean (U.S. Fish Commission) gave an account

of oyster-cultural methods, experiments, and new proposals. He pointed out the difficulties in "spat" collecting, and showed that if these could be overcome the problem of raising oysters successfully would be solved. He dwelt on the effects of bad aeration, and of changes of temperature, and on the difficulty in retaining the embryos in closed areas, such as the *mare piccolo* at Taranto and the Brenéguy lake in France. Finally he discussed the cultural methods recently patented in the United States.

Prof. W. A. Herdman and Prof. R. Boyce gave a paper on oysters and typhoid, in which they explained the investigations they had made on the normal and abnormal life-conditions of the oyster, including the effect of pathogenic organisms. The oysters were laid down in various kinds of water, and fed on a variety of substances, both in the laboratories at Liverpool and also at the Port Erin Biological Station. Some of the results obtained are: the beneficial effects of aeration, the superiority of natural food (protophyta, &c.) over artificial (oatmeal, &c.), the deleterious effects of stagnation, great toleration of sewage, inimical effect of typhoid fæcal matter, the identification of *Bacillus typhosus* in oysters fourteen days after infection. The observations are still in progress, and a Committee of the British Association has been formed for the purpose of carrying on the investigation.

Dr. H. C. Sorby read a paper on the oyster culture in the Colne district, which was to be visited by a party of zoologists from the Section the Wednesday following. He described the grounds where spat was obtained, and the celebrated Pyefleet creek where the "natives" are fattened for the market.

Mr. J. T. Cunningham gave the last of the fishery papers, on fish and fishing grounds in the North Sea. This author disputed the idea that the great quantities of young plaice in the eastern parts of the North Sea are derived from the spawn and embryos carried across by currents, and that these plaice when they grow large supply those parts of the North Sea that lie further west. He suggests that the plaice on the German side are a smaller race, and that they correspond in distribution to a tract of warmer Atlantic water. He urged the necessity for a scientific investigation of the North Sea fisheries, and for experiments in rearing young food fish in artificial ponds. A discussion followed, in which the authors of the papers, the President, Mr. Alward, Mr. A. O. Walker, and others took part.

In the afternoon a discussion took place on zoological bibliography, opened by Dr. Haviland Field with an account of his scheme for the establishment of an international bibliographical bureau, to be located at Zurich. The organisation is now nearly completed, and the bureau is expected to start work in January 1896. Dr. Field asks England to form a National Committee, to organise a service of correspondents, and to give a grant towards the Bureau. A Committee of the British Association has been appointed to consider the matter and report.

Dr. Field also read a paper on the date of publication of zoological papers, in which he urged that the date of *distribution* be adopted as "publication."

Rev. T. R. R. Stebbing gave a paper on economy of labour in zoology, proposing that an effort should be made to gather into a succinct form all the most indispensable knowledge in each branch of zoology.

Prof. G. Gilson (Louvain) described the septal organs of *Owenia fusiformis*; Prof. F. Y. Edgeworth read a paper on the statistics of wasps; and Mr. W. Garstang exhibited a simple and efficient collecting reservoir for the surface tow-net. This tow-net was experimented with on Saturday's dredging expedition, and was found to work very satisfactorily.

On Monday forenoon, Prof. L. C. Miall gave an account (illustrated by the lantern) of our present knowledge of the causes and conditions of insect transformation. He pointed out the fundamental distinction between the metamorphoses of insects and those of other animals. The metamorphoses of marine animals were larval, those of insects adult metamorphoses, the migratory stage being late in the life. In insects the migration was undertaken by the adult, the feeding by the young. This caused considerable difference between these two stages, the adult insect becoming more and more highly organised and specialised and the larva more and more degenerate. This marked contrast brought about the necessity for a quiescent pupa stage between. This paper led to some discussion on metamorphosis.

Dr. H. C. Sorby exhibited a series of marine animals caught in the Suffolk estuaries, and mounted as lantern-slides after various methods of preparation.

Dr. Sorby gave an account of his apparatus for catching minute marine animals, and for estimating the number of organisms in given quantities of sea water.

Dr. E. Frankland read a paper on conditions affecting bacterial life in river water, in which he showed that in a series of monthly observations on the water of the Thames bacteria were more numerous in winter than in summer. There were three conditions which might affect the bacteria, and which he had disentangled, viz. temperature, sunshine, and the volume of water. Sunshine was a powerful germicide, but its effect ceases at a small depth in muddy water. The amount of microbes was found to vary with the amount of flood water. Storage has a very beneficial effect in purifying river water from bacteria.

Prof. A. C. Haddon made an appeal to zoologists to urge upon Government and scientific societies the necessity for an immediate exploration of oceanic islands of the Pacific. He pointed out that the great depths of the sea would remain for long unaltered, that the Antarctic was probably not undergoing any rapid change, but that the fauna and flora of the islands, and the customs of their inhabitants, were all undergoing change from year to year, and therefore ought to receive our first attention.

A paper on the Coccidæ of Ceylon, by Mr. E. E. Green, illustrated by beautiful plates, was read by Prof. Howes.

Dr. H. O. Forbes gave a paper, "Criticisms on some points in the summary of the results of the *Challenger* Expedition," in which he dealt with the supposed greater size of the sun in Carboniferous times, and also with the views of Dr. Murray in reference to the occurrence of similar forms in Arctic and Antarctic regions. Finally he pointed out that the evidence for an Antarctic continent in Tertiary times is really supported by the *Challenger* collections, rather than the reverse, as held by Dr. Murray.

A paper on the marine fauna of Houtman's Abrolhos Islands, West Australia, by W. Saville-Kent, showed that the anomalous character of the fauna of Abrolhos can only be accounted for by the assumption that an ocean current setting in from the equatorial Indian Ocean penetrates as far south as this island group.

Dr. Gregg Wilson read a paper on hereditary polydactylism, and also one on the reproduction of the common crab. Dr. Wilson was of opinion that an increased size limit would be a very distinct protection to the crab. A close time at the end of the year would protect the female at a time when there is most destruction.

On Tuesday, Prof. Lloyd Morgan gave an account of his experiments on instinct in young birds. He reared young moorhens, chicks, &c., for the purpose of determining how far the activities of locomotion (swimming, diving, running, flying), feeding, bathing, &c., are instinctive or congenital, and how far their definiteness is a matter of individual acquisition. It was found that timidity had a congenital basis, but was perfected by individual acquisition. There was no instinctive avoidance of insects with warning colours, but such avoidance was rapidly acquired by the individual. There appears to be little support for the view that what is individually acquired is then passed on by heredity.

Dr. Bashford Dean gave an exhibition of ova and larvæ of *Amia*, *Lepidosteus* and *Acipenser*, with some notes on the early development of the Ganoids, in which he brought out that Embryology supports the views derived from Palæontology. Dr. Dean considers that *Lepidosteus* is the oldest or most primitive, and *Amia* the form which comes nearest to the Teleosts.

Dr. Otto Maas (Munich) discussed some questions relating to the morphology and distribution of Medusæ. He exhibited some plates of supposed deep-sea Medusæ from the *Albatross* expedition showing the prevalence of a purplish tint, which he supposed to be the complementary tint to the green phosphorescent light given out by many deep-sea animals.

Mr. J. E. Moore's paper on spermatogenesis in birds, showed that the spermatic elements of pigeons have a marked tendency to form multinucleate masses. The whole course seems to correspond more closely with elasmobranchs than with mammals.

Prof. G. B. Howes read a paper on the mammalian hyoid. He showed that there were two types: (1) Proterostylic, found only in man and marmosets, and (2) Opisthostylic, known only

in rabbits and some other rodents. The following papers: On the development of the teeth in certain Insectivora, by M. F. Woodward; on the poison apparatus of certain snakes, by G. S. West; on the value of myology in the classification of animals, by F. G. Parsons; and on ultimate vital units, by Miss Nina Layard, concluded the ordinary sittings of the Section.

A notable feature of the meeting was the very successful dredging expeditions organised for the Zoological Section by the Local Committee, with the help of the President of the Section and Dr. H. C. Sorby. On Saturday a large steamer was chartered from the Railway Company for dredging outside Harwich. Many hauls of the dredge, and of various forms of tow-net, both surface and bottom, were made off the Naze and in the neighbourhood of the Gunfleet bank. Large quantities of material were obtained, including representatives of most groups of the Invertebrata. The specimens picked out were arranged in a number of large glass jars, and on the return journey Prof. Herdman gave a demonstration on the most interesting forms obtained. On Wednesday, the 18th, the second zoological excursion took place, to Wyvenhoe to inspect the Colne Oyster Fishery, by invitation of the Mayor and Corporation of Colchester. The party were taken on board the new steam oyster dredger of the Fishery Board, and hauls of the dredges were obtained at various points in the estuary of the Colne in order to show the condition of the oyster ground. Large quantities of the Polyzoon *Alcyonidium gelatinosum* and of common Ascidians, especially *Ascidiella virginea*, were found associated with the oysters. The steamer then proceeded to the Pyefleet creek, where three millions of the famous Colchester "natives" are now fattening: here the party landed and inspected the packing sheds, where they were entertained to an oyster luncheon. On returning to the steamer, dredging was again carried on further down the estuary, so as to see as much as possible of the ground, and the different ages and conditions of the oyster. Every facility was given to the party for examining this important fishery, and a most favourable impression was received of the healthiness of the ground, the purity of the water, and the excellent condition of the stock.

GEOGRAPHY AT THE BRITISH ASSOCIATION.

THE brilliant International Geographical Congress, recently held in London, seems to have afforded sufficient intellectual dissipation for most British geographers this year, and many familiar faces were absent from Section E. Comparatively few papers were presented for reading, and several of these were read by the Secretaries, as the authors could not attend. It is doubtful whether papers presented in this way should be brought before the Association, for fair discussion is impossible unless the author is present to support his arguments and answer questions.

If Section E retained its usual popularity this year—and the large lecture hall was occasionally crowded—it was not because of the sensational character of the communications made; there was not even a lady-traveller to read a paper. A characteristic of the meeting was the exceptional scientific value of the papers, which dealt less with exploration than with research.

During recent years the President of Section E has almost always been a practical geographer with a commanding knowledge of one branch of his subject, and this year the succession was worthily upheld by Mr. H. J. Mackinder, the Reader in Geography at Oxford, whose experience of higher education in geography enabled him to formulate a scheme for restoring that science to its proper place in a rational university system. The older universities have not responded as was expected to the proposals of the Royal Geographical Society as to the institution of Chairs of Geography, and the time seems to have come for the Society to take a fresh departure, either independently or in conjunction with a new university not blind to the value of the experiment which has been tried and found satisfactory in Germany. A Committee of the British Association has been appointed, without a grant, to investigate the teaching of geography in this country, Mr. Mackinder being chairman and Mr. Herbertson secretary.

The President's contention that geography is not "the science of all things," but a correlating study dealing with the results of all sciences relating to the earth from a special standpoint, was driven home by many of the papers presented to the meeting.

Mr. W. B. Blaikie demonstrated by his greatly-improved cosmosphere the astronomical relations of geography, the combination of a terrestrial globe with a transparent celestial globe on which the constellations are printed, forming a great advance on the old armillary sphere; while the ingenious device of removing a celestial and terrestrial hemisphere allowed of the working of plane problems on the section as readily as of spherical problems on the surface of the outer sphere.

Climatology was discussed in the report of the Committee on the climate of tropical Africa, which was presented by Mr. Ravenstein, the chairman. It shows the results already obtained from the six stations in tropical Africa equipped by the Association. The Committee was reappointed with a small grant and with a change of secretary, Mr. H. N. Dickson taking the place of Dr. H. R. Mill.

Dr. John Murray gave a sketch of the central problem of oceanography—the circulation of the oceans; and the Section instructed the President to write a letter to Dr. Murray, congratulating him on the completion of the *Challenger* Reports, the most important contribution to physical geography of recent years.

Mr. H. N. Dickson summarised the result of the recent international observations on the North Atlantic, in which he took part, and by the aid of lantern diagrams showed that the distribution of the temperature of the surface-water was intimately associated with the distribution of mean atmospheric pressure over the ocean, and that consequently the temperature of the Atlantic water was an important factor in determining the weather as well as the climate of Western Europe.

Mr. A. Trevor Battye read an interesting paper on the struggle for existence in Arctic regions, dealing with biogeographical problems, but unfortunately there was no time to discuss it. A biological discussion which greatly pleased the audience, but was perhaps somewhat inappropriate to the Section, arose on Mr. Borchgrevink's paper describing his recent experiences in the far South, and a proposed plan for a private Antarctic expedition. Sir Joseph Hooker, the veteran of Ross's Antarctic voyages, who was received with great enthusiasm, referred to his adventures in the Antarctic seas, and while congratulating Mr. Borchgrevink on his work in the Norwegian whaling trip, expressed little hope of great results following a private expedition. Sir William Flower had the meeting with him in declaring that no more attempts should be made to send out ships on the pretext of looking for whales or seals, but with the hope of gaining scientific information.

The return to Vardö of the *Windward*, after landing Mr. Jackson in Franz-Josef Land, occurred during the meeting, and Mr. Montefiore, Secretary of the Jackson-Harmsworth expedition, gave a brief account of the start of the land party.

In the historical aspect of geography, Mr. J. L. Myres contributed a discussion of the maps of Herodotus, which enabled an interesting contrast to be drawn between the *à priori* methods of the ancient world and the scientific inductions of to-day.

The papers descriptive of exploration dealt with Africa and Asia. Captain Hinde's experiences in the Congo State, and Mr. G. F. Scott-Elliot's admirable expedition for the scientific study of the Ruwenzori region, have already been before the public in other forms. Mr. H. S. Cowper's journey through Tarhuna and Gharian in Tripoli was new, and the archæological features which he observed seem to be deserving of further study.

The Rev. W. Weston gave one of the most valuable travel-papers—an account of his explorations in the Japanese Alps. This range occupies the centre of the largest island, with summits rising to elevations of over 10,000 feet. The snowfall on the western side is enormous on account of the moisture in the prevailing wind, while the eastern side of the range remains comparatively free of snow. Although the snow-line in summer is as low as 7000 feet in places, there are no signs of glacial action. The volcanic mountains abound in hot mineral springs of high repute as baths, and ores of copper and silver are mined in several places. The flora and fauna are both rich, and the people retain their ancient politeness and hospitality, while many curious customs and beliefs survive amongst them.

Mr. John Dodd, who was not able to be present, sent an exhaustive memoir on Formosa, where he had resided from 1864 to 1890. As a trader he had been much in contact with the aboriginal tribes of the interior, and he gave a graphic account of

their mode of life and their relations with the Chinese colonists. The resources of the island were described, and the prospects of foreign trade discussed. Probably no European is so well able as Mr. Dodd to speak from experience of the latest accession to the empire of Japan.

Dr. A. Markoff drew attention to the geography of Russian Asia, especially with reference to the Siberian railway.

Major Darwin gave an epitome of the work of the sixth International Geographical Congress.

Mr. Miller Christy directed the attention of geographers to the remote islet of Rockall, off the west coast of Scotland, which has never been properly studied, and he suggested that it would be a good field of research for a hardy yachtsman. This paper provoked a lively discussion, in which the value of Rockall as a weather-forecasting station was referred to, and the practical difficulties in the way of utilising it considered.

The Section authorised the President to write a letter of condolence to the parents of the late Mr. Joseph Thomson, expressing the high opinion universally held as to the value of the work he did in Africa, and the warm affection with which his genial personality was regarded by every geographer.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE Report just issued by the Somerset County Education Committee shows that the work of technical education is being developed, and mostly in the right direction. Much of the instruction given was of a very elementary character, but this is just what is needed by the type of student for whom it is intended. It is satisfactory to note that a course of experimental science was followed by a number of teachers. The instruction was confined almost entirely to experiments suitable for class demonstration, and, where possible, for repetition by evening school pupils. The words of Mr. C. H. Bothamley, the Director of Technical Instruction in Somerset, as to the use of such practical work, are worth repeating here. "Since the chief object of science teaching in evening schools is not so much to impart a knowledge of useful facts (though that is important), as to train pupils to use their eyes in their daily occupations, to observe accurately, and to reason correctly from what they have observed, it is clear that, if this end is to be attained, the pupils must see things for themselves, and not simply be told things, and the experimental proofs must be complete, and the reasoning based on them must be sound." We will go further and say that the only real scientific knowledge is obtained not from seeing experiments performed, but by doing them. Clear and accurate class demonstrations are undoubtedly good, but practical work carried out by the pupils themselves is far better, and the more facilities that are given for such work, the firmer will be the foundation upon which a superstructure of technical education can be built.

AMONG the recent appointments we notice the following:— Dr. A. Heydweiller, Privat-docent in Physics and Physical Chemistry at Strassburg, to be Extraordinary Professor at Breslau; Dr. Leo Gruenhut to succeed the late Prof. Borgmann at the Fresenius Chemical Laboratory, Wiesbaden; Dr. K. W. v. Dalla-Torre, Privat-docent in Zoology at Innsbruck, to be Extraordinary Professor. Dr. K. Zickler to the full Professorship of Electrotechnology at the Brünn Technical High School, and Dr. Dzieslewski to a similar post in the Technische Hochschule at Lemberg; Dr. E. Yung to succeed the late Carl Vogt as Professor of Comparative Anatomy and Zoology at Geneva; Dr. B. Weinstein to be Extraordinary Professor of Physics in Berlin University; Dr. Max Verworn to be Extraordinary Professor in Physiology at Jena; Dr. Herbert Hurst to be Demonstrator in Zoology, and Mr. Vaughan Jennings to be Demonstrator in Geology, at the Royal College of Science, Dublin; Dr. J. P. Kuenen to the new Harris Chair of Physics in University College, Dundee; Dr. Rawson to be Headmaster of Huddersfield Technical School.

AT the recent Matriculation Examination of the City and Guilds Central Technical College, seventy-six candidates presented themselves, and sixty-two have been admitted to the College. The highest place was taken by M. Solomon, to whom the Clothworkers' Scholarship of £60 a year and free education has been awarded.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 23.—M. Fizeau in the chair.—On a specimen of black diamond from Brazil, by M. Henri Moissan. The specimen is from Bahia Province, and weighs 630 grams (about 3073 carats). Its surface is in part rough, appearing when slightly magnified as if gas had escaped therefrom while in a pasty condition. It resembles the microscopic grains of crystallised carbon produced in the interior of suddenly cooled silver and iron masses. This specimen is porous, and has lost weight since removal from the soil to the extent of sixteen grams.—On the existence of phlorizic glycosuria in dogs after section of the spinal cord, by M. R. Lépine. On the administration of phlorizine, glycosuria follows almost as in the case of healthy dogs, and differs from the latter case merely in the production of a less total quantity of glucose.—A brochure entitled "The actual limits of our science; a presidential address to the British Association at Oxford, delivered August 8, 1894, by the Marquis of Salisbury" (translated by M. W. de Fonvielle), has been printed in the Correspondence of the Academy.—On the composition of pélagéine, by MM. A. B. Griffiths and C. Platt. The violet pigment of the Medusa (*Pelagia*) has the composition $C_{20}H_{17}NO_7$, and is termed by the authors pélagéine. It is soluble in alcohol, ether, and acetic acid, very soluble in carbon disulphide, and insoluble in water. It gives no characteristic absorption bands.

CONTENTS.

THURSDAY, OCTOBER 10, 1895.

LIEBIG.

Justus von Liebig: his Life and Work (1809–73). By W. A. Shenstone, F.I.C. (London: Cassell and Co., Limited, 1895.)

To those who listened—it is now twenty years ago to the Faraday Lecture given by the late Prof. Hofmann within the walls of the Royal Institution to the Fellows of the Chemical Society of London, or to those who have since read the report of this eloquent and enthusiastic discourse in the *Transactions* of the Chemical Society, the task of preparing a new account of the life and labours of Liebig would appear to be a very difficult one.

But to say merely that Mr. Shenstone has succeeded in this difficult task, would be scarcely to do justice to his admirable little volume, which has evidently been very carefully compiled, and which, while it possesses literary charm of its own, gives a clear and, at the same time, critical summary of the work and writings of the great chemist, which makes it, for popular reading at all events, preferable to Hofmann's brilliant lecture. Mr. Shenstone is evidently a master of exposition, and if in reading through the pages of his book the scientific man encounters one or two statements or expressions of opinion with which he cannot agree, he will be ready to condone these delinquencies in view of the generally excellent style of the whole. And notwithstanding the remark in the preface, that the object has been "not so much to dwell upon Liebig's private life as to tell what he was, what he did, and why all chemists and all those who are versed in the history of science admire and esteem him so greatly," the book, as a biographical sketch, is superior to the lecture. As pointed out by the author, it is quite true, and as remarkable as it is true, that few people nowadays, even among students of chemistry, know much about Liebig's scientific work and his services to the great departments of applied chemistry in physiology, medicine, and agriculture. Liebig's extract of meat, Liebig's potash bulbs, and Liebig's condenser are the only things which a present-day student can usually recall if asked to give an account of Liebig's work, and these he seems generally to regard as trivial inventions deserving of little remark. Liebig's life, cut short, as one would say in these days of general longevity, at the early age of threescore years and ten, was full of activity. The Royal Society Catalogue of Scientific Papers gives a list of upwards of three hundred papers published by him, of which some five-and-twenty were issued under joint authorship with Wöhler, his life-long friend and associate. And the *Annalen*, which to this day are familiarly referred to as "Liebig's," contain in the first 165 volumes issued during his lifetime all the long array of memoirs which embody the results of the researches of the master and his pupils.

Up to the age of sixteen, little promise of future greatness was given by the restless boy, at once "the plague of his teachers and the sorrow of his parents," as he was told by the Rector of the Gymnasium. This period of his life, marked chiefly by conflict with his schoolmasters, reminds one of Darwin's early days at Shrewsbury. And examples of this kind, of which many are now well known, fill one with wonder that the schoolmaster does not yet recognise the need for greater elasticity in the prevalent system of education.

The ideal schoolboy is an orderly machine, always obedient, receptive, submissive, ready in the cricket-field, and with real or simulated enthusiasm for football, despising all other games, and conservative to the backbone. He is the darling of the master, who sends him home with glowing reports and arms-full of prize-books. It seems never to occur to any one that there may be natures to which the classical languages and history make no appeal, who have not the gift of the mathematician, and who do not even care to play at cricket or football. If such appear in a public school they have a bad time of it, dragging out their miserable days at the bottom of the form, regarded as fools by the masters, and as muffs by the boys. And yet among these school failures there may be Liebigs or Darwins, or at any rate there may be, and commonly there is, the material out of which good and useful citizens are made, if only they had a chance to show what they can do.

It is not surprising that Germany should cherish the memory of Liebig, for to his example and influence she undoubtedly owes the development and activity of her chemical schools; and it is interesting to note the relative progress made by the chief European nations in this direction. In Liebig's youth the supremacy of the English and French chemists was unquestioned, Berzelius alone representing the science in Sweden. It was, as Liebig himself says, "a wretched time for chemistry in Germany."

Since that day things have greatly changed, the German laboratories have outnumbered those of England and France together, and their output of scientific results has so greatly exceeded the achievements of all other European countries as to have formed a subject of not undeserved reproach to the rest of them.

At the present time, however, things are not so bad, and there is great hope, from the renewed activity of the universities and technical schools in France and in England, as well as in other parts of Europe and in America during the last few years, that these other countries will in future contribute their full share to the work of experimental investigation and the encouragement of scientific education and thought.

It would be scarcely fair to the author of this "Life" to make any attempt to epitomise it, short and compact as it is. Those who are interested must read the book, and those who read it will certainly be interested. But the estimate formed by the author of the relative value and importance of the several kinds of service rendered by Liebig to the world, seems to be scarcely in agreement with that which is more generally current among chemists and physiologists. First in importance we should place Liebig's work in the domain of organic chemistry. Having shown how to analyse carbon compounds, he led the way in their investigation, and by the introduction of the theory of compound radicles laid the whole foundation of modern organic chemistry. Scarcely second in importance was the establishment of the system of practical teaching in the laboratory at Giessen, which certainly set an example soon followed by all the universities on the

continent, and led to the erection of laboratories in England, not in the great universities, to their shame be it said, but at such places as University College, London, and the College of Chemistry.

Liebig's researches in connection with physiology and agriculture were of the utmost importance in their day, but chiefly by reason of the stimulus afforded to inquiry; for while the whole, or nearly the whole, of his chemical work remains as firmly established as ever, the greater part of his physiological theories in relation to plant nutrition, to fermentation, and to animal physiology, have been either superseded altogether, or so modified as to be no longer recognisable.

The author will probably see fit, on further reflection, to alter some of the views expressed in his own remarks; but enough has been said to show that Mr. Shenstone has made a contribution to the "Century Series" which will, we venture to think, be by no means the least attractive and interesting of these useful little volumes.

W. A. T.

THE SELECTION OF HEALTH RESORTS.

Climates and Baths of Great Britain. Vol. i. (London: Macmillan and Co., 1895.)

THIS work is the outcome of the report of a committee appointed by the Royal Medical and Chirurgical Society of London for the purpose of investigating questions of importance with reference to the climatology and balneology of Great Britain and Ireland.

The information contained in the volume—which deals with the climate of the south of England and with the chief medicinal springs of Great Britain—may be summarised as follows:

(1) Information received from medical practitioners in the districts dealt with.

(2) The results of personal investigations by members of the committee.

(3) The analysis of published vital statistics of the localities in question.

That the treatment of the climatology of very small areas of these islands is a difficult and complex matter, is a fact patent to every one; it is every one's experience, for instance, that one side of a bay or headland, owing to its exposure, may be tonic and bracing, whereas the other side, owing to a different aspect, or to protection by high cliffs and woodland, may be warm and relaxing. But since meteorological data are of undoubted value in determining the suitability of an area for the residence of those suffering from various diseases, it is certain that some measure of the utility of the present work should be gauged from the detail and precision of these data; and the book will be found lacking in this respect. Little blame is attachable, however, to the contributors, who have in the majority of cases made the most of their available information; the fact is, we have not yet at hand sufficient data to enable a scientific work upon the climatic conditions of all the many small areas here dealt with to be penned; the records are so few, that it is very frequently found necessary to supplement instrumental observations by personal impressions. Thus we are constantly told that one place is *probably* colder than another, that it is thought to have more mist and moisture in the atmosphere, and so on; and one so frequently encounters such remarks as "there are no climatic records, but the impression is," &c., that the conviction is more and more borne home that it would have been well if the committee had first taken some steps, through medical men and others, to secure more scientific data before publishing the present volume. With rare exceptions, precise meteorological data are confined to towns and their immediate neighbourhood; and to show the difficulty with which the committee had to contend in the case of one important county (*i.e.* Somerset), it is sufficient to state that this county possesses at the present time only one station of the Royal Meteorological Society.

Then, again, atmospheric conditions and health are so largely the outcome of geological factors, that in a few instances it is matter for regret that this subject is not treated with a little more fulness; and in such a work one would expect to find some observations upon the mean height, and the extent of variation from the mean, in the ground-water level, knowing as we do the important bearing which this has upon health and disease.

So far as the information relates to the healthiness of the various areas treated of, and their suitability for residence by patients suffering from various diseases, much will be found of real value: but here again the contributors have had to face great difficulties—difficulties which in many respects are practically insurmountable; and here again the work presents some shortcomings. In making deductions from vital statistics, it would have been better and safer to have done so from as many returns as possible, and not to have rested satisfied, as in so many instances, with the actual records of just one brief year; and it would, moreover, have been more serviceable to those who would like to make their own deductions as to the relative advantages of different areas, if instead of the actual number of deaths being given, the rates of the more important diseases had been worked out for each locality. As it is, it would be a matter of no small labour to decide which of the many areas dealt with stands best with regard to *relative* immunity from any particular disease.

In the reports of local practitioners there is occasionally some evidence of the touch of a loving hand, the attractions and healthiness of the part being enthusiastically attested to; and for this reason, again, it will be no easy matter to conclude, from a perusal of the work, as to which is the most desirable spot to select; but at least one is not likely to fix upon Dartmoor, which an informant asserts has on an average 319 wet days in the year. Most of this local information, however, is very fair and impartial, and the conscientious and judicial manner in which conclusions are drawn by the different authors from the information at their hands is a striking feature of the work.

The committee points out that in a work comprising information of many sorts and from many sources, it is inevitable that a certain amount of error must have crept in; but as a matter of fact, the reader will discover scarcely any error of commission; what blemishes the work possesses are undoubtedly on the score of omission. There is one glaring instance of contradiction which we have noticed, and which will serve to present a good example, to the lay mind, of how doctors disagree. On

p. 38 we read. "The influence of sea air in causing anaemia is apparent on many parts of the coast," and on p. 47, "It may be stated that the infrequency of anaemia in the local inhabitants is no doubt due to their proximity to the Atlantic."

To instance the difficulty, which frequently presents itself, of arriving at just conclusions from the statistical information acquired by the committee, let us ask ourselves what inference may justly be drawn when the phthisis rate is high in certain health resorts. It is very properly pointed out that much of this excess is doubtless due to phthisical immigrants to a spot which is known to be congenial to phthisical patients. Quite true! But if we cannot ascertain *to what extent* the rate is influenced by phthisical immigration, how is one to know whether the local conditions, *per se*, are favourable or not to the disease in question? It is conceivable, in this relation, that certain limited areas of England with comparatively mild and equitable climates have now a native population strongly predisposed to phthisis from the fact that their ancestors were originally phthisical immigrants attracted to the spot; so that even if it were practicable that the vital statistics of visitors could be separately compiled, the local and climatic advantages or disadvantages of the area in respect of this disease could never be put upon a scientific basis from vital returns alone. It is well known, moreover, that deductions drawn from meteorological data on the score of the suitability of the various areas for the residence of those suffering from different diseases, must be made with many reservations, that the subject does not admit of generalisations; for, *inter alia*, the suitability of the climates of certain health resorts for different patients is governed to such an extent by that wonderful personal factor that makes the same spot bracing to one and relaxing to another, benevolent to a certain disease in one and malignant to that disease in another, that frequently the individual can only arrive at the conclusion as to which area suits him best by an actual personal experiment. And thus it comes about that perhaps, after all, the surest lines upon which a physician can act, are in the main empirical as to his patient. We have lived long enough in these islands to know by experience which are the warmest, driest, and most sheltered spots, which are the dampest, and which are the most bracing and relaxing, and it is quite a question whether meteorological data will help the physician much farther. He will generally select for his patient what has been proved by the experience of many generations to be a congenial site, and nothing short of a cautious experiment with the patient himself will suffice to tell him which of several alternative sites suits his patient best; but to this end the experiences and views of other practicising physicians would be of immense value, and one is tempted to ask whether a work embodying and summarising as many as possible of these experiences would not serve even a more useful purpose than the first 500 pages of this book.

The chapters dealing with the medicinal waters of Great Britain are well written, useful, concise and impartial.

The committee hopes to deal in a further report with the climatology of the remaining districts, and with those mineral springs which are not included in the present volume.

OUR BOOK SHELF.

Abrégé de la Théorie des Fonctions Elliptiques. Par Charles Henry. 124 pp. (Paris: Nony, 1895.)

AN introductory course of elliptic functions, intended for those who have a fair acquaintance with integral calculus, should consist of three stages. In the first stage the subject would be approached as a development of integral calculus, the addition theorem and periodicity obtained, and a large number of applications made to problems whose solutions can be expressed in the notation of elliptic functions. Difficulties of the multiple interpretation of the square roots of variable functions would be pointed out, and left. In the second stage an elementary introduction to the modern descriptive theory of functions of a complex variable would be furnished, containing a fairly full account of the theory of doubly periodic functions, illustrated at every stage by examples from the functions whose existence has been foreshadowed in the first stage. The third stage would be a systematic development of the elliptic functions, with the help of the elementary theory of functions, finishing, not beginning, with the differential equation and the applications to integral calculus. Such a course would require at least twenty-five hour-lectures, and the unfamiliar character of the second and third stages would make a careful revision necessary.

The present little volume is concerned with the third stage; on the whole, there can be no doubt that it is the most suitable handbook which has yet appeared for the use of teachers engaged in such a course as sketched above. The elliptic functions are obtained by the infinite double series for $\wp'(u)$; and certainly the idea is the right one, though it is easier to begin with the series for $\wp'(u)$. The differential equation is hence obtained, and the following chapter attempts to establish the functions on that basis. It seems preferable that this should be postponed, and treated only by Riemann's methods. Chapters iii. and iv. introduce the functions ζu and σu, as is quite proper; but it would seem much better that the addition equation, obtained in chapter v., should be obtained independently of the σ functions, and by Abel's method, with the help of a plane cubic curve. The functions $\sigma_1(u)$, $\sigma_2(u)$, $\sigma_3(u)$, are then obtained, and hence it is proved that the functions $\sqrt{\wp u - e_1}, \ldots$ are single-valued functions of u. It is a distinct step in the right direction to make the statement that these functions $\sqrt{\wp u - e_1}, \ldots$ are single-valued; but the fact ought to be obtained before, and independently of, the investigation of their actual values. The same remark holds in regard to the functions $cn\, u$, $dn\, u$: if $x = sn\, u$, it ought to be shown that $\sqrt{1-x^2}$ is single-valued before its actual value is obtained, and the remark emphasised by proving that such a function as $\sqrt{(1 - sn\, u)(1 - k\, sn\, u)}$ is equally a single-valued function of u. The fact, which is obtained, that all doubly periodic functions are rationally expressible by $\wp u$ and $\wp' u$, ought to be compared with the fact that all doubly periodic functions are rationally expressible by $sn\, u$ and $cn\, u\, dn\, u$; and it ought to be clearly seen that when we are dealing with Jacobi's functions, $cn\, u$ is no more a function of the same kind as $sn\, u$ than is $\sqrt{\wp u - e_1}$ of the same kind as $\wp u$ when we are dealing with Weierstrass's functions. In these two cases respectively, $cn\, u$ and $\sqrt{\wp u - e_1}$ are *factorial* functions, which ought to be carefully distinguished from the two fundamental functions whereby the algebraical irrationality under consideration is resolved.

With these criticisms, and the remark that the accounts of the transformation and of Jacobi's θ functions are not so full as one desires, we may conclude, strongly recommending all who desire a useful class book, to which, however, many explanations and illustrative examples must be supplied, to adopt the book. H. F. BAKER.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Clausius' Virial Theorem.

THE question raised by Colonel Basevi, in NATURE for August 29, illustrates the importance of keeping in view a clear statement of what a general theorem such as that of Clausius with respect to the *virial* asserts, and the essential relativity of the forces which are regarded as acting on the particles, and of the kinetic energy of the system. The theorem asserts, I think, that if the motion of the system of particles be continued over any interval of time, t_1, the excess of the mean value of the kinetic energy of the system for that interval of time over the virial for the same interval is equal to the excess of the value of $\frac{1}{4t_1}\Sigma m\frac{d(\rho^2)}{dt}$ at the end of the interval over its value at the beginning, ρ being the distance of a specimen particle from the origin and m its mass, and the summation being extended over all the particles of the system.

It may be noticed here that the mean value of the kinetic energy of a system for an interval of time t_1 is equal to the *action* of the system for that interval taken per unit of the time in the interval.

There can be no doubt that the theorem is true, and will be verified by any test case to which it can be applied. The proof given by Clausius himself is perhaps the simplest, but the following mode of arriving at the theorem is instructive in some ways. Refer the particles to a system of rectangular axes in the ordinary way, and adopt the fluxional notation for velocities and accelerations. Thus taking a specimen particle, which is at the point x, y, z, at time t, regarding, as we are at liberty to do, the velocities $\dot{x}, \dot{y}, \dot{z}$, as functions of the position of the particle in the motion, we have

$$m\left(\dot{x}\frac{\partial\dot{x}}{\partial x} + \dot{y}\frac{\partial\dot{x}}{\partial y} + \dot{z}\frac{\partial\dot{x}}{\partial z}\right) = m\ddot{x} = \mathrm{X}$$

and two other equations for Y, Z, which can be written down from this by symmetry. Multiplying these equations by x, y, z respectively, adding, and rearranging, we easily find

$$\frac{m}{2}\left(\dot{x}^2 + \dot{y}^2 + \dot{z}^2\right)dt = -\tfrac{1}{2}\left(\mathrm{X}x + \mathrm{Y}y + \mathrm{Z}z\right)dt + \frac{m}{2}d\left(x\dot{x} + y\dot{y} + z\dot{z}\right).$$

Integrated from $t = 0$ to $t = t_1$, and extended to all the particles, this gives

$$\tfrac{1}{2}\Sigma m\int_0^{t_1}(\dot{x}^2 + \dot{y}^2 + \dot{z}^2)dt = -\tfrac{1}{2}\Sigma\int_0^{t_1}(\mathrm{X}x + \mathrm{Y}y + \mathrm{Z}z)dt + \tfrac{1}{2}\Big[\Sigma m(x\dot{x} + y\dot{y} + z\dot{z})\Big]_0^{t_1}.$$

The expression on the left [which may be written

$$\Sigma m\int(\dot{x}dx + \dot{y}dy + \dot{z}dz)]$$

is nowhere asserted, so far as I know, to be kinetic energy, but is the time integral of the kinetic energy (that is the *action* of the system) for the time-interval t_1. Dividing both sides by t_1 we get the theorem as stated above, namely

$$\frac{1}{t_1}\int_0^{t_1}\mathrm{T}\,dt = -\frac{1}{2t_1}\Sigma\int_0^{t_1}(\mathrm{X}x + \mathrm{Y}y + \mathrm{Z}z)dt + \frac{1}{4t_1}\Big[\Sigma m\frac{d}{dt}(x^2 + y^2 + z^2)\Big]_0^{t_1},$$

where T denotes the kinetic energy of the system at the instant t.

It is clear that if t_1 be taken very great, and the velocity and the distance of each particle from the origin be always finite, the term on the left is neither infinite nor zero, while the last term on the right becomes vanishingly small. This is Clausius' case of "stationary motion", in which it is justifiable to write

$$\frac{1}{t_1}\int_0^{t_1}\mathrm{T}dt = -\frac{1}{2t_1}\Sigma\int_0^{t_1}(\mathrm{X}x + \mathrm{Y}y + \mathrm{Z}z)dt.$$

The expression on the right is the *virial*, and is in the circumstances stated undoubtedly equal to the time average or mean value of the kinetic energy, as the equation asserts.

If R be the force acting on a particle in the direction *towards* the origin along the line joining the origin with the particle, and ρ the distance of the particle from the origin, we have

$$\mathrm{X}x + \mathrm{Y}y + \mathrm{Z}z = -\mathrm{R}\rho,$$

and the theorem for stationary motion may be stated thus,

Mean value of T = mean value of $\tfrac{1}{2}\Sigma\mathrm{R}\rho$,

where the summation takes in each particle once, and once only.

Let us apply this to the case taken by Lord Rayleigh, and alleged by Colonel Basevi to contradict the theory, of two particles each of mass m, at a distance apart $r (= 2\rho)$, revolving round their common centre of gravity. Here, taking the origin at the common centre of gravity, we have constant values of the virial and of T, namely $\tfrac{1}{2}\Sigma\mathrm{R}\rho = \mathrm{R}\rho$ and $\mathrm{T} = m\mathrm{V}^2$. Thus, $m\mathrm{V}^2/\rho = \mathrm{R}$, which, as Lord Rayleigh remarks, agrees with the law of centrifugal force.

If we take the motion relatively to one of the two particles regarded as at rest, we get the same result. The relative velocity of the other particle becomes $2\mathrm{V}$, and the corresponding kinetic energy $2m\mathrm{V}^2$, the distance of the origin from the other particle 2ρ, and from itself zero. Since the acceleration of the moving particle relatively to the particle now supposed reduced to rest, is double its acceleration relatively to the common centre of gravity, the force now considered as acting on the moving particle must be taken as 2R. Thus we have $2m\mathrm{V}^2 = \tfrac{1}{2}2\mathrm{R} \times 2\rho$, or as before, $m\mathrm{V}^2/\rho = \mathrm{R}$.

If we do not suppose the origin to coincide with one of the particles reduced to rest in this manner, but to coincide for the moment with the *position* of one of the particles, the velocity of each particle is V, the force towards the origin on that distant from it r is R, and we have $\mathrm{T} = m\mathrm{V}^2$, $\tfrac{1}{2}\Sigma\mathrm{R}\rho = \tfrac{1}{2}\mathrm{R}r$, since now $\rho = r$. Hence once more $m\mathrm{V}^2/\rho = \mathrm{R}$.

Similarly, any other origin and axes of reference would give the same result. Colonel Basevi has, it seems to me, overlooked the fact that in the theorem it is the forces acting on each particle relatively to the assumed axes, and the corresponding motions that must be taken into account, and that in the case of a system of particles between which exist forces of mutual attraction, the stress between a given pair can only enter once into the value of $\tfrac{1}{2}\Sigma\mathrm{R}r$.

A. GRAY.

Bangor, September 1.

I THINK the fort will not surrender at Colonel Basevi's summons. We have

$$m\frac{d}{dt}\left(x\frac{dx}{dt}\right) = mx\frac{d^2x}{dt^2} + m\left(\frac{dx}{dt}\right)^2;$$

and if we put $x = u$ and $\frac{dx}{dt} = v$, this may be written

$$m\frac{d}{dt}(uv) = mu\frac{dv}{dt} + mv\frac{du}{dt}$$

and

$$(uv)_t - (uv)_0 = \int_0^t u\frac{dv}{dt}dt + \int_0^t v\frac{du}{dt}dt = \int_0^t udv + \int_0^t vdu,$$

if you please so to write it. This corresponds to Colonel Basevi's equation, except that I have written v for his $\dot{x}$.

But now $m\int vdu$, or $m\int v\frac{du}{dt}dt$, *does* represent kinetic energy.

And $-m\int_0^t udv$ or $-m\int_0^t x\frac{d^2x}{dt^2}dt$ is the virial. The equation shows that if for a certain time t, the right-hand member, vanishes, then on the average of that time t, the two terms on the right are equal and opposite.

The form $\Sigma\mathrm{R}r$ is a rather slippery one. If in the example which Colonel Basevi quotes from Lord Rayleigh, you put $\mathrm{X}x + \mathrm{Y}y$ for $\mathrm{R}r$, it comes out easily. For we may take for origin the centre of the circle of radius ρ. Then

$$\mathrm{X} = \frac{x}{\rho}f, \quad \mathrm{Y} = \frac{y}{\rho}f \text{ and } \mathrm{X}x + \mathrm{Y}y = f\rho.$$

And therefore

$$\Sigma\tfrac{1}{2}mv^2 = \Sigma\tfrac{1}{2}mf\rho,$$

or

$$f = \frac{v^2}{\rho}.$$

S. H. BURBURY.

COLONEL BASEVI'S criticisms of Clausius' virial theorem are not justifiable.

In the first place, the left-hand side of his equation at the foot of p. 413 should be $ux - [ux]_{t=0}$, since the latter term is not necessarily zero even for periodic motion: *e.g.* it equals 1 if $x = \exp. (\sin t)$.

In the next place, though this difference obviously vanishes for periodic motion when the "suitable value given to t" is a multiple of the period, yet for this *same* value of t the areas $\int u dx$ and $\int x du$ will *not* vanish; indeed for *no* value of t can the former vanish, as it represents $\int (dx/dt)^2 dt$, which is the sum of essentially positive quantities. Hence we can have but $\int u dx = - \int x du$ when $xu = [xu]_{t=0}$.

Thirdly, though in the case of stationary motion the areas $\int u dx$ and $- \int x du$ may not be exactly equal for any value of t, yet their difference can only fluctuate within certain narrow limits, so that when multiplied by $m/2t$ it becomes vanishingly small if t is large enough, which is all that Clausius asserts.

Fourthly, Clausius does *not* take $m \int u dx$ to represent kinetic energy, but this expression divided by $2t$.

Fifthly, the fact is overlooked that R refers, not to single particles, but to *pairs* of particles; so that in Lord Rayleigh's case, $\frac{1}{2}\Sigma \mathrm{R}r = \frac{1}{2}\mathrm{R}r$, and not $\mathrm{R}r$, as asserted, there being only *one* pair of particles in question, and the virial equation *does* therefore give $\mathrm{R} = mv^2/\rho$, the ordinary law of force for uniform circular motion.

Lastly, there is no ground whatever for taking $\frac{3}{2}\mathrm{V}r$ and $\frac{1}{2}\Sigma \mathrm{R}r$ as equal terms, there being absolutely no connection between them except that both represent energy; indeed, by this assumption Colonel Basevi obtains a formula which gives for the pressure in an ideal gas only half its proper value.

Christ Church, Oxford. ROBERT E. BAYNES.

Hutton's "Theory of the Earth."

IT is to be doubted whether any work, with the exception of Lyell's "Principles," has had a more important influence on the science of geology than Hutton's "Theory of the Earth," in which for the first time the true mode of studying the science was set forth and its fundamental facts outlined.

The theory was first propounded in a paper of some ninety-five pages, written in 1785, which appeared in 1788 in the first volume of the *Transactions* of the Royal Society of Edinburgh, and was at once attacked by a number of hostile critics.

Ten years later, in 1795, it was republished in Edinburgh, greatly extended, and including the results of much additional work, in two good-sized octavo volumes. These included the substance of a number of papers published by Hutton after the appearance of the first outline, as well as answers to his various critics, and is the work which has become a classic in the science.

The work, however, in its published form is evidently incomplete, for on the title-page it is stated to consist of four parts, and, in the table of contents, volume i. is called part i. and volume ii. part ii. Volume ii., furthermore, concludes abruptly with the following words: "Therefore in pursuing this object. I am next to examine facts, with regard to the mineral part of the theory . . . and endeavour to answer objections or solve difficulties which may naturally occur from the consideration of particular appearances."

Parts iii. and iv., so far as I can ascertain, if written, were never published. In the library of the Geological Society of London, however, there is a manuscript of Hutton's which is apparently a portion of one or other of these parts. It is bound in book form, and was presented to the library by Leonhard Horner, Esq., and in a note by that gentleman, presenting it to the Society, it is stated to be one of a series, and to have been given by Dr. Playfair, the populariser of Hutton's work, to Lord Webb Seymour, and on the death of this nobleman to have passed to the Duke of Somerset, who gave it to Mr. Horner. It bears no title, and consists of six chapters numbered from iv. to ix., and was evidently continued in another manuscript, as the last page, forming the conclusion of chapter ix., bears the words "chapter x." at the lower corner.

The manuscript treats chiefly of a subject the investigation of which has been so prolific of results in recent years, namely granite contacts, and especially the contact of granite masses with "schistus." He shows that the granite was not a "primitive" rock on which the schist was deposited, but that it was intruded through the latter in a molten condition, and holds that it was the agent by which mountain-chains were upheaved, supporting his proposition by a description of the relations of these rocks in various parts of Scotland and elsewhere, among them the Island of Arran. To the description of this last-mentioned locality a whole chapter is devoted, in which the true nature of the pitch-stones is also set forth, and the derivation of the felsites from them by a process of dentrification is recognised.

It is merely desired in the present letter to draw attention to the fact that at least some other volumes of this manuscript are extant, and to urge upon those who may know where they might be sought, or who may by chance come upon them, the importance of preserving them, and of placing them, if not in the Geological Society's library with the fragment above referred to, at least in some library where they may be at once secure and available for use.

The book is one of the most remarkable which has appeared in the history of geological science, and all who are interested in the science must desire to see it secured and preserved in its completed form. FRANK D. ADAMS.

McGill University, Montreal.

Abnormal Atlantic Waves.

IT happens that I have only quite lately seen a letter on this subject in NATURE of March 7, from Mr. E. C. Stromeyer of Glasgow. It may perhaps be of interest to some of your readers to learn that on January 6, 1891, and about 4 p.m., the people of Funchal, the chief town of Madeira, and situate on the south coast (lat. 32° 37′ 45″ N., long. 16° 55′ 20″ W.) were astonished by the arrival of a great wave which burst with violence on the shore, coming seemingly from the S.E. or E.S.E. The sea had been calm previously, and the wind was light. At Machico, a village some fifteen miles to the east of Funchal, a similar phenomenon took place contemporaneously, and also at Camara de Lobos, a village about six miles to the west. At the latter place, where there is a small bay amongst the rocks, there were three risings of the sea, one much higher than the others. The bottom of the bay was laid bare, and fishes were seen struggling in the mud. The boats lying on the beach were more or less damaged, but I did not hear that other property was injured.

Two electric cables belonging to the Brazilian Submarine Telegraph Company connect Funchal with Lisbon. Now, it is worth noting that early on the morning after the occurrence of the great wave, when the Company's officials stationed at Funchal went as usual to test the cables, one of them was found to be broken in deep water at a distance of seventeen or eighteen miles to the south of Madeira, whilst the other cable was in good working order. It is an unsolved question whether the same cause that produced the great wave had also broken the cable, or whether the two events were simply coincident but due to independent causes.

Slight shocks of earthquake are felt at distant intervals at Madeira, but no seismic disturbance was noticed near the date of the great wave.

As the wave came from the southwards, I asked a friend to make inquiry at Tenerife whether anything of the sort had been experienced there. The reply was that nothing extraordinary had occurred on January 6 at Santa Cruz on the south coast of that island. At Puerto Orotava, on the north coast, there was bright weather at the time, with light winds, and no wave had occurred, nor had any earthquake been felt.

JAMES YATE JOHNSON.

Funchal, Madeira, September 17.

Leaf-absorption.

A FEW weeks ago I threw some cuttings of the common Privet (*Ligustrum vulgare*) on the borders in the garden. Of these cuttings some perished, while the remainder were drawn into the soil by the worms, some with the cut end downwards, some only by a single leaf, leaving all the rest of the cutting *en*

... *air*. These latter are, at the end of this time, all as fresh and healthily green as they were at the moment of cutting off the parent plant, notwithstanding the very hot weather we have recently experienced. It seems to me that this is a clear proof that the *rôle* of leaves is to *absorb* as well as *evaporate*, a point on which much doubt has often been expressed. G. PAUL.

Harrogate, September 29.

IT has been proved over and over again, and it is easy to prove, that the leaves of some plants, though probably not of all, are capable, under certain conditions—usually abnormal conditions—of absorbing aqueous vapour or fluid water; but this action can hardly be regarded as a function, though I am not prepared to say that absorption of moisture by leaves is in no case a part of the every-day life of a plant. The return to turgidity of the leaves of a plant during the night is, however, in a general way, due, doubtless, to reduced transpiration, rather than absorption from the atmosphere. Yet in the absence of a counteracting current of water from the roots, the leaves of some plants, especially of those inhabiting almost rainless regions, but where the air sometimes reaches almost complete saturation, absorb moisture. At least, so it is asserted. With regard to absorption by detached leaves, or by leaves of detached branches, the development of the action depends, apart from other circumstances, on the amount of vital energy left; and this is determined, to a great extent, by age. The common Privet is a shrub of extraordinary vitality, rare in our native vegetation. I hardly need add that proof of leaves being able to absorb water may be had by inserting withered leaves in water by their upper halves, leaving the stalk out. If not too old, or too much dried, the whole leaves will regain turgidity, though the process may be a slow one.

W. BOTTING HEMSLEY.

Tertiary Fossil Ants in the Isle of Wight.

IN a paper published in NATURE for August 22, p. 399, by Prof. C. Emery, on "The Origin of European and North American Ants," the author states that "the Sicilian amber of Miocene age contains genera which belong to the actual Indian and Australian fauna, while the Baltic amber contains the genera Formica, Lasius, and Myrmica." In the Bembridge limestone in the Isle of Wight, of Eocene age, the same as the Baltic amber, the following genera occur: Formica, Myrmica, and Camponotus, and some others not yet described. In my collection there are a large number of these Hymenoptera, generally well preserved, and seem to be more numerous than any of the other insects from the same beds. It is only of late years that any number of insects have been met with in the British tertiaries, and it is well to record the two genera referred to Formica and Myrmica, being found both in the Baltic amber and Bembridge limestone. Among the numerous fossil insects in my possession from the Lias, no trace of any ants has been observed, and it seems that they did not come into existence until the later Tertiary epoch.

P. B. BRODIE.

THE NORMAL SCHOOL AT PARIS.

IN connection with the celebrations of the centenary of the foundation of the École Normale in Paris, referred to at the time in these columns (vol. lii. p. 613), a ponderous tome has been published[1] containing the complete history of the school, and details concerning the most renowned of its alumni. Opportunity is thus afforded of giving a sketch of the development of a school which has played an important part in the history of education for nearly a century, and which has been the [illegible] college of many of the most distinguished Professors in France.

The most elaborate article in the volume is a detailed history, by M. Paul Dupuy, on "L'École Normale de l'an III." This article has furnished the particulars with reference to the early history of the school given in this contribution.

When the Convention of the 9th Brumaire, An. III. (October 30, 1794), passed the decree to which the Normal School owes its foundation, it realised an idea which had occupied the attention of the University and Parliament for many years. So far back as 1645 the University of Paris considered a proposal by the rector, Dumonstier, to provide the means for the education of teachers and principals. After the expulsion of the Jesuits in 1761, the Parliament of Paris began to carry out the idea by instituting fellowships and uniting at Louis-le-Grand the scholars of the small colleges of the University. At the time when Parliament was taking these steps, Barletti de Saint-Paul was forming a training school for teachers, in which his principles of personal pedagogy were taught; and Bernardin de Saint-Pierre pleaded for a college of instruction. "J'admire avec étonnement," he wrote in 1789, "que tous les arts ont parmi nous leur apprentissage, excepté le plus difficile de tous, celui de former les hommes." To the influence which these educational reformers had in bringing the matter before the Government of the Revolution must be added the impulse derived from Germany, through Alsace. Alsace was then the only province of France able to furnish ideas and models for popular instruction. It had been touched by the great pedagogic movement in Germany, and its great influence upon the three Revolutionary Assemblies makes it prominent in the history of the Normal School.

Practically every part of the educational system of France owes its development to the Republic. The Committee of Public Welfare early concerned itself with the question of national education, and Commissions were appointed to report upon the best means for developing an educational system. In 1793 a plan was put forward to establish normal schools for the training of teachers. Nothing definite was formulated, however, with reference to the Normal School until September 1794, when the Committee of Public Instruction adopted a series of articles, the first of which was to the effect that "there should be established, at Paris, a Normal School, where instruction in the art of teaching science should be given to persons already possessing scientific knowledge." At the end of the following month, the National Convention, after a discussion of the scheme and the subjects to be taught, passed a law for the establishment of Écoles normales. The idea was to establish these schools in various parts of France, but it was not then realised, and the Normal School at Paris is the only one that owes its existence directly to the law of the Convention. Referring to the designation of the schools, an official note reads: "The word *normal*, which has been applied to the schools newly decreed, is taken from geometry. It expresses really the perpendicular or level. In the sense employed in this case it announces that all knowledge belonging to science, to the arts, to belles-lettres, &c., will there be taught, and taught to all equally." Science was thus placed upon the same footing as the humanities. The methods and results of investigation were not to be known to a few, but were to be taught by the most eminent men it was possible to obtain. The first programme of the courses and professors shows the scope of the instruction given.

Subjects.	Professors.
Mathematics	Lagrange and Laplace.
Physics	Haüy.
Descriptive Geometry	Monge.
Natural History	Daubenton.
Chemistry	Berthollet.
Agriculture	Thouin.
Geography	Buache and Mentelle.
History	Volney.
Morals	Bernardin de St. Pierre.
Grammar	Sicard.
Analysis of the Understanding	Garat.
Literature	La Harpe.

A glance at this list will show that the professors were selected on account of their eminence in different

[1] [illegible] (Paris: [illegible])

branches of knowledge rather than for purely pedagogic ability, though the object for which the school was founded was to instruct teachers in the principles of their profession. Berthollet was the only one of the professors of science who paid any serious attention to that subject in the official programme issued to the students; his colleagues confined themselves to purely scientific matters. Methods of research appear to have formed the subjects of the lectures rather than methods of exposition and education; Lagrange and Laplace made this plain in the following announcement of their courses: "To present the most important discoveries that have been made in the domain of science, to develop the principles underlying them; to notice the acute and valuable ideas which gave birth to them; to indicate the most direct road to discovery, and the best sources where details can be obtained; to show what is still to be done, and the steps it is necessary to take; these are the objects of the Normal School, and it is from this point of view that mathematics will be taught."

On January 21, 1795, the lectures commenced at the Museum d'histoire naturelle, the amphitheatre of which had just been completed, and which was given up provisionally to the Normal School. In the presence of a large assembly, Lakanal read the decree establishing the school, and was followed by Laplace, Haüy, and Monge, each of whom read their programmes, and indicated the lines they intended to follow. But the excited state of France during this period was such that the students could not be properly disciplined. Political petitions and manifestos frequently emanated from the school, and there appears to have been an almost entire want of organisation. The excessive petulance of the students showed itself during the lectures, and especially in debates after the lectures, the subjects of which were freely discussed and criticised, to the frequent embarrassment of the professors. Eventually the debates were suppressed in the case of the science lectures. Haüy substituted the debates by laboratory work, and the professors of mathematics instituted debating societies to be managed entirely by the students, who were to mutually assist one another. These conferences were only organised for mathematics, and they appear to have been installed at the College de France, where they were held every day. The "conference" system of education is a legacy from the Normal School of the year III. of the National Convention; to that school is also largely due the place which science now occupies in the French system of education; letters and science were taught by men of equal high rank and authority, and the students selected either branch of knowledge, according to their inclinations and natural gifts. The students at the school were drawn from all parts of France, and maintained by the Republic. But the national exchequer at the time could not stand any extra drain upon its impoverished resources. It is, therefore, no matter of surprise that when the courses ended in May 1795, the school was closed.

In spite of its imperfections, the School of the Convention exerted great and beneficial influence upon the French nation. Biot, in his history of science during the French Revolution, compares the school to a "vast luminous column which rose so high from the middle of a desolate land that its great brilliancy covered France and enlightened the future." And, speaking at the Paris Academy of Sciences in 1833, Arago said, with reference to the school, "It was always necessary to go back to the École normale to find the first public instruction in descriptive geometry. From that school the instruction passed, almost without modifications, to the École polytechnique. From the École normale also dates a veritable revolution in the study of pure mathematics. The demonstrations, methods, and important theories hidden in academic collections, were for the first time presented to students, and encouraged them to rebuild, on new bases, the works intended for education." Arago thus showed that, through the Normal School, science gained the right of an important place in public education. He insisted upon another point none the less important, viz. that at the Normal School, for the first time, at least officially, public education was given by the first men of intellect in the country. "With some rare exceptions, scientific investigators at one time formed in France a class totally distinct from that of the professors. By bringing the first geometers, the first physicists, the first naturalists into the professoriate, the Convention endowed the educational functions with unusual advantages, the fortunate results of which are still felt. In the eyes of the public, the school that bore the names of Lagrange, Laplace, Monge, and Berthollet could claim equality with the highest places of instruction." The first Normal School, in fact, in spite of its brief existence, founded a tradition which was preserved during the Restoration, and under the second Empire, and which has had a decisive influence upon the history of education in France. For this reason, M. Dupuy is justified in concluding his detailed history of the School of the Convention with the words: "The centenary that the École normale has celebrated this year is therefore more than the centenary of its name; it is that of the institution itself under its first form."

The second stage in the history of the Normal School began in 1808 (that is, four years after Napoleon had changed France into an Empire), with an Imperial decree establishing "un pensionnat normal, destiné à recevoir jusqu'à trois cents jeunes gens qui y seront formés à l'art d'enseigner les lettres et les sciences." This decree extending the organisation of the French University, created two years before, founded definitely the present school. Before students were permitted to enter the school, they had to agree to remain in the teaching profession at least ten years. They attended classes at the Collège de France, the École polytechnique, and the Museum d'histoire naturelle, according to whether they intended to instruct in letters, or in different branches of science. An annual grant of three hundred thousand francs (£12,000) was voted for the expenses of the school. The regulations were based upon those of the colleges of the old university, so the students were prevented from taking part in the affairs of the political world. This organisation, however, did not last long; for in 1814 there came the entrance of France by the Allies, the abdication of Napoleon, and the tragic hundred days, all of which, with later events, had their effects upon the school. Louis XVIII. proposed to change the organisations of the school and university, and a decree with this end in view was passed in February 1815. But when Napoleon returned from Elbe, a few days later, he entirely suppressed the new regulations, and re-established the Imperial University in accordance with the decree of 1808. And when the Empire finally fell, the ministers of Louis XVIII. abandoned the idea of changing the organisation, and themselves supported the Imperial system. The school existed up to 1822 under these rules, when it was decided that its place should be taken by Écoles normales partielles. Four years later the school was re-established, but in order not to excite memories of the Revolution and the Empire, it was named the "École préparatoire." Only in the name did this school differ from the old Normal School, and even that was restored by Louis Phillippe, Duke of Orleans, who, in August 1830, shortly after he became King of the French, issued an order that "the school devoted to the education of professors, and for some years carried on under the name of École préparatoire, is to reassume the title of École normale." A little later, the school was organised on the lines upon which the studies are carried there to-day. The duration of the course of study, which in the École préparatoire had been two years, was definitely fixed at three years,

and the sections of science and letters were more clearly separated than they had ever been before. After studying together during the first year, the science students, during the second and the third years, were arranged into two divisions, one of the physical and mathematical sciences, the other of natural sciences, the chemists being classified with the naturalists. In the second year the mathematicians and physicists had a few courses in common with the chemists and naturalists, but during the third year were kept altogether distinct.

The Government of Louis Philippe, which, in a way, established the fundamental system of primary instruction in France, gave the Normal School a firm standing by instituting competition and new classes; it also took steps to provide proper accommodation for the students. The buildings of the Plessis, where the studies were conducted, were falling to pieces, and it was recognised that new ones would have to be provided. In 1838 the site in the rue d'Ulm, now occupied by the school, was chosen; the plans were prepared, and money required to execute them was voted in the spring of 1841.

But six years passed before the work was done, and it was not until 1847 that the school was transferred to its new domicile, and the title of "École normal supérieure" was inscribed over the door. M. de Salvandy presided over the opening ceremony, and the director of studies, Dubois, who succeeded Cousin in 1840, read a summary of the history of the school. From that time until 1848, when Louis Napoleon became President of the French Republic, no change of importance occurred. The first event which, of the whole of the religious reactions favoured by the future Emperor of the French, foreshadowed rigorous changes in the school's regulations, was the substitution of M. Dubois by M. Michelle, rector of the Besançon Academy, in July 1850. The new director took the rank of inspector-general, and the school ceased to be represented upon the Council of the University. A year later, M. Vacherot, the director of studies, followed Dubois, and then M. Jules Simon, whose lectures were suspended at the end of 1851, resigned his connection with the school. The idea of suppressing the school altogether was afterwards seriously considered, but fortunately it was not carried into execution. Attempts were made to limit the freedom with which subjects were dealt, and, for a time, Protestants and Jews were refused admission. A better period commenced in 1857, when Nisard succeeded Michelle as the director of the school, and Pasteur became the director of scientific studies. Five science Fellowships were created in the following year, and the holders of them carried on researches under Henri Saint-Clair Deville and Pasteur, whose investigations increased the school's reputation.

After the affairs of 1870, which deposed Louis Napoleon and established the third Republic, Bersot was nominated director of the school by Jules Simon, and occupied that position until 1880. Under him, the constitution of the school was sustained, and brought back to what it was under the direction of Cousin and of Dubois. Bersot died in 1880, and the fifteen years that have elapsed since his death form the last period in the eventful history of the Normal School. M. Fustel de Coulanges was the director from 1880 to 1883, and since then the present director, M. Georges Perrot, has occupied that position. In 1880 a section of natural sciences was re-established, and this, with other improvements in the internal organisation, has assisted the school to the high place it now occupies.

The second part of the volume, from which many of the foregoing details were obtained, is taken up with biographies of the directors (each accompanied by a fine photogravure of the subject) and of papers referring to the men who have helped to develop the different departments of the school. Passing over the former section, we arrive at an account of the mathematical work at the school, by M. Jules Tannery. The high standing of this department may be judged by the fact that, of the six members of the Section of Geometry of the Paris Academy of Sciences, three belong to the Normal School. The Section of Astronomy contains two old students—one the present Director of the Paris Observatory. The school has contributed to this Academy the names of Pouillet, Delafosse, Pasteur, Jamin, V. Puiseux, P. Desains, Bouquet, Van Tieghem, Debray, Hébert, Tisserand, Fouqué, Wolf, Darboux, Troost, Mascart, Lippmann, Duclaux, Picard, Appel, and Perrier. M. Bertrand, the eminent Perpetual Secretary of the Academy, was one of the first among the illustrious men who have made the school what it is, and encouraged its students to scientific investigation. After him, Cauchy dominated mathematical education at the school. Hermite, Puiseux, Briot, and Bouquet were the close friends and disciples of this profound geometrician, who, during the early part of this century, gave mathematical science so great an impetus. Of these, only Hermite survives, and he celebrated his jubilee a few months ago. Among those who benefited by Hermite's instruction and counsel stand out the names of Baillaud, Charve, Floquet, and Pellet. Appel, Picard, and Goursat are among other students who have brought credit to their *alma mater*.

Verdet, whose electrical and optical researches are known to every physicist, became maître de conférences, that is, professor, of physics in 1848, and held that position until 1866. Mascart succeeded him for a few months, and was followed by Bertin-Mourot, who remained at the head of the physical department until 1884, since which year MM. Violle, Bouty, and Brillonin have filled the post.

Of all the teachers that the school has had, none have exercised greater influence upon it than Saint-Claire Deville. For thirty years he devoted his activities to the advancement of science at the school and to the welfare of his students. He succeeded Balard in 1851 as maitre de conférences in the section of chemistry, and at once commenced to reorganise the work and develop research. His advice to students who looked to books to supply them with subjects of investigation, was: "Fermez bien vite tous les liveres, venez au laboratoire, passez-y toute la journée, faites-y n'importe quoi, reprenez par exemple minutilusement un travail classique: vous êtes intelligent, vous ne tarderez pas à trouvez quelque résultat interressant." His numerous pupils profited by his invitation to work whenever possible in the laboratory, and many of them became his collaborators. Among these occur the names of Debray, Troost, Fouqué, Fernet, Lamy, Lechartier, Mascart, Isambert, Ditte, Joly, André, Angot, Dufet, Margottet, Chappuis, Parmentier, all of whom have advanced scientific instruction and research in France. Henri Deville never refused an investigator access to his laboratory, no matter what line of work was taken up, and the result was that not only chemists, but students of natural history, astronomy, and even an alchemist, availed themselves of the opportunity. After devoting the activities of a lifetime to science, Henri Deville died in July 1881, and by his death France lost one of its brightest lights.

Debray held a Fellowship at the Normal School when Henri Deville became the maître de conférences, and the two great investigators worked side by side for thirty years. He entered the school in 1847, and succeeded his master as professor at the Sorbonne and as maître de conférences at the school in 1875. He died in June 1888. Chemistry is at present under the charge of MM. Gernez and Joly.

The department of natural science in the school was established in 1880. The school had not existed until then, however, without paying any attention to the study of that division of scientific knowledge. M. Delafosse was maitre de conférences of zoology, botany, geo-

logy, and mineralogy so far back as 1827, and among the naturalists who taught one or other of the subjects before the new section was created were Hébert, Lory, Fouqué, Van Tieghém, Dastre, Perriér, Cornu, Giard, Le Monnier, and Bonnier. The feature of the instruction now given is the large attention paid to field work. Frequent geological, botanical and zoological excursions are made under the charge of the professors, both during the school year and the holidays. At the marine biological stations, holiday courses are always offered. Owing to the labours of Prof. de Lacaze-Duthiers, biological laboratories have been established at various points on the French coast. Since 1881, many students of the Normal School have worked at the stations at Roscoff, Banyuls, Concarneau, Wimereux, and Saint-Waast, and the knowledge they have thus gained from nature herself is far in advance of that received through lectures or from books.

Pasteur's connection with the school has a melancholy interest at the present time. Before he left the Faculty of Sciences at Lille, to become administrator and director of studies, he had made his important researches on the tartrates of soda and ammonia, and had commenced the study of fermentation. He therefore wanted a laboratory in which to continue his researches, but the school could not at the time offer him one. After a little difficulty, one small room, about ten feet square, was obtained, and in that restricted space he made some of his most valuable discoveries. This accommodation however, was gradually increased. In 1862 a large room was expressly constructed for Pasteur's work, and was added to from time to time as the value of the researches carried on came to be recognised. Finally, it was impossible for him to carry on his extensive researches under the hospitable roof in the rue d'Ulm, and he had to remove to a larger building. A few years later his work for science and humanity was recognised by the construction, at a cost of more than £100,000, raised by international subscription, of the Pasteur Institute in Paris, where the results of his researches are daily applied, and where the remains of the great investigator will finally rest.

The valuable *Annales d'École Normale* owe their commencement to M. Pasteur. The journal was first issued in 1864, and many important memoirs by members of the teaching staff, and by students, have appeared in it. Pasteur was editor from 1864 to 1870, and was succeeded by Henri Deville, who held the position until 1881, though the publication must have entailed pecuniary loss. Finally, the *Annales* were placed upon a firm footing by M. Zevort, Director of Secondary Education, who twelve years ago increased the subscription list by providing for the introduction of the journal into a number of *lycées*, and since then the assistance thus rendered has been continued by succeeding Directors of Higher and Secondary Education. M. Debray held the editorship of the *Annales* from 1882 to 1888, and M. Hermite now edits it, with an editorial committee comprising many of the most eminent men of science in France.

Many other names, in addition to those already mentioned, have contributed to the glory of the school. The work of Galois, for instance, whose short life ended in 1832, while still a student at the school, has had great influence upon the development of mathematics.

In the early part of this century, little attention was paid to astronomy at the Normal School. The mathematicians there produced a number of important memoirs on celestial mechanics, and made astronomical tables, but practical astronomy was entirely neglected. When Le Verrier became director of the Paris Observatory, he obtained permission for a limited number of students to work at the Observatory while still retaining their position in the school. Victor Puiseux and Paul Desains were the two first students selected, and they were succeeded by Paul Serret and Marié-Davy. Le Verrier thus opened a new career for students at the school, and the way they availed themselves of it is shown by the fact, that, in 1866, there were as many as fifteen of them upon the Observatory staff. Among the names of astronomers who were students at the school, are MM. Tisserand, Rayet, André, Angot, Stéphan, Simon, and Voigt; and at one time or another the school has provided directors for all the State observatories in France.

What more need be said? The names and works of the school's alumni are known and honoured throughout the scientific world, and that is sufficient testimony to the character of the instruction. The French Government is generous in its treatment of the school, but the expenditure is returned increased a hundredfold through the works of the students. And not only do these works benefit the Republic; they have an international value. Therefore the centenary which the school celebrated this year, interests all who are concerned with the advancement of natural knowledge.

R. A. Gregory.

THE "GEMMI" DISASTER.

A month ago, the Swiss newspapers were full of various accounts of a destructive avalanche which took place at the Gemmi on September 11, at 4 a.m. The first report read as follows: "A large part of the Altels glacier got loose and slipped down, covering three kilometres of ground on the Spital Alp, two hours' walk above Kandersteg. Men (6) and cattle (300) have been killed by the slipped mass. The break across the glacier may be seen from the valley with the naked eye. Help has been sent up from the villages of Leuk, Kandersteg, and Frutigen." (*Allg. Schw. Ztg.*, September 12.) More correct details afterwards decreased the loss of cattle by about one half, and the whole damage is estimated at from 60,000 to 80,000 francs.

The part concerned will be perfectly familiar to many English travellers. Few foot-tourists in Switzerland miss the Pass of the Gemmi, which bridges the beautiful limestone mountains between Canton Bern and Canton Wallis at their western end. The tourist coming from the North leaves the broad Aare Valley of Canton Bern and its lakes at Thun, and ascends gradually through the lateral Kander Valley towards the glaciated chain of the Diablerets, Oldenhorn, Wildstrübel and Altels on the southern horizon. The characteristic group of snowy summits known as the Blümlis Alp closes in the southeastern. The valley itself is bestrewn with gigantic remnants of old mountain-slips, now clad with fir-tree and a rich flora. At Kandersteg it narrows, long moraines fringe the mountains, and the driving-road is left for a a steep winding footpath which climbs the mountain-sides beneath the shade of densely-grown larch and fir. The main stream hurls over rocky escarp and raves in deep ravine. A sudden opening in the wood discloses the tributary stream of the Gastern, its grey cliffs, and tumbling waterfalls; surely one of the most picturesque glens in the Alps!

Immediately beyond this point of view, the path descends slightly for a short distance and bends round the base of a wooded hill, known as the *Stierenbergli*, before it once more rises to the mountain pasturage and châlets of the *Spital*. Here, the sound of cow-bells rings over a grassy river-flat, hemmed in east and west by mountain ridges, northward by a thick tongue of moraine. Only one steep, narrow passage defiles the northern rocks and marks the contact of the Altels range with the moraine tongue. A dammed-up lake basin, often dry in summer, lies on the other side of the moraine where the road leads to the cosily-sheltered Schwarenbach Inn. Three-quarters of an hour's farther walk on rocky shelving

ground takes the tourist past the Daubensee to the height of the Gemmi Pass and the Hotel.

Such was the walk to the Gemmi before the avalanche occurred. Now the broad pasturage flat, the narrow defile above it to the Schwarenbach Inn, as well as several passages of the road below, especially the "Stierenbergli," lie beneath masses of ruin and disorder. Fir-slopes have been felled at one blow. Dismembered parts of cattle have been floated hither, thither, in the ice-stream. What makes it the sadder is that all had been in readiness on the [illegible] Alp for the departure of the herd-boys and [illegible] the following day to their villages in Wallis.

[illegible] Altels M[illegible] D[illegible] and fragments of chalet in the ice-stream.

[illegible] the cause of the disaster was the fall of the ice-mass, it must not be forgotten that the actual destruction [illegible] wind pressure ("Windschlag") in [illegible] of the falling mass. Living things and timber are [illegible] to the ground, or borne to considerable distances. The mass of ice then buries all beneath tons of weight, and transports still farther, tearing and breaking [illegible].

The enormous rush of wind, together with the terrifying sounds of the avalanche, gave the people of the neighbourhood a rough awakening from their night's rest. They thought an earthquake was convulsing them. Only one witnessed the coming of the avalanche, that was the waitress at the Schwarenbach Inn, who had just risen to prepare an early cup of coffee for some of the guests. She rushed out, in time to see the ice skimming the road's corner on its way to destroy the Spital Alp. Had the fall taken place half a day sooner or later, tourists must inevitably have suffered on the much-frequented path.

Dr. Albert Heim, Professor of Geology at Zurich, was at once telegraphed for to make a thorough investigation of the disaster. The result of his examination will not be fully published until the end of the year. Meantime some of the more exact details may be stated here. The accompanying photographs are a few of those taken at Prof. Heim's wish immediately after the disaster.

The first shows the break in the ice on the Altels Mountain. It occurred near the foot of the nevé or "Firn-snow" region of the Altels glacier, at a height of 3300 metres (11,000 feet). The mass of ice which broke away measured about 300 metres in length, 200 metres in breath, and 30 metres in thickness. It streamed down the steep-dipping, smooth slabs of limestone rock on Altels, and spread itself out fan-like on the Spital Alp, 1900 metres high (6270 feet). The vertical height of the fall was therefore some 4700 feet. The immense impetus thus gained caused the ice to pursue its course up the steep incline of the "Oeschinen Grat." The main part in the centre of the avalanche "fan" dashed itself with its spray of ice-dust and débris against the ridge, surmounted it in parts as high as 2360 metres, over 7700 feet, and pitched many fragments upon different levels on the other, or Oeschinen Valley, side of the ridge. The outer wings of the fan, on the other hand, curved backwards; that on the north side can be traced as a return stream from Winteregg to the Stierenbergli corner of the Gemmi road referred to above (Fig. 2).

This return stream did especial damage to the trees; and nothing can be more striking than the sight of the long larch and fir trunks felled in one and the same direction, and clean-cut along a definite line. One hillock has been stripped of its timber on one side, while no harm has been done on the other. The course of the avalanche has left its trail of stems; up torn roots, ravaged châlet, dead cattle, even cheeses may be distinguished jammed in the general heaps of ruin.

The result on the ice of its own motion and pressure during its fall deserves attention (Fig. 3). The photograph shows the typical form which the ice takes, viz. that of har

rounded pieces of ice of all sizes mixed in loose ice powder. Friction produces various markings on the rolled ice.

There is altogether a remarkably small proportion of carried *rock*-débris mixed with the ice. The whole field of ice on the Spital Alp simply portrays a "Staub Lawine," or dust avalanche on a large scale. In the course of a few years nature itself will have removed the last signs of a wreckage which at present hundreds of willing hands are doing their best to clear away in part from road and Alp.

MARIA M. OGILVIE.

FIG. 2. Return stream covering the Gemmi road at the Stierenberg corner.

FIG. 3. Ice structure in the avalanche.

THE LATE PROFESSOR HOPPE-SEYLER.[1]

ERNEST FELIX IMMANUEL HOPPE[2] was born in Freiburg on the Unstrut (Saxony) on December 26, 1825. At the age of nine he lost his mother, and at eleven, being left an orphan by the death of his father, he was taken charge of and educated by the governing body of an endowed institution in Halle. After the completion of his school course he commenced in 1846 the study of the natural sciences as a student of the University of Halle. Migrating early in his student's career to Leipzig, he had the good fortune to lay the foundations of his knowledge of anatomy and physiology under the three distinguished brothers Weber (Ernst Heinrich, Wilhelm and Eduard), to study chemistry under Erdmann, and under the eminent physiological chemist Karl Gotthold Lehmann, medicine under Oppolzer, surgery under Günther, and pathological anatomy under Bock. Hoppe spent the last two semesters of his student's course in Berlin, following the courses of Romberg, Langenbeck and Casper. He took the degree of Doctor of Medicine in 1850, presenting a dissertation "De cartilaginum structura et chondrino nonnulla," which he dedicated to his former master E. H. Weber, and which indicated the impulse he had received towards anatomical as well as chemical investigation, on the one hand through the influence of the Webers, on the other through that of K. G. Lehmann.

Having settled in medical practice in Berlin, Hoppe was appointed medical officer to the workhouse, and whilst occupying this post, devoted himself to researches, partly chemico-physiological and partly clinical. To the former class belong investigations on cartilage bone and tooth, as well as chemical analyses of certain so-called transudations; to the latter, studies of which the object was to discover the physical principles which underlie many of the phenomena revealed by the percussion and auscultation of the chest in disease. In 1856, Hoppe was appointed Prosector in the University of Greisswald, where he qualified as Privat-docent: here, however, he only remained until 1858, when he was recalled to Berlin by Virchow, in order to act as his assistant. Virchow had just been appointed the first ordinary professor of pathological anatomy in the University, and Hoppe, as his only assistant, was at first called upon to take a part in all the work of the Pathological Institute, whether anatomical or chemical. Very soon, however, he was enabled to confine his attention to researches in physiological and pathological chemistry, and to the superintendence of the chemical laboratory of the Insti-

[1] Though some weeks have elapsed since the death of this eminent man of science, a brief account of his life and an attempt to convey some idea of the part which he played in the advancement of physiological chemistry may not prove uninteresting to the readers of NATURE. In the preparation of this paper I have been greatly assisted by the information contained in an article which appeared in the *Vossiche Zeitung* of August 12.—A. G.

[2] The subject of this notice changed his name from Hoppe to Hoppe-Seyler somewhere about the year 1862.

tute. In 1860, Hoppe was appointed an extraordinary professor in the philosophical faculty of the University of Berlin. In 1861 (he had now assumed the name of Hoppe-Seyler) he was appointed to the chair of Applied Chemistry in the University of Tübingen, where he had as colleagues the botanist von Mohl, the physiologist Vierordt, the anatomist Leydig, the chemist Strecker, and the great physician Niemeyer.

It was whilst in Tubingen that Hoppe-Seyler published (1866–1870), under the title of "Medicinisch-Chemische Untersuchungen," a series of valuable papers by his pupils and himself, some of which will be always referred to by thorough students of physiological chemistry; such are the researches of Diakonow on lecithin, of Miescher on nuclein, and Hoppe-Seyler's own papers on hæmoglobin, its compounds and certain of its derivatives.

When, in 1872, after the conclusion of the Franco-German war, the German Government gave to Strasburg the new and splendidly-endowed Kaiser Wilhelm's Universitat, Hoppe-Seyler was one of the distinguished men chosen to fill its chairs, being appointed to the only ordinary professorship of Physiological Chemistry in the German empire. Among those who were called with him, and who were destined to shed a brilliant lustre on the new academy, which had arisen Phœnix-like out of the ashes of the old Strasburg, were such men as Waldeyer, Recklinghausen, Leyden, Gusserow, Schmiedeberg and Fluckiger. No wonder that Strasburg has already become one of the chief centres of research in Europe!

Amongst the laboratories—the so-called institutes—which are clustered around the Hospital of Strasburg, is the so-called Physiologisch-Chemische Institut, in which since his appointment Hoppe-Seyler has continued the work which he had begun in Berlin and in Tübingen, surrounded by pupils, many of whom—I shall merely name Baumann, Brieger, Kossel, and Thierfelder—have won for themselves honourable positions in contemporary science, and for their master the reputation of a great teacher, in the best sense of the term. Here Hoppe-Seyler worked until the very eve of his death. Leaving Strasburg apparently in the fulness of health and vigour to enjoy a few weeks of rest on his property by the shores of the Lake of Constance, Hoppe-Seyler was to be spared the misery of prolonged illness. Some sudden and unsuspected cardiac mischief brought to a standstill the life of a man of singularly great activity, intellectual as well as physical. He died on the forenoon of August 10, 1895.

Arthur Gamgee.

THE FUNERAL OF PASTEUR.

AMID signs of national sorrow, the funeral of Pasteur took place on Saturday last. France, more than any other nation, knows how to do honour to the memory of those who have contributed to her greatness, and by giving a national funeral, as well as taking the cost of it upon herself, she has once more shown the esteem in which she holds those who have devoted their lives to the increase of the world's knowledge and happiness. How very full was this expression may be gathered from the report of the *Times* correspondent at Paris. We read: "Quite a small army of infantry, marines, cavalry, artillery, and municipal guards, mounted and on foot; deputations from all the schools and learned societies; most of those who speak and of those who govern and command in the name of France, came to render homage to the stainless glory of this Frenchman, whose genius devoted its efforts to the whole of mankind, and who deserves the gratitude of the world, not merely for the labour which he accomplished but for the new paths which he opened to science by the fresh discoveries which he made for the benefit of mankind." Shortly after ten o'clock on Saturday morning, the troops and innumerable deputations, which had assembled in and near the Pasteur Institute, marched past before the coffin containing the body of the illustrious investigator. The funeral procession was then organised. General Saussier, surrounded by his staff, and followed by the first division of infantry, preceded the hearse, and behind him came a long line of deputations, many of which had wreaths in their centre. A number of wreaths were borne on litters, and others were carried on six cars, each drawn by a pair of horses.

"Along the route from the Rue Dutot to Notre Dame," says the *Times* correspondent, "the compact and silent crowd respectfully uncovered their heads as the hearse passed, and the two thousand soldiers and policemen, drawn up in line to keep the way clear, had absolutely nothing to do. The pall-bearers were M. Poincaré, M. Joseph Bertrand, M. Georges Perrot, Dr. Brouardel, M. Gaston Boissier, and M. Bergeron. After marching for an hour and a half along the left bank of the Seine, the procession reached the square of Notre Dame. The aspect of the Cathedral was most impressive. The presence of President Faure, the Grand Duke Constantine, Prince Nicholas of Greece, Cardinal Richard, the whole of the Diplomatic Corps, the Ministers, the Institute of France, the office-bearers of the Senate and the Chamber of Deputies, the red-robed Judges, the members of the University faculties, in orange, red, and crimson robes, and the other distinguished persons invited—all this display of official mourning was coupled with and yet eclipsed by the profound silence, the manifest grief. The immense crowd was a rare and impressive, if not a unique spectacle."

The Royal Society was represented by Mr. W. T. Thiselton-Dyer, C.M.G., Director of the Royal Gardens, Kew. At the final funeral, which will be held in connection with the Centenary of the Institute, on the 25th inst., several of the Officers and Fellows of the Society will be present, together with many delegates from other of our learned societies.

After the service in Notre Dame, the coffin containing Pasteur's remains was removed to a catafalque outside the Cathedral, and M. Poincaré delivered an oration before it, on behalf of the Government.

Thus does France venerate the memory of her noblest son. But France is not alone in her grief. The human race joins with her in mourning the loss of one who has done so much for humanity and science. The name of him to whom the world owes so much good is imperishable.

NOTES.

IN July of this year, a special Parliamentary Committee, of which Mr. Rhodes, the Premier, was a member, sat in Cape Town to consider the advisability of beginning a systematic geological survey of the Colony. The Committee, after hearing evidence, recommended the House of Assembly to appoint a standing Commission which should take charge of the work, and become in the first instance responsible for its being efficiently carried out. Parliament having accepted this recommendation, the warrant appointing the Commission has been duly drawn up and signed by the Governor of the Colony. The following gentlemen compose the Commission: the Hon. J. X. Merriman, M.L.A.; Dr. Gill, Astronomer Royal; Dr. Muir, Superintendent General of Education; Mr. Charles Currey, Under-Secretary for Agriculture; and Mr. Thomas Stewart. The three first-mentioned are Trustees of the South African Museum, Cape Town, and it is intended that the geological staff shall have its headquarters in the new museum building, which is just approaching completion. In past years a great

amount of detached work, chiefly of the nature of prospecting and reporting upon mineral occurrences, has been done in Cape Colony, while many European geologists have written papers dealing with the rocks, fossils, and in some cases the structural characters of different portions of the Colony which at various times they happened to have visited. The Commission intends, as one of its first duties, to have a bibliography of all such papers and reports published, but will at the same time have an organised systematic scheme of field work entered upon. A topographical map on a scale of two miles to an inch has already been published for about one-twelfth of the entire area of the Colony, and it is intended to utilise this for the geological details.

DR. W. S. CHURCH will deliver the Harveian oration before the Royal College of Physicians, on Friday, October 18.

PROF. RAOULT, of Grenoble University, has been awarded the prize of twenty thousand francs given biennially by one of the bodies constituting the Institute of France, and awarded this year by the Academy of Sciences.

WE regret to notice the death of Prof. A. von Bardeleben, the eminent surgeon, and for many years one of the Presidents of the Berlin Medical Society. The death is also announced of Baron Felix Larrey, member of the Paris Academy of Medicine, and author of a number of works on military surgery.

THE *Bulletin* of the Royal Gardens, Kew, announces that Sir Joseph Hooker has presented the Gardens with a replica of a portrait of the late Dr. T. Thomson, F.R.S. Dr. Thomson was the first botanist to enter the Karakoram mountains, and was for some time Director of the Calcutta Botanic Gardens.

DURING the Leyden Zoology Congress a small volume, entitled "Guide Zoologique de la Hollande," was presented to the members. This little book, containing a number of photographs, was compiled by the General Secretary to the Congress, Dr. Hoek, and is full of information on the zoological laboratories, the museums, the zoological station and the zoological gardens, as well as concerning the study and the teaching of zoology in Holland. Several chapters are, moreover, devoted to the fauna of the country.

AT last week's meeting of the Pharmaceutical Society of Great Britain, the Hanbury Medal was presented to Dr. A. E. Vogl, Professor of Pharmacology in the University of Vienna, through Count Clary, Prof. Vogl being unable to attend in person. The medal is awarded biennially in accordance with the condition of the Hanbury Memorial Fund, and the award rests with the Presidents of the Pharmaceutical Society, Linnean Society, Chemical Society, and the British Pharmaceutical Conference. The first presentation was made in 1881, the recipient being Prof. Flückiger.

AT the Royal Microscopical Society, on Wednesday, October 16, the following papers will be read:—"On the Division of the Chromosomes in the Pollen Mother-Cell of Lilium," by Prof. J. B. Farmer; "New and Critical Fungi," by G. Massee; "A Fluorescent Bacillus," by F. J. Reid.

THE inaugural lecture of the newly-instituted "Course of Scientific Instruction in Hygiene and Public Health" at Bedford College for Women, was delivered by Dr. Louis Parkes on Saturday afternoon, October 5. The course aims at promoting systematic instruction in hygiene and all those allied branches of science necessary to a thorough knowledge of sanitation and laws of health, and so qualifying women to become teachers and lecturers, and inspectors of workshops and factories where female labour is employed.

A MEETING of the Institution of Mechanical Engineers will be held on Wednesday, October 23, and Thursday, October 24, a the Royal United Service Institution, Whitehall. The chair will be taken by the President, Prof. Alexander B. W. Kennedy, F.R.S., and the following papers will be read and discussed, as far as time permits:—"The Electric Lighting of Edinburgh," by Mr. Henry R. J. Burstall; "Report on the Lille Experiments upon the Efficiency of Ropes and Belts for the Transmission of Power," translated by Prof. David S. Capper; "Observations on the Lille Experiments upon the Efficiency of Ropes and Belts for the Transmission of Power," also by Prof. Capper.

THE death of Moritz Willkomm, the eminent botanist and geographical explorer, is announced in the *Geographical Journal*. Of his life we read:—"Born in 1821, at Herwigsdorf, in the kingdom of Saxony, after 1841 he studied medicine and natural science at Leipzig. In 1844 he for the first time visited the Pyrenean peninsula, which he subsequently traversed so often, sometimes by the year together, making thorough investigations into the botanical, geognostical, and geographical relations of the country. After having, in 1852, gained some experience as teacher of botany at Leipzig, and having been called thence first to Tharandt, and afterwards, in 1868, to Dorpat, he occupied the chair of Botany at the German University at Prague from 1873 until the receipt of his pension in 1892, being at the same time Director of the Botanical Garden in that city. He did much good work by his rich botanical collections, principally from Spain and the Balearic Isles, as well as by his special botanical works dealing especially with the descriptive side of the science; whilst as a geographer he did lasting service, not only in connection with the geography of plants—in particular in South-West and Central Europe—but also by his comprehensive geographical description of Spain and Portugal; and, above all, he threw light on the geography of Austria by his excellent work on the Böhmerwald (1878), which region he was the first to throw open to science in its most inaccessible parts, still at the time clothed with primeval forest."

WITH reference to the letter by Mr. Pillsbury on "Colour Standards" (NATURE, August 22, p. 390), Mr. J. W. Lovibond writes from Salisbury:—"In justice to myself, may I be allowed to point out that the difficulties named no longer exist, since it remains as an experimental fact that the solution of every position which Mr. Pillsbury describes as desirable and lacking is now a matter of everyday routine in many laboratories and manufactories. . . . Every sensation, whether of light or colour, which can be differentiated by the vision can be matched by means of the Tintometer Standard Glasses, and defined by means of a system of colour terms; the colour sensation itself can be reproduced at any future time by simply using the matching glasses. The operation of matching a colour is so easy that in those factories where frequent changes of colour require noting, or where it is necessary to work up to a given colour, an intelligent workman is found competent to effect them."

THE current number of *Himmel und Erde* contains the concluding part of two interesting articles on scientific balloon ascents, by Dr. R. Süring, of Potsdam. The author briefly reviews all ascents since that by Jeffries and Blanchard on November 30, 1784, and shows that relatively little use has been made of the observations, probably because they have not always been free from objection, or from the fact that most ascents have been of an isolated character. The principal exceptions, among the older ascents, are the celebrated voyages of Welsh a Glaisher, and more recently those made by the Bavarians and

Russians: the latter dealing more especially with wind conditions in high and low barometric pressures. The German Society for the promotion of scientific balloon ascents, under the patronage of the Emperor, will probably obtain important results, and solve several open questions relating to cloud formation, and atmospheric electricity under various hygrometric conditions of the atmosphere.

A SUSPENSION for physical instruments free from the vibrations of the laboratory would be an inestimable boon to physicists, especially in crowded cities. At Leyden University, Prof. Einthoven mounted his delicate capillary electrometer on an iron plate floating on mercury. This device was exceedingly successful, although somewhat cumbersome and bulky, and he was thus enabled to take a photographic record of the instrument magnified 800 times. Sir G. B. Airy was in the habit of placing his artificial horizon upon a table suspended by caoutchouc bands attached to another table similarly suspended, the arrangement being repeated three times. This, however, was even more cumbersome. Now Herr W. H. Julius, in *Wiedemann's Annalen*, describes a contrivance which is both simple and effective. It consists of a small circular table suspended by three vertical wires about 6 or 8 feet long, the ends of which form the points of an equilateral triangle. A movable weight is attached to a rod projecting downwards from the centre of the table. It can be clamped in any position, so as to bring the centre of gravity of the table and the instrument into the plane of the table itself. Any lateral displacement of the upper ends of the wire will start waves down the wires, which will arrive at the table simultaneously, but will only affect it perceptibly when the period of the disturbance coincides with the period of oscillation of the table about the point of suspension. Even then the axis of the table is always strictly vertical. To clamp the oscillations peculiar to the suspension the author attached little vanes, dipping into oil or water, to the table. With a rough preliminary apparatus constructed in this manner, the author succeeded in reducing the vibrations to one-tenth of their original amplitude.

THAT sedimentation plays an important part in the purification of water, was shown as long ago as the year 1886 by Dr. Percy Frankland in the case of his laboratory experiments on the removal of micro-organisms from water. That it is a factor of great importance in the storage of water in reservoirs, was also shown by him in his investigations at the London water-works: but quite recently Dr. H. J. van 't Hoff has indicated how this now recognised process of sedimentation may be taken advantage of in the abstraction of tidal water for purposes of water-supply. It appears that the city of Rotterdam derives its water-supply from the river Maas, and that the Company's intake is situated within the tidal area of the river; the water is, however, only abstracted at particular times, *i.e.* two hours after high-water has been reached. During this period the river is at rest, and sedimentation can proceed unhindered, and Dr. van 't Hoff estimates that at least 50 per cent. of the bacteria present are [illegible] during this time of comparative stagnation. Unlike the city of Hamburg, which before and during the great cholera epidemic [illegible] tidal water from the river Elbe, and distributed it in a raw condition in Rotterdam, the Maas water is submitted to filtration before delivery. In consequence, however, of a very large demand on the resources of the water-works, [illegible] considerably higher than it should be; and this, combined with the unpleasant circumstance that [illegible] by conducting it into the river, [illegible] a bad bacterial filtrate. Dr. van 't Hoff [illegible], unfortunately, give any figures for the [illegible] very [illegible] bacteriological [illegible] Rotterdam water-works are doubtless [illegible] the improvement which takes place in this tidal water through sedimentation, rendering the raw water comparatively easy to deal with, whilst its microbial contents after the stagnation period average only from 4,000 to 10,000 per cubic centimetre, a remarkably small number for a polluted water.

MESSRS. OLIPHANT, ANDERSON, AND FERRIER are about to issue a new popular science series for children, under the title of "Science Talks to Young Thinkers." The first volume is "Nature's Story," by Mr. H. Farquhar.

THE last part of "The Natural History of Plants," by Kerner and Oliver, which Messrs. Blackie have for some months been issuing, has just appeared, and the whole of that excellent work can therefore now be obtained in volumes.

MESSRS. CASSELL AND CO. have issued the first part of a "new and revised edition" of Sir Robert Ball's "Story of the Heavens." We hope that succeeding parts have been brought up to the present state of knowledge, so that the edition will really be a revised one.

SEVERAL years ago it was intimated by a circular that Dr. Buchanan White was engaged in the preparation of a Flora of Perthshire, which he hoped to issue after a brief period of time. Dr. White's death, last December, prevented its issue by himself, but he left it in a state that permits of its immediate publication: and we are glad to notice the announcement that the book is to be issued on behalf of the Perthshire Society of Natural Science. Prof. Trail, F.R.S., has undertaken to edit it, and to preface it with a sketch of the author's life and scientific work.

A SERIES of five simply-worded books on wild flowers, by Dr. M. C. Cooke, has been published by Messrs. T. Nelson and Sons. The volumes are entitled "Down the Lane and Back," "Through the Copse," "A Stroll in a Marsh," "Around a Cornfield," and "Across a Common." Written in an attractive conversational style, and with scanty use of the "hard words" which children, and even those of older growth, always associate with the study of nature, the books are well suited to the juvenile public for whom they are intended.

WE are glad to note that the Harveian oration delivered by Dr. Lauder Brunton before the Royal College of Physicians last October, and printed in full in these columns at the time, has been published in the form of a handy volume by Messrs. Macmillan and Co. It will be remembered that the subject of the oration was "Modern Developments of Harvey's Work"; and those who know how well and fully Dr. Brunton treated his subject, will be gratified at the publication of the oration in a convenient form. The volume is dedicated to Sir J. Russell Reynolds, the President of the Royal College of Physicians.

WE have received the second part of Mr. J. W. Taylor's "Monograph of the Land and Freshwater Mollusca of the British Isles," from Messrs. Taylor Brothers, Leeds, and are pleased to see that the high standard of excellence to which we called attention in our notice of the first part is well sustained. The descriptive text is clear, and generally accurate, while the paper, print, and illustrations (coloured and otherwise) are all praiseworthy. The present part practically completes the consideration of the shell, and the next issue will be devoted to the animal and its organisation.

THE "Zoologisches Adressbuch," which R. Friedlander and Sohn, Berlin, have edited and published in connection with the Deutsche Zoologische Gesellschaft, will prove of very great assistance to workers in all parts of the world. The volume contains the names and address of zoologists, anatomists, physiologists, and zoo-palæontologists of all countries. The classification is according to countries, the towns of which are

arranged (with a few exceptions) in alphabetical order, while the names follow the same order. Each name is followed by a full address, and by an indication of the special branch or branches of study in which the person it designates is interested. To give an example of the scope of the contents, it may be said that under London we find the names of the professors and assistants in the zoological and kindred departments in the various colleges and medical schools: the staffs of the departments of zoology and geology in the Natural History Museum; some of the members of the Geological Survey of England and Wales; a list of the members of the Zoological Society; the names and places of meeting of a number of London and suburban scientific societies interested more or less in zoology; and lists of draughtsmen, opticians, publishers, and of taxidermists and dealers in animals. In some cases the lists are much fuller than in others, owing probably to the fact that some colleges and institutions furnished the publishers with more detailed lists than others. But though a few names are omitted from the places where one first looks for them, they can in most cases be found somewhere in the volume. Very valuable is an index of the names arranged in groups according to the subjects especially studied, and a geographical index. And, finally, the personal index at the end of the volume renders it possible to find the name, address, and special work of any zoologist entered in the work in a few moments. It is well known that the Germans excel in producing directories of the kind before us, and, so far as we can make out, the present work will sustain their reputation. Being international, the directory will help to bring together observers accumulated in widely separated regions of our globe, and so will lead to a better knowledge of the world's fauna. We congratulate Messrs. Friedländer upon the enterprise they have shown in preparing and producing such a useful work; and we hope the time is not far distant when the designations of students and investigators in the domain of physical science will be brought together in a similar directory.

THE additions to the Zoological Society's Gardens during the past week include a Black Ape (*Cynopithecus niger*) from Celebes, presented by Mr. Frank Greswolde Williams; a Rhesus Monkey (*Macacus rhesus*, ♂) from India, presented by Mr. H. Small; a Bonnet Monkey (*Macacus sinicus*, ♀), a Macaque Monkey (*Macacus cynomolgus*, ♀) from India, presented by Mrs. Lionel Smith; a White-tailed Ichneumon (*Herpestes albicauda*), two Blotched Genets (*Genetta tigrina*) from Natal, presented by Mr. W. Champion; a Cape Hyrax (*Hyrax capensis*), two Suricates (*Suricata tetradactyla*) from South Africa, presented by Mr. J. E. Matcham; two Norwegian Lemmings (*Myodes lemmus*) from Norway, presented by Mrs. Haig Thomas; a Passerine Parrakeet (*Psittacula passerina*) from Brazil, a Silky Cow-Bird (*Molothrus bonariensis*), a Red-crested Cardinal (*Paroaria cucullata*) from South America, presented by Mr. R. Norton; two Common Kingfishers (*Alcedo ispida*), British, presented by Mr. J. A. Clark; a Passerine Parrakeet (*Psittacula passerina*) from Brazil, a Tuberculated Iguana (*Iguana tuberculata*) from the West Indies, two Common Teguexins (*Tupinambis teguexin*) from South America, deposited.

OUR ASTRONOMICAL COLUMN.

MEASUREMENT OF PLANETARY DIAMETERS.—In a paper giving particulars of measurements of the polar diameter of Mars (*Astronomical Journal*, No. 354), Prof. Campbell gives an interesting summary of the conditions of planetary measures in general. He points out that measurements of diameter are affected by a variety of errors, among them being spherical and chromatic aberration, imperfect atmospheric conditions, irradiation, diffraction, and imperfect focus, all of which tend to increase the apparent diameter of the object; while, in addition, personal equation and accidental errors may also affect the results. The effects of spherical and chromatic aberration, as well as of diffraction, may be regarded as constant throughout a series of measures of any given object. Differential refraction can be satisfactorily corrected for, but the irregular refraction caused by the unsteadiness of the atmosphere, and resulting in "poor seeing," may produce very large errors indeed. The apparent increase of diameter due to irradiation may be regarded as sensibly constant with any given telescope, eyepiece, planet, and background. Imperfect focus may produce considerable and variable errors: in the 36-inch Lick telescope, an error of a thousandth of an inch in focussing increases the diameter of a planet by 0″·02. Experiments as to the best method of procedure were made by Prof. Campbell in June and July, 1894, with the result that the following programme was adopted in the case of Mars: (*a*) All the observations were made with the sun above the horizon, and the advantages of a bright sky background were very marked; it was believed to reduce all the errors, except possibly that of personal equation. (*b*) Observations were only made in a tranquil atmosphere. (*c*) The same eyepiece was used throughout. (*d*) An eyepiece cap with a very small aperture was employed. (*e*) The observer's eyes were always similarly situated with respect to the threads of the micrometer. (*f*) The micrometer threads were always placed parallel to the great circle passing through Mars and the sun. (*g*) The micrometer threads were placed directly upon the opposite limbs of the planet.

Following this programme, and adopting Young's value of 1/219 for the polar compression, the most probable polar diameter of Mars, at distance unity, was found to be 9″·25 ± 0″·012, while the equatorial diameter resulting from the measures was 9″·30.

THE CRATERS ON THE MOON.—Much has been learnt about the configurations of the lunar surface since the idea of examining very greatly enlarged photographs came into practice. It was only natural, however, that many interested in the subject should have looked upon the interesting results of Dr. Weinek with scepticism, for it was hard to believe that such detail structure could be so perfectly secured on the photographic plates. Such doubts as to their existence were somewhat increased by the fact that many details were invisible to eye observations, or at any rate were thought to be, but the fact was not sufficiently grasped that the photographic plates showed only the detail as it appeared at the moment of the exposure, which might have differed considerably from that which preceded it or, followed it by a few seconds.

Every confidence is now placed in the photographic records, and under suitable and similar observing conditions the eye should be ably to verify them directly. M. C. M. Gaudibert, in *Astr. Nach.* No. 3310, tells us of his discovery, with an instrument of 200 m.m. aperture, by eye observations alone, of a small crater only 800 metres in diameter. It lies on the top of the central mountain of Albategnius. This crater has been subsequently found by Dr. Weinek on a negative taken by MM. Loewy and Puiseux 1894, February 13, 4h. 0m. Mean Time Paris.

A diligent search by M. Gaudibert has also enabled him to secure the necessary observational conditions to see the two small craters discovered by Weinek near the crater and to the east of the Rephees mountains.

SUGGESTION FOR ASTRONOMICAL RESEARCH.—Dr. Isaac Roberts draws attention to a piece of useful astronomical work which may be performed by those who take a practical interest in the subject, namely, to determine what changes, if any, have taken place among the stars in the regions photographed by him at intervals during the past eight years. In the first instance it will only be necessary to compare the earlier photographs published in his well-known "Photographs of Stars, Star Clusters, and Nebulæ" with the new series now appearing in *Knowledge*; but arrangements are being made which will enable investigators to refer to glass positives, or the negatives themselves, to settle any doubtful points. The photographs being enlarged to the same scale, comparative measurements may readily be made by means of a *réseau* ruled on glass, and a transparent protractor will enable position angles to be determined. The scale of the photographs is such that any change of position exceeding three seconds of arc may be detected by careful measurements. "Thus, a system of astronomical research would be inaugurated, that must eventually add largely to existing knowledge."

ANTHROPOLOGY AT THE BRITISH ASSOCIATION.

ON Thursday, September 12, the President's address was delivered. The address was followed by craniological papers.

Sir W. H. Flower exhibited four skulls of the aboriginal inhabitants of Jamaica, who had disappeared before the English occupation in the seventeenth century. They resemble the Carib type, and have been more or less markedly deformed during life.

The President, in the absence of Dr. J. G. Garson, gave an account of the physical characteristics of the "New Race" lately discovered in Egypt. Some 200 skulls were secured, and parts of 400-500 skeletons. The average index of length lies between 73 and 75: the alveolar index shows three predominant types, about 94, 96, and 99·5, which are confirmed by the male and female indices taken separately, and indicate a mixture of races. The nasal index is 54: wider than the European (47), and Egyptian and Guanche types (49), which are thus excluded. The great excess, especially from one of the sites explored, of female skulls of very small capacity is explained by supposing a segregation of a part of the race, and subsequent marriage of the smaller-headed women into the normal branch. The well-known decrease of cranial capacity in tropical, as compared with arctic and temperate races, suggests that the new race originated in tropical Africa. But the type of skull appears to be distinct from that of the negro: and the hair which has been found is either straight or wavy.

Each afternoon of the meeting was devoted to a lantern lecture of a somewhat more popular kind than the morning's work. On Thursday the President described the remains and civilisation of the "New Race" in Egypt, whose physical features had been already examined.

Several rites were discovered this winter between Ballas and Nagada, near Thebes, of an entirely un-Egyptian character. All the pottery was hand-made, though the potter's wheel had long been known in Egypt: and though metal was not entirely unknown, the great majority of the implements were of very delicately worked flint. The long knife-blades, and the forked spear-heads with peculiar hafting, for bringing down running deer, are particularly notable. Very beautifully formed jars of hard stone, with perforated ears for suspension, are also a characteristic manufacture, and are imitated in clay with painted marbling, and also later by the native Egyptians. Extensive cemeteries have been explored, and the manner of interment has been determined: the bodies were buried on one side in a contracted posture, with many vessels and other funeral furniture, and with "a great burning" as part of the ceremony. This, and the peculiar physical type of the people seem to connect them with the ancient Amorites of Southern Palestine; while, on the other hand, they seem to have invaded Egypt from the Libyan Desert, and to belong closely to the early inhabitants of the north coast of Africa. The date of their occupation of Egypt is fixed by the interposition of their tombs between those of sixth and twelfth dynasty Egyptians: so that their presence explains the fall of the Pyramid-Building dynasty, and the gap which has been observed at this point in the sequence of Egyptian history.

On Friday, Mr. H. W. Seton-Karr exhibited a large series of flint implements from Somali-land, and of illustrative photographs. The flint is of local origin, and a number of factories has been identified.

Mr. W. J. Knowles sent a "striated flint implement" from the North of Ireland, which gave rise to some discussion as to its origin.

Mr. B. Harrison contributed a report on the plateau flints of North Kent.

Mr. H. Stopes exhibited graving tools from the terrace-gravels of the Thames Valley and Palæolithic projectiles. In discussion, however, the human workmanship of some of the specimens was called in question.

The President gave a demonstration, with numerous illustrations, of flint and metal working in ancient Egypt. The earliest implements in Egypt are of Palæolithic types, found undisturbed and deeply stained by exposure, on the surface of the desert, 800-1200 feet above the Nile Valley. More advanced workmanship, with long parallel flaking, appears in the gravels of the Nile, 30 feet above the river. No intermediate stages are known between these and the rectangular-faced flakes of the fourth dynasty. The "New Race" which overthrew the Pyramid-builders surpassed all known flint-workers in the length, flatness, and regularity of their knives, javelin-heads, and sickle-flints. Bangles and other ornaments of great delicacy were made of the same flint. Under the XII. dynasty straight-backed and curved knives, adzes, axes with lugs, scrapers and sickles of native workmanship occur: but under the XVIII. dynasty, after another period of eclipse, bronze is found to have superseded flint. Flint implements, however, of a coarser kind, continued to be used as late as the fourth century A.D.

Metal-working is first found under the III. dynasty, and copper tools are habitually used under the IV. for mason's work; copper needles were also in use. Only one sample of bronze is known of this age; the rest are of pure copper. The "New Race," though devoted to stonework, produced occasional fine copper implements; one notable dagger is of an "Ægean" type. Under XII. dynasty, copper is still predominant, and much commoner; tempered with copper oxide and with arsenic. Bronze begins with XVIII. dynasty. Silver and gold are well worked from an early period; almost absent from "New Race" graves, which, however, seem to have been rifled. Iron has not been found earlier than foreign, mostly Greek deposits of XXVI. dynasty (650-550 B.C.). Earlier supposed allusions to "iron" in inscriptions really refer to "bronze."

Mr. H. Swainson Cowper gave a lantern lecture on the Senams, or megalithic monuments of Tripoli, of which he has visited nearly sixty. Rectangular enclosures of good masonry are associated with trilithons like those of Stonehenge, but with very narrow apertures between the jambs; the height varies from 6 to 15 feet. They are erected on footing stones, and are apparently designed to hold additional superstructures of wood. The forms of the stones themselves also sometimes recall carpentry types, which in so treeless a country are remarkable. A massive stone altar, often grooved, and level with the ground, sometimes stands in front of a trilithon. The few sculptures associated with the Senams are of Roman style, with Phallic subjects; but are not necessarily contemporary with the monuments themselves. The Senams appear to have been objects of worship, and usually stand upon hill-tops. Mr. Swainson Cowper suggests that they are analogous to the "Asherah" of the Old Testament, and to similar structures represented on Babylonian cylinders.

Mr. W. J. Lewis Abbott sent a report on the Hastings kitchen midden. The fissures in the sandstone cliffs at Hastings have been used as dwellings in Neolithic times, and the refuse, containing numerous flakes, implements, and fragments of pottery, has accumulated in front of their openings.

Saturday.—Ethnology.—The tenth report of the Committee on the North-Western Tribes of Canada was presented. This Committee was appointed at the Montreal Meeting 1884, and has published, hitherto, the following important memoirs in its reports to the British Association:

Introduction (Report VII.). Sir Daniel Wilson.
Circular of Inquiry (III.).
North American Ethnology (V.). Mr. Horatio Hale.
Linguistic Ethnology (VIII.). Mr. Horatio Hale.
Physical Characteristics (VII.). Dr. Franz Boas.
The Blackfoot Indians (I.). Mr. Horatio Hale.
The Blackfoot Indians (II.). Rev. E. F. Wilson.
The Sarcee Indians (IV.). Rev. E. F. Wilson.
The Kootenay Indians (VIII.). Dr. A. F. Chamberlain.
Ethnology of British Columbia (V.). Mr. Horatio Hale.
Notes on Indians of British Columbia (IV.). Dr. Franz Boas.
Reports on Indians of British Columbia (V.-X.). Dr. Franz Boas.

The report now presented contains a further account of the physical characteristics of the tribes of the North Pacific Coast; notes on the Tinneh Tribe of Nicola Valley, by Mr. James Teit; on the Tinneh Tribe of Portland Canal, and on the Nass River Indians, by Dr. Boas; and the grammar and vocabulary of the Nisk·â and Tsetsâ'ut languages.

Much, however, remains to be done in order to give a satisfactory review of the anthropology, even of British Columbia: in particular, the influence of the tribes of Millbank Sound on their neighbours; the highly developed art of the Haida, and the complicated symbolic and conventional ornaments; and the peculiar distribution of physical types need further elucidation.

The Committee has accordingly been reappointed with a grant of £100, in order to enable Dr. Boas to continue his important investigations.

Captain S. L. Hinde read a paper on the cannibal tribes of

the Congo. Cannibalism is in his experience in this region almost universal, on the increase, and peculiarly inveterate. An extensive traffic in human flesh prevails, and slaves as well as prisoners are kept and sold for food. Even corpses are disinterred in spite of charms on the graves: the flesh is always cooked or smoked, but is not here eaten from any religious or superstitious motive. The practice of filing the front teeth is not found to be coextensive with that of cannibalism.

Mr. Darnell Davis derived the name "cannibal" from the Caribs of the West Indies, who, however, are not man-eaters; Mr. Elworthy discussed the theory of cannibalism as a means to acquire the properties of the thing eaten; and Mr. Hartland the survivals, in Europe, of ceremonial and sepulchral cannibalism.

Captain Hinde also described the pigmies of Central Africa, nomadic hunters, of less than four feet stature.

Mr. A. Montefiore gave an account of the Samoyads of the Arctic Tundras.

Reports were presented by the Committees on physical deviations of children from the normal, and on anthropometric measurements in schools.

The anthropometric laboratory, which is usually organised during meetings of the Association, was not this year available.

On Monday, Mr. Elworthy read a paper on horns of honour, dishonour, and safety. The head is the object of honour, and is adorned with symbolic attributes. Horns are symbolic of the crescent-goddess: so of divine power, protection and favour in general. Conversely, to "scorn" (French *écorner*) is to deprive of such horns and prestige. The paper gave rise to some comment. Not all horns are crescent-symbols: most were originally worn attached to skins; ornaments are decorative first, symbolic afterwards.

Mrs. Grove discussed the religious origin of dances, as forms of magic or worship. Weapon-dances arise from worship of weapons, or of an armed deity: ritual dances from the love of dancing attributed to the deity, and as the expression of exalted enthusiasm; funeral dances propitiate either death, or the departed soul. As civilisation advances, the expressions of emotion are restrained, and dances lose their meaning and popularity.

The report of the Ethnographic Survey of the United Kingdom was read by Mr. Hartland, who was followed by Mr. J. Gray with observations specially relating to East Aberdeenshire, and by Dr. Garson with similar results from Suffolk. Work has also been begun in Hertfordshire and East Anglia (by the Cambridge Sub-Committee), and is projected in Galloway, and in Caithness, Elgin, and Nairn, by Dr. Walter Gregor.

Mr. C. G. de Betham read a fully illustrated paper on the peculiarities of the Suffolk dialect, which retains an unusual number of Anglo-Saxon idioms; and on the proverbs, traditions, and folk-medicine of the district. Mr. Lingwood exhibited two young ash-trees from Needham Market, which had been split in order to pass sick children through the stem.

Mr. Clodd read a paper on the objects and method of the study of folk-lore, which was followed by a lantern lecture by Prof. A. C. Haddon, on the same subject, exhibiting a series of persons, trees, wells, and other natural objects and prehistoric monuments to which traditions are attached, and illustrating a number of games and ceremonies, in which primitive beliefs and practices are perpetuated.

On Tuesday a formal discussion took place on the results of interference with the civilisation of native races. The subject was briefly introduced by the President, and papers were contributed by Lord Stanmore, Prof. A. C. Haddon (New Guinea), Dr. Cust (India), Dr. H. O. Forbes (Dutch East Indies), Messrs. E. Im Thurm and Darnell Davis (British Guiana), Ling Roth (Tasmania and Australia), and Raynbird (Central India). The course of the debate was summed up by the President as follows. The principle of government should be to protect the natives against their own weakness, the evil influences of debt, and the loss of their land. Rigorous impartiality may be the greatest injustice to the natives, and it is only by dealing with them from their own sense of justice that influence can be obtained. Native customs should not be unnecessarily interfered with, and then only with careful attention to the native point of view. Laws of morality differ in various countries, and what is "right" here is "wrong" there. Changes of detail should be left to the change of native opinion, rather than be enforced by law. It is, for instance, as cruel and disastrous to dress a native of a jungle in our tight, ill-ventilated clothes, as to expose an European naked in a tropical climate. With regard to education, opinions seem to differ; the completely savage brain cannot acquire our ways of thought suddenly without excessive strain and enfeeblement; but native races differ very widely in receptivity and imitativeness. What is above all things necessary is that sympathy of fellow-feeling which at once places one man on an easy and equal footing with another, and which savage races are very quick to perceive and reciprocate.

Rev. Hartwell Jones followed with a philological contribution to the history of primitive warfare in Greece and Italy.

Dr. Garson described a skull found in Thames Valley gravel, which contains palæolithic implements, and claimed it as palæolithic on morphological grounds; supported by Mr. Stopes. Sir John Evans, Prof. Boyd Dawkins, and Mr. Myres disputed the attribution.

A large collection was exhibited of photographs illustrative of the Andamanese and their civilisation, sent by Mr. Maurice Portman.

On Wednesday, Dr. Munro gave a fully illustrated lantern lecture on the newly discovered Neolithic settlement at Butmir in Bosnia. Flint and jasper weapons were manufactured in great variety on the spot, while polished hammers and axes were brought from a distance; and black pottery, with elaborate incised angular ornaments, was extensively made. A principal feature in the site is the occurrence of irregular depressions in the basal clay below the débris. Continental observers considered these to be the floors of huts; but Signor Pigorini and Dr. Munro found traces of piles, and argued that the houses were pile-dwellings, and that the hollows were made to obtain clay for wattle-work and pottery. Sir John Evans supported the pile-dwelling theory, and suggested that dredging might explain the irregularity of the hollows.

Mr. A. J. Evans described a series of primitive European idols, with diagrams and exhibits. Beginning with the marble images of the Greek archipelago, he sketched the area over which kindred figures occur, in Italy, Sicily, Spain, Liguria; and thence into Central Europe and the shores of the Baltic, and even as far as Orkney. The Oriental origin of these figures, formerly maintained, is now strongly contested: they probably testify to an indigenous practice of burying at first actual, and subsequently substituted attendants with deceased persons. Prof. Petrie compared the Maltese seated figures with those of the "New Race" in Egypt.

Dr. Munro presented a further report on the Lake Village of Glastonbury. Amongst the relics found were examples of pottery which were, undoubtedly, highly ornamented specimens of late Celtic art. Other articles unearthed must have been imported two or three centuries before the Roman occupation. Prof. Boyd Dawkins regarded the evidence as conclusive that the Lake Village of Glastonbury might be dated from 200 B.C. to the time of the Roman occupation.

Mr. Theodore Bent contributed a paper on the natives of Southern Arabia.

The Section was closed with a hearty vote of thanks to the President.

MECHANICS AT THE BRITISH ASSOCIATION.

SECTION G, which is devoted to mechanical science, had an unusually heavy programme at the late Ipswich meeting; indeed it was rather too heavy for the majority of members, for often the proceedings were carried on before a very scanty audience. It is a question whether, in this Section at any rate, a good deal of judicious weeding could not be done. Of course it is understood that "mechanical science" shall be translated as engineering in general—and that is a very good thing, as otherwise many good papers on what is generally known as "civil engineering" would be shut out from the Association altogether but with a most benevolent desire to give all branches of applied science a hearing, one cannot help thinking it would be an advantage to every one concerned especially the authors—if some proffered contributions were returned with thanks. The fact is, an exercise of the selective faculty, and perhaps a little more callousness to the demands made by the sensitiveness of authors, would do much towards rendering the proceedings in Section G more bearable than they have been for some time past.

There was, however, a good deal that was interesting and distinctly valuable in the proceedings of the Section at this year's meeting. The pity was that it should have been often wasted

on an all but empty room. Another cause of complaint on the part of members of this Section was that the second Wednesday was a *dies non*. Doubtless very few object to a whole holiday at these meetings, but what people do find fault with is that they should be kept hard at work on Saturday, when there are pleasant excursions, to be turned adrift on Wednesday. Of course one can go home and cut the business short, and that is what many do, and the Thursday's excursions thus suffer. Indeed a conscientious member, determined to do his Section G thoroughly, was unable to go to any of Saturday's excursions, and would have to spend an idle day waiting for the Thursday's excursions. The excursions are the great feature of the Association meetings, as they bring members together and make them known to each other in a way that no other institution or society does. Possibly more has been done for the Advancement of Science by such means than by the meetings of Sections, for there are other associations which afford opportunities for the reading and discussion of papers, but none which offer the same social facilities as the British Association. When it is remembered that only two Sections met on the second Wednesday, it is a question whether it would not be of advantage to make it a rule to fix the whole day excursions for Wednesday instead of Thursday. We are aware that this would create difficulties in regard to meetings of general committees, but surely these could be overcome.

The President of Section G this year was Prof. L. F. Vernon-Harcourt, who opened the proceedings of the Section by reading his presidential address.

The first paper taken was a contribution by Major-General Webber, on light railways as an assistance to agriculture. It contained the main elements of a scheme which the author had thought out for introducing a system of light railways in Suffolk. A good deal of attention was given to the subject of gauge, which the author considered should be narrower than the standard gauge of the country, viz. 4 ft. 8½ in. There is much to be said in favour of a narrow gauge for auxiliary railways, but also much to be said against it. No doubt a narrow gauge is cheaper than a wider one, but perhaps not so much cheaper as many persons imagine. Sharper curves can also be taken with a narrow gauge, and it can be laid in position where often the broader gauge would necessitate the widening of the road. On the other hand, the standard gauge enables the waggons and trucks of the trunk lines to be run on the auxiliary railways. It may be said that a light railway demands—on the score of cheapness—that the road bed shall be of a less substantial character than that of the trunk lines; but here it is essential to bear one fact in mind. The massive permanent way of our trunk lines is required for the heavy locomotives running at high speed. With small engines and comparatively slow speed very light permanent way will carry the ordinary railway goods stock with safety. The first thing, however, which has to be done in order to facilitate the introduction of auxiliary railways in this country, is to give power to the Board of Trade to relax its own regulations.

A paper by M. A. Gobert, of Brussels, on a freezing process for shaft-sinking, was next read. In general principle the suggestion is not new. In cases where water-bearing strata is encountered in shaft-sinking, a freezing medium is caused to [illegible] in pipes. The vehicle used is ammonia, which, evaporating in the pipes, produces the freezing effect.

The next paper read was of considerable interest; it was a [illegible] by Mr. W. H. Wheeler, of Boston, on the effect of [illegible] atmospheric pressure on the tides. For many years past [illegible] has been making observations on this subject. From [illegible] of two years' tides at the Port of Boston, (excluding [illegible] when the element of wind would affect the case), he found that out of 152 observations, 61 gave results opposite [illegible] which would have been expected by the readings of the [illegible] alone; for a high barometer was frequently [illegible] by a high tide, and a low barometer by [illegible]. On the other hand it was found, with few [illegible] that when the wind blows with any force along [illegible] the same direction as the main stream of the [illegible] at all the ports along the coast will be [illegible] height given in the tide tables; and [illegible] against the flood tide, high water will be [illegible]. [illegible] to figures quoted in the [illegible] effect of wind [illegible] as to affect the tide as [illegible] 5 to 6 feet and a difference of as much as 8 [illegible] been observed between two succeeding tides. An analysis of the register of tides at Boston Dock for two years showed that 24 per cent. of the whole tides recorded were sufficiently affected by the wind to vary 6 inches from the calculated height. Thirty varied by 2 feet, seven by 3 feet, six by 3½ feet, three by 4 feet, two by 4½ feet, one by over 5 feet, and one by 6 feet 3 inches. From the observations he has made, Mr. Wheeler has deduced the approximate rule that with a given force of wind of 3 on the Beaufort scale a tide will be raised or depressed by half an inch for every foot of range. With a force of from 4 to 6, the variation may be expected to be 1 inch for every foot, with a gale from 7 to 8 it will be 1½ inches, and if the gale increases to 10 it will be 2 inches. It will be seen that the subject is one which possesses not only scientific interest, but considerable practical importance to mariners; and so far as we are aware, Mr. Wheeler is the first who has obtained quantitative results of this nature. In the discussion which followed, it was pointed out that the time element would have to be given its due value.

At the second sitting of the Section, on Friday, the 13th ult., Mr. G. J. Symons gave what was really a lecture on the autumn floods of 1894. This contribution was discussed together with a paper by Messrs. Rapier and Stoney, on weirs in rivers. Any contribution by Mr. Symons is sure to meet with a good reception at a meeting of the Association, and Mr. Stoney's work in connection with river engineering is also so well known, that it was not surprising that the attendance in the Section should be a full one when the sitting opened. The floods of November of last year, it will be remembered, were of an unusually severe character, a great part of the low-lying lands of the Thames Valley being submerged. The meteorological conditions which led to these floods were traced by Mr. Symons, and the effects stated. With regard to the latter, it would be but to repeat a long history of flooded homes, spoiled furniture, and general damage to property. The extent of course will never be known, but it was sufficient to be accounted a calamity of considerable magnitude. There were two periods of heavy rainfall quickly succeeding each other, but it was the second which was the immediate cause of damage; the first, if it had stood alone, would have been comparatively innocuous. The first period occurred at the end of October, and nearly all the additional land water caused by it had passed over Teddington Weir before the second period arrived. The November rains, however, found the earth well saturated, and the water that fell ran therefore almost wholly into the river bed, with the unfortunate results before referred to. The moral Mr. Symons chiefly strove to impress was the necessity of automatic records and communication between different divisions of a water shed, so that prompt warning might be given of a probable flood. Such precautions are taken by continental nations, but in England they are sadly neglected. The necessity for obtaining accurate data, and treating it in a systematic and scientific manner by trained observers, was well illustrated by instances given; for example, the river Mole was at its highest four days before the Thames, and if the warning thus given by nature had been heeded, much of the damage which followed might have been prevented.

The second paper gave a good description of the movable weirs which have become identified with Mr. Stoney's name, and which were so prominently brought before public notice in connection with the Manchester Ship Canal. A more recent example, and one which is better known to Londoners, is that at Richmond, where there is a half-tide lock and a series of lifting weirs. It has been claimed that if many of the fixed weirs in the Thames were removed, and these lifting weirs substituted for them, that there would be less danger from flooding of the river. How far Messrs. Rapier and Stoney go in this direction we did not gather from the paper, but such we took to be the general drift of their argument. The position was disputed during the discussion which followed, it being maintained by some speakers that even if the flow of water were absolutely unimpeded as far as Teddington Weir, the tidal portion of the river-channel is not of sufficient section to carry off all water that comes down in time of heaviest rainfall. The question is complicated by the ebb and flow of the tide, but it ought not to be impossible to arrive at a fairly definite conclusion. The matter is one which wants investigation by a competent authority, for we did not notice that any more than general statements were made in support of the alleged insufficiency of the tidal channel; and the statements, therefore, did not appear to rest on a substantial basis of fact. The problem of the utilisation of the head of water at the weirs in the Thames was also brought forward. Without

going into details, it may be said that the discussion tended to show that there is little probability of any useful work being done in this direction unless some entirely new departure in the construction of turbines be discovered. Mr. Stoney, however, in his reply to the discussion, gave a sketch of a very ingenious device by which he proposed to increase the available head in the case of its diminution by the rise of water in a river. We think, however, that something more than this will be needed before the Thames weirs become commercially successful as a source of power.

Dr. Anderson described a rotating fan he had devised, to be used in place of bellows for organ-blowing. The application was successful, as might be supposed in the case where a volume of air, large in comparison with its velocity, was required to be set in motion. A paper by Mr. Birt, on the growth of the port of Harwich, was interesting from a commercial and economic point of view, and may be taken in conjunction with a note by the President, on the Hook of Holland route.

A description of a railway up Snowdon, which is in course of construction, brought the proceedings of the day to a close.

On Saturday the proceedings commenced with the presentation of two reports by Committees of the Section: the first on standardising, and the second on coast erosion. The standardising report was of an interim character, and does not require extended notice, in prospect of being brought forward again. The coast erosion report was also presented in another Section. Mr. A. G. Lyster gave a long description of the dredging operations now going on at the mouth of the Mersey to reduce the bar which has too long been allowed to impede the navigation of our great Atlantic port.

A paper by Mr. E. Hesketh, describing a process of refrigerating by carbonic anhydride, was next taken. This was a very interesting contribution, and afforded a good example of the type of paper that should be presented to the Section. It does not, however, lend itself very easily to our present purpose, as it consisted mainly of details of construction of the machinery, which, though highly interesting, it would be impossible for us to make clear without the many illustrations by which the author explained his meaning. Another good and characteristic paper was contributed by Mr. J. Napier, who described an installation that has been carried out at Ipswich of the Hermite process of purifying sewage. Briefly stated, the process consists of passing an electric current through sea water. A part of the chlorides is converted into hypochlorite, and a deodorising agent is thus obtained. The electrolysed water is passed into the drains or sewers. The system, if worked to the full extent, as proposed by the inventor, would consist of having a separate service of the electrolysed water laid on for use in closets, house drains, &c. The system has been in use but a short time in Ipswich, and is said to promise very favourably by those who have been connected with its working.

The Monday of the meeting is always devoted by Section G to electrical engineering, and at the recent meeting the proceedings on that day, the 16th ult., were opened by a long paper from the pen of Mr. Philip Dawson, on the modern application of electricity to traction purposes. Mr. Dawson has evidently travelled much in the United States, and has there collected a vast amount of data bearing on the subject of his paper. To attempt to follow him into the details he gave in his paper would be hopeless in this report. He is a strong advocate of the trolley system of transmission, holding that it will supersede all others; and indeed experience in America goes far to bear him out in this. It is needless here to point out how great has been the progress made in the United States in tramway propulsion by electricity; but one fact stated at the meeting may be repeated, as it puts the whole matter very forcibly. It was said that it is becoming a great problem what is to be done with the horses that are being pushed out of the field by electricity. In some places they are being killed for the sake of their hides and tallow; whilst in other districts good horses were to be bought at two dollars each. The latter figure we think may be open to question, for surely a dead horse is worth more than two dollars. However, there is no doubt that electric traction has made immense strides in America, and has in great cities practically supplanted not only the horse and mule, but is fast edging out its mechanical rivals the cable and steam engine.

The next item on the agenda was a paper by Messrs. Preece and Trotter, on an improved portable photometer. This paper was listened to with great interest; Mr. Trotter illustrating his remarks by examples of the different forms and apparatus he had devised for street work. The paper began by a definition of what is meant by illumination. When light falls upon a surface, that surface is said to be illuminated. The illumination depends simply upon the light falling on the surface, and has nothing to do with the reflecting power of the surface, just as rainfall is independent of the nature of the soil. It depends also on the cosine of the angle of incidence. The lighting of streets and of buildings may be specified by the maximum and minimum illumination. The primary purpose of an illumination photometer is to measure the resulting illumination produced by any arrangements of lamps irrespective of their number, their height, or their candle-power. The instrument under notice consisted of a box, on the upper surface of which is a diaphragm of white card painted with a whitewash of magnesia and isinglass. It has one or more star-shaped perforations. Immediately below it, within the box, is a white screen capable of adjustment at different angles and two small electric lamps of different candle-power, either or both of which can be used. A portable secondary battery is used to supply them with current. The illumination of the hinged screen inside the box varies approximately as the cosine of the angle of incidence of the light from the electric lamps upon it. A handle with a pointer moving over a graduated scale is connected to the screen with a system of levers, and the inclination is so adjusted that the illumination of the screen is equal to that of the perforated diaphragm, the perforations seeming to disappear when this balance is affected. The illumination can then be read off on the scale in units of the illumination due to one standard candle at one foot distance. The object of the levers is to give an open and convenient scale. The scale is graduated by experiment, and does not depend upon the the cosine law. The colour difficulty, where arc light or daylight is to be measured, is reduced by the use of a yellow tinted diaphragm and a blue-tinted screen, the tints being selected so that the readings are the same as the mean of a large number of measurements made with white screens. By means of a graduated quadrant and a gnomon the angle and the cosine of the angle of incidence of the light from a lamp may be measured. Rules are given for deducing the height of the lamp and the slant height, and hence the candle-power of the lamp.

The discussion on this paper was of a very brief nature, and elicited no new facts of importance.

Mr. H. A. Earle read a paper on storage batteries, dealing chiefly with the chloride battery which has lately been introduced, and which, it is claimed, possesses the advantages of other and earlier types without many of the attendant disadvantages, chiefly from the fact that an oxide paste is not used. A mixture of chloride of lead and chloride of zinc is cast into small tablets, which have cast round them at high pressure a frame of antimonious lead. The subsequent elimination of the chloride and zinc leaves a porous structure of pure lead of a crystalline nature, good conductivity, and with a large surface exposed to the electrolyte. The result is a large capacity for a given weight and space occupied.

At the last sitting of the Section, held on Tuesday, the 17th ult., nine papers were read and discussed. We must deal with these very briefly. The first was by Mr. P. V. Luke, and was entitled "the field telegraph in the Chitral campaign." It was of a popular nature, and was illustrated by magic lantern. Mr. G. Johnstone Stoney explained, by the aid of the apparatus itself, a movement designed to attain astronomical accuracy in the motion of siderostats. Without the aid of diagrams it would not be possible to explain the mechanism, and we will leave it therefore for the present. A paper by Mr. F. W. Turner explained the modern process of preparing flour from the wheat berry by means of metal rollers in place of the old millstones. The paper was very interesting and treated the whole subject throughout, illustrations of the various machines used being hung on the walls. Mr. J. Southward gave an interesting description of the Linotype process of printing, describing in detail and by the aid of illustrations the really wonderful machine which has been devised for the purpose. Mr. R. E. Crompton, in a memorandum on the B. A. screw gauge for small screws, pointed out the advantage that would follow if complete uniformity were observed among manufacturers in this matter, and dwelt on the desirability of a standard plate being provided for the purpose by the Board of Trade. Mr. John Key contributed a paper describing the differences in the practice of English and foreign Government departments and registration societies in their

requirements for the provision for safety in marine boilers and engines. The want of uniformity here again is undoubted.

Lieut. B. Baden-Powell described a means he suggested for navigating the air by means of kites. He pointed out that as greater height above the surface of the earth is reached, the wind nearly always increases in force. At 1000 yards it often blows at three times the velocity that it does near the surface. He proposes to take advantage of this difference by sending one kite to the upper atmosphere, and keeping another nearer the ground. The two kites would be connected by a long line, and the weight to be carried would be attached to the line at a point nearer to the lower kite than to the higher. The lower kite would thus supply a retarding medium to the upper, so that the effect would be the same in principle, though not in degree, as if the upper kite were held to the earth by a string, and the lower kite were towed through the air by a boy running with the string in his hand. By the forces thus brought to bear both kites would be kept flying although not held to the earth by a string in the usual way, and it is thought that possibly they might be navigated in directions other than that in which the wind might be blowing. It will be seen that the author depends on the difference in velocity of currents of air at two heights; and were this difference to fail, or to become insufficient, the experimenter would come to the ground. This might prove awkward unless a clear field were provided for the descent. The suggestion however is ingenious, and no doubt many persons interested in the problem of aerial navigation would be pleased to see the author put his theories to the test of practice.

The last paper presented at the meeting was a contribution by Prof. A. E. Elliott, of Cardiff, on receiver and condenser drop. It is a subject that deserves far more consideration and discussion by members of the Section than they were able to give on hearing it read rapidly at the end of the meeting. Papers of this nature should be read at one meeting, and the discussions adjourned until another; or perhaps it would be better to distribute them two or three months before the meeting, and dispense with reading altogether. A joint meeting of Sections A and G would afford the appropriate audience for considering the subject of Prof. Elliott's memoir.

BOTANY AT THE BRITISH ASSOCIATION.

THE President (Mr. Thiselton-Dyer) exhibited photographs and specimens of a large cedar (*Cedrus Deodara*, Loud.) from Kew, which had been struck and completely shattered by lightning on August 10. It was pointed out that the main stem had been in part blown into matchwood by the violence of the shock, and branches were torn off with large portions of the trunk adhering to their base. Prof. Oliver Lodge took part in the discussion as to the probable explanation of the unusual nature of the explosion, which seemed to have been centrifugal, the stem having been disrupted from the centre, and not merely stripped superficially.

Prof. Bretland Farmer described a set of wax models illustrating the typical forms passed through, and the chief variations exhibited, by the chromosomes during the division of the nucleus in the spore-mother cells of plants. The wax employed is made of a mixture of one part of white wax, with five parts of paraffin, the melting point of which is about 50° C.

THALLOPHYTA.

Experimental studies in the variation of yeast cells, by Dr. Emil Chr. Hansen (Copenhagen). The author gave an account of his earlier and more recent investigations. Among the latter he especially dwelt on those in which, by one treatment, varieties were produced that gave more, and by another treatment less, alcohol than their parent cells. He pointed out that the observed variations could be grouped under certain rules. From his researches on the agencies and causes to which variation is due, he found that temperature was the most influential external factor.[1]

A false *bacterium*, by Prof. Marshall Ward, F.R.S.

On the formation of bacterial colonies, by Prof. Marshall Ward, F.R.S.

On the structure of bacterial cells, by Harold Wager. In this paper an account was given of the present state of our knowledge of the cells of bacteria. Reference was made to the observations of Schottelius, Migula, De Bary, Bütschli, and others. The author showed that it is possible to demonstrate in the majority of bacterial cells the presence of two substances, one of which may be regarded as protoplasmic in nature, and a second, which stains deeply when acted upon by fuchsin and kindred staining substances, and which may be regarded as nuclear. It was pointed out that this nuclear substance does not possess the structure of nuclei in the cells of higher plants.

Note on the occurrence in New Zealand of two forms of Peltoid *Trentepohliaceae*, and their relation to the lichen *Strigula*, by A. Vaughan Jennings. The *Trentepohliaceae* which form epiphyllous cell-plates are at present known only from the tropics. They have been recorded from South America, India, Ceylon, and the East Indies, but not up to the present time from New Zealand. The author gave a summary of previous literature, and described two forms found by himself in New Zealand. (1) *Phycopeltis expansa*, sp. nov. This species forms wide-spreading yellow cell-plates on the leaves of *Nesodaphne*; it bears two kinds of sporangia, and is often associated with brown fungus hyphæ growing between the cell rows, but not affecting the growth of the alga. On the other hand, when attacked by different hyphæ, the result is the formation of the lichen *Strigula*, which in Ceylon was shown by Ward to have for its algal element *Mycoidea parasitica*, Cunn. (2) *Phycopeltis nigra*, sp. nov. On leaves of *Nesodaphne* and fronds of *Asplenium falcatum*. Two distinct varieties of this species were described. The plant is never attacked by fungus hyphæ, and never takes any part in lichen formation, even when on the same leaf with *Phycopeltis expansa* and the associated *Strigula*.

BRYOPHYTA AND PTERIDOPHYTA.

On a supposed case of symbiosis in *Tetraplodon*, by Prof. F. E. Weiss. The author exhibited specimens of *Tetraplodon* from the Cuchullin Hills in Skye, where it was found plentifully on animal excreta. In September he found many of the patches mixed with an orange-coloured *Peziza*, which did not appear to have in any way injured the moss plants. The rhizoids of the moss, however, contained in many cases fungal hyphæ closely resembling those of the *Peziza*, and though present in the cells of the moss, these latter did not seem to be injured by them. He suggested that this might be an instance of symbiosis; the moss, as in the case of other green plants, making use of the fungal hyphæ to obtain its nutriment from the organic material. The ultimate proof of such a case of symbiosis would, however, necessarily depend upon culture experiments, which he understood were now being made by another observer.

Remarks on the Archesporium, by Prof. F. O. Bower, F.R.S. Prof. Bower pointed out that the recognition of the archesporium as consistently of hypodermal origin cannot be upheld, and quoted as exceptions *Equisetum*, *Isoetes*, *Ophioglossum*, and especially the leptosporangiate ferns. He laid down the general principle that the sporangia, as regards their development, should be studied in the light of a knowledge of the apical meristems of the plants in question. Where the apical meristems are stratified, the archesporium is hypodermal in the usual sense; where initial cells occur, the archesporium is derived by periclinal divisions of superficial cells. Intermediate types of meristem show an intermediate type of origin of the archesporium. He cited as an illustrative case that of *Ophioglossum*, admitting that the hypodermal band of potential archesporium, which he had previously described, does not occur always or in all species. But so far from thus giving up the case for a comparison with *Lycopodium*, he holds that as *Ophioglossum* has a single initial cell in stem and root, it would be contrary to experience to expect or demand a hypodermal archesporium. (The details will shortly be published elsewhere, with illustrations.)

On the prothallus and embryo of *Danæa*, by G. Brebner. Mr. Brebner gave an account of the prothallus and sexual organs of *Danæa simplicifolia*, Rudge, as the result of investigations made on some material from the Botanic Gardens in British Guiana. He pointed out that there is a close similarity between the *Danæa* and the other two genera of the *Marattiaceæ*, *Angiopteris* and *Marattia*, of which the prothallus has been previously described. An interesting fact was noted as regards the prothallus rhizoids, which possess a distinctly septate structure, and so far resemble a moss protonema. Possibly similar septate rhizoids may be found in the other marattiaceous genera. The development of the antheridia of *Danæa* agrees in the main with that in *Marattia* and *Angiopteris*:

[1] A [illegible] of Dr. Hansen's work will be published in the *Annals of Botany*.

the material did not allow of any developmental study of the archegonia. The concentric bundle of the primary embryonic stem shows an endodermal layer. On the whole the author found in *Danæa* a complete agreement, in all essential features, with *Angiopteris* and *Marattia*, as regards prothallus, reproductive organs, and embryo development.

PHYSIOLOGY, &c.

The localisation, the transport and *rôle* of hydroycanic acid in *Pangium edule*, Reinw., by Dr. M. Treub (Buitenzorg, Java).—Five years ago Dr. Greshoff made the remarkable discovery that the poisonous substance contained in great quantities in all the parts of *Pangium edule*, was nothing else than hydrocyanic acid. This interesting chemical discovery was the starting-point of Dr. Treub's physiological investigations. In microchemical researches hydrocyanic acid presents a considerable advantage as compared with the great majority of substances to be detected in tissues by reagents; namely, that the Prussian blue reaction, easily applicable in microchemical research, gives completely trustworthy results. The appearance of Prussian blue in a cell may be accepted as certain proof of the previous occurrence in the cell of hydrocyanic acid, no other substance producing the same reaction. The leaves prove to be the chief factories of hydrocyanic acid in *Pangium*, though there are other much smaller local factories of this substance in the tissues of other organs. The hydrocyanic acid formed in the leaves is conducted through the leaf-stalks to the stem, and distributed to the spots where plastic material is wanted. The acid travels in the phloem of the fibro-vascular bundles. Dr. Treub regards the hydrocyanic acid in *Pangium edule* as one of the first plastic materials for building up proteids; he thinks it is, in this plant, the first detectable, and perhaps the first formed product of the assimilation of inorganic nitrogen. In accordance with this hypothesis, the formation of hydrocyanic acid in *Pangium* depends, on the one hand, on the presence of carbo-hydrates or analogous products of the carbon-assimilation, and, on the other hand, on the presence of nitrates. These two points were proved, or at least rendered acceptable, by a great number of experiments made by Dr. Treub in the Buitenzorg Gardens. (The details of this investigation will be found in a paper appearing in the forthcoming number of the *Annales de iardin botanique de Buitenzorg*.)

On the diurnal variation in the amount of diastase in foliage leaves, by Prof. Reynolds Green, F.R.S. The diastase which is present in foliage leaves varies in amount during the day, being greatest in the early morning, and least after sunset. The cause of the variation has been ascertained to be chiefly, if not entirely, due to the action of the sunlight. The author showed last year, at the Oxford meeting, that diastatic extracts exposed to sunlight or electric light, without the interposition of any form of screen, have their activity largely impaired, the damage amounting sometimes to 70 per cent. Experiments made upon the living leaf of the scarlet-runner showed a similar destructive action of the light, the amount of destruction only amounting, however, to about 10 to 20 per cent. The author attributes this difference to the screening action of the proteids in the cells of the leaf.

On cross and self fertilisation, with special reference to pollen prepotency, by J. C. Willis. The time has passed for regarding self-fertilisation as being always necessarily harmful in itself, and it is now recognised as a regular feature in the life-history of many plants. There are many species of plants in which both self and cross pollination occur nearly, or quite, simultaneously, and it is very desirable to know what happens in these cases. Darwin's experiments render it probable that prepotency of foreign pollen is usual. The author's experiments have been devoted to a study of the relative chemical attraction of "own" and "foreign" pollen by the same stigma (chiefly in gelatine and agar cultures), and have given negative results. It seems probable, putting together all the various known facts, that prepotency, where it occurs, is due to actions set up after the pollen tubes have entered the stigma, these actions tending to favour the growth of the "foreign" pollen-tubes, and to check that of the "own" pollen.

PALÆOBOTANY.

The chief results of Williamson's work on the Carboniferous plants, by Dr. D. H. Scott, F.R.S. The origin and history of the late Prof. Williamson's researches on the Carboniferous flora were briefly traced. His great work, chiefly, though not entirely, contained in his long series of memoirs in the *Philosophical Transactions* of the Royal Society, consisted in thoroughly elucidating the structure of British fossil plants of the Coal period, and thus determining, on a sound basis, the main lines of their affinities.

Four of the principal types investigated by Williamson were selected for illustration the *Calamarieæ*, the *Sphenophylleæ*, the *Lyginodendreæ*, and the *Lycopodiaceæ*.

(1) The *Calamarieæ*.—Williamson's great aim, which he kept in view all through, was to demonstrate the essential unity of type of the British Calamites, *i.e.* that they are all Cryptogams, of equisetaceous affinities (though sometimes heterosporous), but possessing precisely the same mode of growth in thickness by means of a cambium, which is now characteristic of Dicotyledons and Gymnosperms. His researches have given us a fairly complete knowledge of the organisation of these arborescent Horse-tails.

(2) The *Sphenophylleæ*, a remarkable group of vascular Cryptogams, unrepresented among living plants, but having certain characters in common both with *Lycopodiaceæ* and *Equisetaceæ*, are now very thoroughly known, owing, in a great degree, to Williamson's investigations. The discovery of the structure of the fructification, absolutely unique among Cryptogams, was in the first instance entirely his own.

(3) The *Lyginodendreæ*.—The existence of this family, which consists of plants with the foliage of ferns, but with stems and roots which recall those of Cycads, was revealed by Williamson. This appears to be the most striking case of an intermediate group yet found among fossil plants.

(4) The *Lycopodiaceæ*.—Williamson added enormously to our knowledge of this great family, and proved conclusively that *Sigillaria* and *Lepidodendron* are essentialy similar in structure, both genera, as well as their allies, being true Lycopodiaceous Cryptogams, but with secondary growth in almost all cases. He demonstrated the relation between the vegetative organs and the fructification in many of these plants, and by his researches on *Stigmaria*, made known the structure of their subterranean parts. The different types of *Lepidodendron*, of which he investigated the structure, were so numerous, as to place our knowledge of these plants on a broad and secure foundation. (The paper was illustrated by lantern-slides, partly from Williamson's figures, and partly original.)

On a new form of fructification in *Sphenophyllum*, by Gra Solms-Laubach (Strassburg). Graf Solms gave a brief sketch of the history of our knowledge of the fructification of the Carboniferous genus *Sphenophyllum*. He described the type of strobilus originally named by Williamson *Volkmannia Dawsoni*, and subsequently placed by Weiss in the genus *Bowmanites*; this fructification has recently been shown by Williamson and Zeiller to belong to *Sphenophyllum*. The author proceeded to give an account of a new form of strobilus recently obtained from rocks of Culm age in Silesia; this shows certain important deviations from the fructifications previously examined. In the *Sphenophyllum* strobili from the Coal-Measures the axis bears successive verticils of coherent bracts, the sporangia are borne singly at the end of long pedicels twice as numerous as the bracts, and arising from the upper surface of the coherent disc near the axil. In the Culm species, *Sphenophyllum Römeri*, sp. nov., the bracts of successive whorls are superposed and not alternate, as described by other writers, in the Coal-Measure species: a more important feature of the new form is the occurrence of two sporangia instead of one on each sporangiophore or pedicel.

In the course of his remarks, Graf Solms referred to the unique collection of microscopic preparations of fossil plants left by Prof. Williamson; he emphasised in the strongest terms the immense importance of the collection, and pointed out how every worker in the field of Palæozoic botany must constantly consult the invaluable type specimens in the Williamson cabinets.

On English amber, by Dr. Conwentz (Danzig). The author of this paper gave an account of the Baltic and English amber, and its vegetable contents. After describing the different forms of Tertiary amber, he referred to the occurrence of succinite on the coasts of Essex, Suffolk, and Norfolk; the specimens being usually found with seaweed, thrown up by the tides. Occasionally pieces have been met with weighing over two pounds. Dr. Conwentz described the method of examining the plant fragments enclosed in amber, and compared the manner of preservation with that of recent plant sections mounted in Canada balsam. The amber was originally poured out from the roots,

stems, and branches of injured or broken trees, in the form of resin, which on evaporation became thickened, and finally assumed the form of succinite or some similar substance. For the most part the fossil resin has been derived from the stems and roots of coniferous trees of the genus *Pinus*. In addition to the exceptionally well-preserved tissues of coniferous trees, the Baltic amber has yielded remarkable specimens of monocotyledonous and dicotyledonous flowers. Some of the most striking examples were illustrated by means of the excellent coloured plates from Dr. Conwentz' monographs on the Baltic amber.

The Wealden flora of England, by A. C. Seward. Mr. A. C. Seward, after referring to the various species described by Mantell, Carruthers, Starkie Gardner, and others, from the Wealden strata of England, briefly described a large number of plants from the British Museum collection. During the last few years Mr. Rufford, of Hastings, has obtained an extremely valuable and rich collection of plants from Ecclesbourne, Fairlight, and other localities; and these have now become the property of the nation. The following species are at present known from the Wealden of England; some of these have already been figured in the first volume of the catalogue of the Wealden flora, and the remainder are dealt with in the forthcoming second volume: *Algites valdensis*, sp. nov., *A. catenelloides*, sp. nov., *Chara Knowltoni*, sp. nov., *Marchantites Zeilleri*, sp. nov., *Equisetites Lyelli*, Mant., *E. Burchardti*, Dunk., *E. Yokoyamae*, sp. nov., *Onychiopsis Mantelli* (Brong.), *O. elongata* (Geyl.), *Acrostichopteris Ruffordi*, sp. nov., *Matonidium Goepperti* (Ett.), *Protopteris Witteana*, Schenk., *Ruffordia Goepperti* (Dunk.), *Cladophlebis longipennis*, sp. nov., *C. Albertsii* (Dunk.), *C. Browniana* (Dunk.), *C. Dunkeri* (Schimp.), *Sphenopteris Fontainei*, sp. nov., *S. Fittoni*, sp. nov., *Weichselia Mantelli* (Brong.), *Tempskya Beyrichii* (Schenk.), *T. Dawsoni*, sp. nov., *Sagenopteris Mantelli* (Dunk.), *S. acutifolia*, sp. nov., *Microdictyon Dunkeri*, Schenk., *Dictyophyllum Roemeri*, Schenk., *Leckenbya valdensis*, gen. et sp. nov., *Tempskya Schimperi*, Cord., *Cycadites Roemeri*, Schenk., *C. Saportae*, sp. nov., *Dioonites Dunkerianus* (Goepp.), *D. Brongniarti* (Mant.), *Nilssonia Schaumburgensis* (Dunk.), *Otozamites Klipsteinii*, (Dunk.), *O. Goeppertianus* (Dunk.), *Zamites Buchianus* (Ett.), *Zamites Carruthersi*, sp. nov., *Anomozamites Lyellianus* (Dunk.), *Cycadolepis*, *Carpolithes*, *Androstrobus Nathorsti*, sp. nov., *Conites armatus* (Carr.), *C. [illegible]*, sp. nov., *Bucklandia anomala* (Stokes and Webb), *Fittonia Ruffordi*, sp. nov., *Bennettites Saxbyanus*, Brown, *B. Gibsonianus*, Carr., *B. (Williamsonia) Carruthersi*, sp. nov., *Yatesia Morrisii*, Carr., *Withamia Saportae*, gen. et sp. nov., *Becklesia anomala*, gen. et sp. nov., *Dichopteris*, sp., *Sphenolepidium Kurrianum* (Schenk.), *S. Sternbergianum* (Dunk.), *Pagiophyllum crassifolium* (Schenk.), *Brachyphyllum obesum*, Heer, *B. spinosum*, sp. nov., *Pinites Solmsi*, sp. nov., *P. Dunkeri*, Carr., *P. Mantelli*, Carr., *P. patens*, Carr., *P. Carruthersi*, Gard., &c.

SCIENCE IN THE MAGAZINES.

THE personal reminiscences of Huxley, contributed by Mr. George W. Smalley to the current number of *Scribner*, will bring up pleasant memories to those who were honoured by the friendship of the departed naturalist, and they form an affectionate tribute "to the memory of one of the truest men who ever lived, one of the manliest, and in all points the noblest." There is in the article so much real testimony to Huxley's greatness, that every student of science will appreciate it. "The emancipation of thought," truly says Mr. Smalley, "that is Huxley's legacy to his century—that was his continual lesson of intellectual honesty." Against those who criticised Huxley's philosophical learning we quote these words: "In truth he was a very expert metaphysician, with an extraordinary knowledge of the literature of metaphysics and philosophy. . . . Huxley was a student, and more than a student, of Descartes. He has written the best short book in existence on Hume. He was a pupil of Aristotle, and therefore not a Platonist. Hobbes taught him much; Berkeley was to him a great thinker; Locke, Butler, and the short list of really great names in English philosophy were all his familiars, while among the great Germans there was, I think, none whom he did not know well—Kant, Hegel, Fichte, and all that illustrious line, not excepting Schopenhauer." But Huxley's claims to recognition as one of the world's foremost thinkers, too unhappily lost to us, need not be enlarged upon here. "He will be remembered as the great physiologist, the great student, the great controversialist, the great thinker and writer. That he will be remembered need not be doubted. The world, it may still be said, does not willingly let die the memory of those who have made it a better world to live in, whose lives as well as whose teachings have been lessons of devotion, of high aims, of wide accomplishments, of honourable purpose; whose achievements are written imperishably in the annals of their own time. Huxley was one of these, and his monument in his life's endeavour. There will be no need to inscribe Right Honourable upon his tomb. The name he bore through life will serve both for epitaph and eulogy."

There are other articles in *Scribner* which will interest the readers of NATURE. One of these is a fully illustrated description of the new Chicago University, by Mr. Herrick. Magnificent buildings have been erected, and an endowment of over £1,200,000 has been bestowed in the short period of four years, as well as a generous annual budget for current expenses. This phenomenal generosity, together with the fact that there will be no question of adequate support as fresh opportunities for development occur, point to the University of Chicago as a great and growing centre of intellectual activity. In some respects the system of the University resembles that of our older Universities, but others—such as the emphasis placed upon the doctor's degree, investigation, research, &c., and the activity of the graduate schools—point to the German University as the prevailing influence. It will astonish many of our schoolmen to know that "the graduates in residence this year—in all over three hundred—form more than one-third of the entire body of students, a larger number than at any other American University. This preponderance of graduate students has been brought about by several reasons: the emphasis placed upon the advanced courses under the leadership of such heads of departments as Profs. Dewey, Hale, von Holst, Laughlin, Michelson and Nef, not to mention others; the special privileges and distinctions granted to graduates (for example, in many departments only graduate students are allowed in the special departmental laboratories, the £6000 annually offered in fellowships and scholarships; and the equal privileges accorded to women. It is a truism that the most distinctive move in American college life of the last decade has been in the sudden interest in post-graduate study. But hitherto in Western institutions, whether college or so-called university, has had the means to provide liberally for advanced studies." It will be clear from this quotation, and more clear from a perusal of the article, that the University of Chicago is developing in the right directions towards scholarship and new knowledge. Chicago people seem to have the cause of higher education at heart, and they are devoting their best energies, as well as generous financial support, to the magnificent institution which has so quickly sprung into existence, and which has such a great future before it.

The third article of scientific interest in *Scribner* is on "Domesticated Birds," by Prof. N. S. Shaler, and is beautifully illustrated.

The sixth of Mr. Herbert Spencer's papers on professional institutions, contributed to the *Contemporary*, deals with the evolution of men of science and philosophers, and will, therefore, be of exceptional interest to our readers. So far as the series has as yet gone, it has been shown that the institutions dealt with were probably derived from the priesthood. Whatever may be the opinion with regard to the connection between the medical profession and priestcraft, it will be generally conceded that astronomy received its first impulse from the exigencies of religious worship. Extracts given by Mr. Spencer from Rawlinson, Layard, and Maury show clearly how closely religion and science (especially astronomical science) were mingled by the Babylonians. With the Egyptians, too, there is abundant evidence to prove an intimate connection between their science and their religion; and the connection is established by the fact that "in every temple there was . . . an astronomer who had to observe the heavens." Astronomy was thus an outgrowth of religion, and the natural knowledge accumulated by the priests formed the beginnings of sciences in Egypt, Assyria, and India. The Greeks imported this knowledge; in other words, they obtained their early science in a slightly developed state. Of the indebtedness of the Greek philosophers to the Egyptian priests there is no doubt whatever, and Mr. Spencer clearly makes out that obligation. The development of Greek science, however, is only in a small measure ascribed to the priesthood, the advances being more of secular than of sacred origin. "During those centuries of darkness which

followed the fall of the Roman Empire," says Mr. Spencer, "nothing to be called science existed. But when, along with gradual reorganisation, the re-genesis of science began, it began as in earlier instances among the cultured men—the priesthood." The man of science and the philosopher have gradually differentiated from the clerical class, one to deal with the concrete and the other to be concerned with abstract matters, and now the distinction between the two is tolerably definite. Simultaneously a subdivision of the body of scientific men has gone on, until we reach these days of minute specialisation. And finally, we have the combination of the units in such institutions as the Royal Society and British Association, and in the serial scientific publications which are general in their scope. In addition to the admirable article summarised in the foregoing, Mr. Spencer contributes to the *Contemporary* a brief note in reply to Prof. Weismann.

A suggestive paper, by Dr. A. R. Wallace, on "The Expressiveness of Speech," appears in the *Fortnightly*. The paper contains a number of interesting facts which point to mouth-gesture as a factor in the origin of language. Here is Dr. Wallace's idea: "In our own language, and probably in all others, a considerable number of the most familiar words are so constructed as to proclaim their meaning more or less distinctly, sometimes by means of imitative sounds, but also, in a large number of cases, by the shape or the movements of the various parts of the mouth used in pronouncing them, and by peculiarities in breathing or in vocalisation, which may express a meaning quite independent of mere sound-imitation." Anthropologists and philologists should be interested in the many facts which Dr. Wallace has brought together in support of his view.

Limits of space prevent us from giving more than brief descriptions of the remaining articles of scientific interest in the magazines received. In *Science Progress*, Mr. F. H. Neville traces recent progress in the study of alloys; galvanotropism in tadpoles is described by Dr. A. Waller, F.R.S.; the chromatophores of animals, by Mr. W. Garstang; the space relation of animals, by Dr. A. Eiloart; and the synthesis of proteids, by Prof. W. D. Halliburton, F.R.S. *Chambers's Journal* has short popular papers on "Horseless Carriages," "New Methods of Illumination," and "Cotton-Seed Oil." In *Good Words* we notice an article on "Falconry," by Mr. R. B. Lodge, illustrated by two photographs from life—one showing a peregrine and partridge, and the other a goshawk and rabbit. The two plates are finely engraved, but we think their value would have been greater had they been photographic reproductions from the original negatives. The *Humanitarian* is distinguished by a psychical article entitled "Dynamic Thought," by Prof. W. F. Barrett; and the *National Review* has a paper in which Selbornians will find pleasure, by the Hon. Mrs. R. Boyle. In addition to the magazines named in the foregoing, we have received the *Sunday Magazine* and *Longman's*.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—Five candidates, namely, R. A. Berry, G. Joyce, H. C. Sheringham, W. M. Tod, and B. N. Wale, have been successful in the recent examination in the science and art of agriculture, and have received the University diploma.

Mr. Charles Smith, Master of Sidney Sussex College, and author of several much-used mathematical text-books, was on October 1 admitted to the office of Vice-Chancellor for the current academical year. The outgoing Vice-Chancellor, Mr. A. Austen Leigh, in his parting address to the Senate, referred in sympathetic terms to the loss sustained by the University in the death of Prof. Cayley and of Prof. Babington. He announced that the latter had bequeathed to the University his large and valuable collection of plants. A part of the address was devoted to a description of the difficulties, chiefly financial, which have attended the inception of the Sedgwick Memorial Museum of Geology. The satisfactory progress made with the extension of the Cavendish Laboratory, now approaching completion, was made a matter of congratulation.

Two scholarships in Natural Science, one of £70 and one of £40 a year, will be competed for at Sidney Sussex College on December 12 to 14. Candidates are to make preliminary application to the tutor, Mr. G. M. Edwards.

The late Prof. Babington has left to the University his botanical library as well as his valuable collection of plants.

Mr. H. F. Baker, of St. John's, and Mr. J. E. Edwards, of Sydney, have been appointed the Moderators, and Mr. R. A. Herman, of Trinity, and Mr. H. W. Richmond, of King's, the Examiners for the Mathematical Tripos of 1896.

Dr. Glaisher has been appointed an Elector to the Isaac Newton Studentship in Astronomy.

A complete series of lectures for agricultural students, under the Cambridge and Counties Agricultural Education Scheme, has been arranged for three terms of the academical year. The syllabus is published in the *University Reporter* of October 8.

The first Entrance Scholarship in Natural Science, of the value of £150, into St. Thomas's Hospital Medical School has been awarded to Mr. Frank B. Skerrett; the second, of the value of £60, being divided between Messrs. Walter B. Fry, George W. Hare, and Alfred B. Lindsey, bracketed equal. The Entrance Scholarship, value £50, for students from the Universities, has been awarded to Mr. Percy W. G. Sargent, St. John's College, Cambridge.

At Guy's Hospital, the Entrance Scholarship in Science, of the value of £150, has been awarded to Mr. P. W. L. Camps, and the Second Entrance Scholarship in Science, of the value of £60, has been awarded to Mr. S. Hodgson.

The Treasury has decided that the annual grant of which King's College, London, was deprived under the late Government may be restored to the college next year without any stipulation as regards tests.

SCIENTIFIC SERIALS.

American Meteorological Journal, September.—Synchronous or simultaneous geographical distribution of hourly wind velocities in the United States, by Dr. F. Waldo. This article is part of a memoir prepared for the U.S. Weather Bureau, and is supplementary to one which appeared in the *Journal* for July (Nature, p. 335). Charts are drawn for midnight and noon, for the extreme months of January and July, for about the centre of the United States, and afford a comprehensive view of the synchronous wind conditions and relations as regards the average velocities. This method of representation obviates the necessity of a lengthy text.—The origin and work of marine meteorology, by Lieut. W. H. Beehler, U.S.N. The author deals more particularly with the history and development of this service in the United States, from the appointment of Lieut. Maury as Superintendent of the U.S. Naval Observatory, in 1844, which led to the Maritime Conference held at Brussels in 1853. The numerous charts published by the American Office formed the basis of the useful wind charts subsequently issued by the Meteorological Department of the Board of Trade, during Admiral FitzRoy's lifetime, and which were widely distributed among seamen. The U.S. Hydrographic Office was established in 1866, and in 1893 there were nearly 3000 observers co-operating with it. The outcome of this was the publication of the Pilot Chart of the North Atlantic Ocean, to which we have often had occasion to refer. About 4000 copies of this chart are distributed monthly, and among other things they have done much towards bringing about the general recognition of the value of the use of oil to still the waves, by which numbers of vessels have been saved from total loss.

Bulletin of the American Mathematical Society (vol. i. No. 10, July 1895).—This closing number of vol. i. contains, in addition to the usual list of new publications and the index, a list of the published papers read before the Society during the year, together with the places of their publication.—Mr. J. M. Brooks gives a clear account of Lie's work on continuous groups *à propos* of Scheffers' edition of the Vorlesungen uber Continuirliche Gruppen mit geometrischen und anderen Anwendungen. "The importance of the group idea itself has long been recognised in its application to the theory of substitutions, and some continuous transformations, such as the pedal transformation, were in use before Lie's work, but were used without their connection with the group idea being discovered, and the discovery and the presentation of the results of it in a systematic form are due to Prof. Lie." Dr. Scheffers has aimed at giving in outline the general theory, and he indicates some lines in which it may be applied.—Prof. J. Harkness, in a review of the second volume

second edition) of Jordan's "Cours d'Analyse de l'ecole Polytechnique," which is devoted to the integral calculus, fully analyses its contents, and pronounces it to be "a substantially new contribution to mathematical literature." "From beginning to end the reader feels that he is being guided by a master-hand."—Prof. F. Hastings Moore writes on a theorem concerning reduced characteristics with denominator 2 (of Prym's "Untersuchungen über die Riemann'sche Thetaformel und die Riemann'sche Charakteristikentheorie," 1882).—A note on the Transitive Substitution Groups of degree 12, by Dr. G. A. Miller, mentions that Camille Jordan in the *Comptes rendus* (vol. lxxv. p. 1757) states that there are *three* primitive groups of degree 12, excluding the groups which contain the alternating group. Dr. Miller has found *four* multiply transitive primitive groups of this degree, excluding the two groups containing the alternating group. The proof is given in the present note.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, September 30.—M. A. Cornu in the chair.—The Perpetual Secretary read a letter from M. J. B. Pasteur, announcing the death of his father, Louis Pasteur, who died at Villeneuve-l'Etang (Garches), on September 28, 1895. M. A. Cornu then expressed the deep feeling of loss in the Academy, and recalled the greatness of the work accomplished by Pasteur. As a mark of respect and sorrow the Academy adjourned after receiving the correspondence. Remarks on the subject of Lord Salisbury's discourse "on the real limits of our science," by M. Emile Blanchard. The author recalls his work in contradiction to the theory of the origin of species advanced by Darwin, and maintains that no single instance has ever been brought forward in answer to his challenge which can be held to verify the assumption that one species may be produced from another by any form of selection.—On glycosuria following ablation of the pancreas, by M. R. Lépine. The sugar contents of the urine have been determined, and glycosuria traced during the first thirty hours, operating on dogs without the use of anæsthetics or morphine.—A study of the mechanical theory of heat, by M. Ch. Brun, has been printed in the correspondence.—The evaporation of liquids and the great capillary theories, by M. G. Van der Mensbrugghe. Most liquids evaporate spontaneously in the air. The consequences follow: (1) The liquid layer whence particles are continually being detached to form vapour cannot have the same density as the liquid in the interior of the mass, otherwise there would be an abrupt passage from the liquid state to vapour; it must, therefore, be admitted that the density of the superficial layer decreases towards the exterior. All capillary theories, supposing liquids incompressible (Laplace), or of the same density throughout (Gauss), are therefore inadequate. (2) When the mass considered is very small (bubbles, liquid films), evaporation causes loss of a perceptible fraction of the total weight. Hence capillary theories regarding a liquid mass as having an invariable volume (Poisson) must be condemned. (3) The constant renewal of the free surface of the superficial layer proves, without possible doubt, that this layer is not in equilibrium. What confidence can then be placed in the theories of Laplace, Gauss, and Poisson, and the works of contemporary analysts Neumann, Mathieu, Van der Waals, Resal, and Poincaré, who formally suppose a liquid mass in equilibrium? After calling attention to the defects of former theories, the author quotes his own theory, derived from a consideration of molecular forces, as giving a sufficient explanation of these consequences.—On a new nitrogenous manure, calcium cyanate, by M. Camille Faure. It is asserted that calcium cyanate can be produced in large quantity in the electric furnace by heating lime and charcoal intensely in an atmosphere of nitrogen, and oxidising the product by air. The cyanate contains a greater proportion of assimilable nitrogen than nitrate, and can be used as a manure.—Syntheses by means of cyanacetic esters, by M. T. Klobb.—Constitution of acids produced in the oxidation of inactive campholenic acids, by M. A. Béhal. The acid $C_6H_{10}O_4$ is dissymmetric dimethylsuccinic acid; the acid $C_7H_{12}O_4$ is one of the two dimethylglutaric acids having the two methyls attached to the same carbon atom, probably $CO_2H.C(CH_3)_2.CH_2.CH_2.CO_2H$. The author claims priority for his work against that of Tiemann.—On the effects of the synodic and anomalistic revolutions of the moon on the distribution of pressure in spring, by M. A. Poincaré.—On a double night ascension (balloon) made on September 4, by MM. G. Hermite and Besançon. Two balloons made voyages from Paris in opposite directions, starting at the same time. The currents observed and used are described, together with details of the voyages.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—A Hand-book to the Birds of Great Britain: Dr. R. B. Sharpe, vol. 2 (Allen).—Climbing in the British Isles: W. P. H. Smith and H. C. Hart, II. Wales and Ireland (Longmans).—Practical Proofs of Chemical Laws: V. Cornish (Longmans).—An Introduction to the Study of Seaweeds: G. Murray (Macmillan).—Catalogue of the Library of the Royal Geographical Society: Dr. H. R. Mill (Murray).—Dynamics: Prof. P. G. Tait (Black).—Farm Foods, or the Rational Feeding of Farm Animals: Prof. E. v. Wolff, translated by H. H. Cousins (Gurney).—The Gold Mines of the Rand: F. H. Hatch and J. A. Chalmers (Macmillan).—The Fauna of British India, including Ceylon and Burma; Birds, Vol. 3: W. T. Blanford (Taylor and Francis).—Popular History of Animals for Young People: H. Scherren (Cassell).—Moral Pathology: Dr. A. E. Giles (Sonnenschein).—The Splash of a Drop: Prof. A. M. Worthington (S.P.C.K.).—Simple Methods for detecting Food Adulteration: J. A. Bower (S.P.C.K.).—Biology Notes, Vol. 1 (Chelmsford).—Einführung in das Studium der Bakteriologie: Dr. C. Günther, Vierte Auflage (Leipzig, Thieme).—Natural History of Selborne: Gilbert White, 2 Vols. illustrated (Macmillan).—The Scientific Foundations of Analytical Chemistry: Prof. W. Ostwald, translated by Dr. G. M'Gowan (Macmillan).—The Structure of Man: Dr. R. Wiedersheim, translated by H. and M. Bernard (Macmillan).—Weather and Disease: A. B. MacDowall (Graphotone Company).—Old Farm Fairies: H. C. McCook (Hodder and Stoughton).

Pamphlets.—Les Limites Actuelles de Notre Science: Marquis de Salisbury, translated by W. de Fonvielle (Paris, Gauthier-Villars).—Reaction!: K. Pearson (Reeves).—Guide to the Collections of Rocks and Fossils belonging to the Geological Survey of Ireland: A. McHenry and W. W. Watts (Dublin, Thom).—A Supplement to a Revised Account of the Experiments made with the Bashforth Chronograph: F. Bashforth (Cambridge University Press).—Ein Brauner Tschimpanse im Dresdner Zoologischen Garten: A. B. Meyer (Berlin, Friedlander).

Serials.—Journal of the Royal Agricultural Society of England, Vol. vi. Part 3 (Murray).—Natural History of Plants: Kerner and Oliver, Part 16 (Blackie).—Mind, October (Williams and Norgate).—Transactions of the Perthshire Society of Natural Science, Vol. 2, Part 2 (Perth).—Geological Magazine, October (Dulau).—Morphologisches Jahrbuch, 23 Band, 1 Heft (Leipzig, Engelmann).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1895, No. 2 (Moscou).—Geographical Magazine, October (Stanford).—Story of the Heavens: Sir R. S. Ball, Part 1 (Cassell).—Ethnologisches Notizblatt, Heft 2 (Berlin, Haack).—Bulletin de l'Académie Royale des Sciences de Belgique, 65^e Année, No. 8 (Bruxelles).—American Naturalist, October (Philadelphia).—Annals of Scottish Natural History, October (Edinburgh, Douglas).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, Vol. 9, No. 6 (Manchester).—Science Progress, October (Scientific Press).—Illustrated Archæologist, and Reliquary, October (Bemrose).—Travaux de la Société des Naturalistes de St. Pétersbourg, Vol. xxiii. (St. Pétersbourg).

CONTENTS.

THURSDAY, OCTOBER 17, 1895.

RECENT ORNITHOLOGY.

The Land Birds in and around St. Andrews. By George Bruce. Dundee: John Leng, 1895.

The Migration of British Birds, including their Post-Glacial Emigration, as Traced by the Application of a New Law of Dispersal. By Charles Dixon. (London: Chapman and Hall, 1895.)

Heligoland as an Ornithological Observatory, the Result of Fifty Years' Experience. By Heinrich Gatke. Translated by Rudolph Rosenstock, M.A. Oxon. (Edinburgh: David Douglas, 1895.)

A Hand-book to the Game Birds. By W. R. Ogilvie-Grant. Vol. i. Sand-grouse, Partridges, Pheasants. (London: Allen and Co., 1895.)

The Land-birds and Game-birds of New England, with Descriptions of the Birds, their Nests and Eggs, their Habits and Notes. By H. D. Minot. With illustrations. Second edition. Edited by William Brewster. (New York: Houghton and Co., 1895.)

Wild England of To-day, and the Wild Life in it. By C. J. Cornish. (London: Seeley and Co., 1895.)

The Pheasant: Natural History. By the Rev. H. A. Macpherson. *Shooting.* By A. J. Stuart-Wortley. *Cooking.* By Alexander Innes Shand. (*The Fur and Feather Series.* London: Longmans, Green, and Co., 1895.)

NO section of vertebrate zoology has in this country attracted more amateur disciples than ornithology; and the literature of perhaps no other group has been burdened by so many useless contributions by writers who, possessing not only little literary qualification for the task, but a very superficial knowledge of the subject, rush into print, assuming that, because they are able to see, they are capable of observing, which are two very different things. Among the number of such contributions must be included a volume of 563 closely-printed octavo pages on "The Land Birds about St. Andrews," by Mr. George Bruce. On the book opening of its own accord at p. 44, the heading of "The Griffon Vulture" caught the eye and surprised us not a little; for the addition of this majestic bird to the avifauna of Fifeshire was quite new to us. On consulting the title-page, however, we discovered that the work was of wider scope than indicated on the cover, and included "a condensed history of the British land birds, with extracts from the poets and observations and anecdotes on natural history." "The single occurrence of a solitary specimen" in Ireland, recorded by Yarrell, is apparently sufficient excuse for this page of padding. A carefully-written account of the birds of Fifeshire would have been welcomed to our lists of local faunas; but with so many excellent histories of British birds in existence (such as that by Mr. Howard Saunders, to mention only one), there was hardly a call, one would have thought, for another, except it were commended by some special feature or novel method of treatment. The special features of this book appear to consist in the superabundant extracts from the poets—more or less, generally less, *à propos*—cuttings from the local newspapers, and quotations from many other sources equally authoritative. Although the "history," such as it is, is very condensed, and not always to be taken on trust, and the anecdotes poor and pointless, there are, nevertheless, in the book not a few observations which we are confident will prove new to most ornithologists. Of these we cull a few, and refer our readers, who desire to dig deeper, to the book itself for others.

"The Isle of Man has proved one of the best stations in Scotland for migration observations."

"The species means every individual bird in creation: for instance, a lark is one species. . . . A genus is a group of these birds so closely resembling each other as hardly to be mistaken, as the raven, the carrion crow. . . . These combined form the genus called *Corvus*, which means in British [*sic*] crow. The plural of *Corvus* is *Corvine*, as genera is the plural of genus."

"Among those naturalists who have recently [!] done so much for the advancement of this branch of science Temnick [!] and Montague [!] deserve to be ranked amongst the first."

Mr. Bruce records the occurrence of the nightingale as far north in Scotland as Paisley and Uddingston, upon the unquestioned authority of one James Anderson in a letter to a local newspaper, apparently. The *Struthionidæ*, we find here, are represented in the British Isles by the genus *Otis*, and that the author of the species *Ulula stridula*, *Salicaria arundinacea*, and *S. phragmites* is Mr. George Bruce, of St. Andrews! According to the title-page he is also the author of "Destiny and other Poems," of which we must confess our ignorance. We trust, however, that the doom of "The Land Birds of St. Andrews" may have no prejudicial effect on his earlier volume.

"The Migration of British Birds" is the new work by Mr. Charles Dixon, which was heralded a short time ago by an article in the *Fortnightly Review* from his own pen. This author's previous volume on a similar subject was exhaustively discussed in NATURE for December 1892. On that occasion the deliberate conclusion was expressed "that Mr. Dixon, author of so many works as he may be, is no authority on the subject of migration, which he has left exactly as he found it." The same verdict must be passed on the present volume, and we might have dismissed it without further discussion but for two reasons. The first is the fact that in one or two important daily journals, whose scientific reviews in general command our entire respect, Mr. Dixon has been rather prematurely elevated to the rank of a Moses in ornithology, and the other is that he declares that his present views are now opposed to those he has expressed in previous works. Whether the abandonment by Mr. Dixon of his former views is due to the criticism to which they were subjected in NATURE, we have not the satisfaction of being informed.

This "new Law" here promulgated to the world (not yet accepted by it) is the "undiscovered principle" which is to solve all the difficulties of geographical distribution, and the dispersal of life, and clear up "the greatest mystery which the whole animal kingdom presents," to quote the words of one of our foremost ornithologists—"a mystery which attracted the earliest writers, and can in its chief point be no more explained by the

modern man of science than by the simple-minded savage or the poet or prophet of antiquity." When writing these pregnant words it was not given to this erudite biologist to foresee the revelation of "this Our new law" of dispersal to Charles Dixon, of which the volume under notice is the first proclamation. This great new "law forbids retreat." To Mr. Dixon it has been revealed that the effect of the slow oncoming of a glacial epoch in either hemisphere was not to cause bird-life to retreat in front of the increasing cold, but really to exterminate all those birds having a range of distribution entirely within the refrigerated areas, and to contract the range of such as were migratory. Those birds alone survived, therefore, whose former range extended beyond the glaciated areas (the unglaciated portions of their range the author calls "refuge areas"); while all those birds which had no refuge area were totally exterminated, and have since been lost to science. The "law," moreover, forbids species in the northern hemisphere ever to increase their range in a southerly direction, and species in the southern hemisphere ever to increase theirs in a northerly direction; and only those northern birds or those southern birds whose refuge areas extended on both sides of the equator are permitted by the "law" to extend their breeding range to regions towards the opposite pole, which presented the most favourable conditions for reproduction. Now "this Our law," we are told, applies not only to birds, but to all life, and is a universal explanation never thought of by any other "biologist of note," of the migration and geographical distribution of species. To show that this is so, Mr. Dixon applies his law to the distribution of "arctic" types in the flora of the southern hemisphere. Sir Joseph Hooker long ago explained the presence of the "Scandinavian" element in that flora, by indicating its migration routes along the meridional highlands of the great continental land masses. Hooker, Huxley and Wallace, and doubtless all those other ornithologists and geologists—among whom are Sharpe and Geikie—who have, according to Mr. Dixon, gone "beyond their last," have been quite misguided by reason of their ignorance of this law. Our latest authority, however, declares with all the emphasis of certainty that "there can have been no emigration of plants from north to south"; "it could never have taken place"; Our "law forbids it." The true solution of the question by Mr. Dixon is, that all the "arctic" plants in the southern as well as in the northern hemisphere, spread from an equatorial centre. Let us take, for example, an "arctic" species common, say, to high northern latitudes, and to New Zealand, and the Southern Andes or South Africa. This species must, in the first instance, have arisen in some part of the equatorial regions from a tropical form, by ascending to the cool arctic zones of one of the mountains—suppose in South America. It must then have followed one of two routes of dispersal. After multiplying it must either have spread right round the equator (the absence of continuous land notwithstanding), crossing again and again the torrid interspaces separating it from other equatorial altitudes, which served it as stepping-stones, till it attained those longitudes whence it could extend its range, as best it might, to its [illegible] northern and southern habitats—a migration-route too remarkable to be easily credited. The alternative route, so far as regards the southern hemisphere, at all events, would be for the species to spread southwards on one of the continents (say South America), till reaching a then-existing Antarctic land, over which it must have gradually dispersed, and in order to reach South Africa or New Zealand, it would have to travel northwards in the very face of Mr. Dixon's inexorable law, which it would thus entirely upset, and with it all the conclusions in the present treatise. How would Mr. Dixon explain, for instance, the distribution of *Petraea arborea* in South America, in West Java, and East Timor? Another method of dispersal may perhaps be predicated as possible by some, namely, the *independent* origin from equatorial ancestors of identical arctic species in high northern and southern latitudes; but any such occurrence is too improbable to be seriously entertained.

This law, which seems to us to fail most lamentably to explain the dispersal of plants, fails not less in regard to the migration of birds. It surely requires no pointing out that during every winter we have numberless boreal species (birds, whales, seals) visiting our shores in retreat south into more genial climes; the sheep feeding on any high hill, and overtaken at the beginning of winter by storms, hasten for food and shelter to lower levels, where they would continue to remain if there came no moderation in the weather of the uplands; and our resident redbreasts for the same reason retreat from the woods before the first snows to the neighbourhood of our homes, and if the winter be specially severe they retreat still further in search of more genial conditions—they do *not* dare the storm and die on the snow. What takes place in miniature during the winter would simply be enacted, there is little doubt, on an extensive scale during a glacial epoch. The migration, to be seen to-day in Western Europe, we are told by Mr. Dixon, was undoubtedly initiated with the passing away of the third glacial period, is undertaken expressly for purposes of reproduction, and is "the constant endeavour of what we must now regard as but the relics of such exiled life to regain and repeople the area that it once occupied during pre-glacial time." Had the migration of pre-glacial times a different cause or motive than that of to-day? Why is migration necessary for the purposes of breeding? Is there not space enough, food enough, and a better climate in the regions where the migrants winter, and to which the parents, indeed, return reinforced by their young, to be dependent on the supplies of that area? How, we may also ask, can the birds which occupied the southern and non-glaciated portion of their range be inspired by "a constant endeavour to regain" an area their parents had never occupied, and had never even known; for those of their species which had occupied and known the northern part of the range, we are assured rather than retreat a step, chose to die under Dixon's "law." The new Commandment which forbids a southern extension of breeding area, "renders," according to Mr. Dixon, "a flight south in spring impossible"; and "all species do not breed [more grammatically, no species breeds] anywhere south of their [its] point of entrance." Yet the penguins defy this law, and though southern hemisphere birds, they migrate equator-wards to breed. In the spring of this year the present writer witnessed, in

the middle of the Irish Sea, a flock of migratory birds crossing (the weather having been specially fine for some time) to England, from Ireland apparently, on a *south-east* course. Before crediting this lob-sided partially-radiating dispersal, we must have more convincing proofs that birds and plants are so peculiarly constituted that an invisible parallel of latitude athwart a congenial region, is, in a particular compass bearing, as impassable to them as an ocean or a sahara. We cannot affect to believe that Mr. Dixon's is a more satisfactory explanation of the mysterious season-flight of birds, than the cause (among others) long ago assigned, that the migrant species come north in spring to breed, impelled by a hereditary impulse at that season (and probably guided by a direction-sense with which they are specially endowed), to return to their old nurseries from the regions whence their ancestors were compelled by geologic and climatic causes to retreat, and in which they were so long acclimatised as to be now unable to withstand the cold winter, with its meagre fare, of their ancestral *patria*, which consequently they forsake again in the autumn.

We cannot afford space to touch on many other points in Mr. Dixon's book in which we believe he has gone astray. We feel no nearer a solution of the mystery of migration than before its publication. Writers on this subject "should thoroughly understand not only the rudiments of the higher philosophy [whatever that may mean] of the geographical distribution of life before they attempt to theorise upon it, or endeavour to demonstrate it." We offer Mr. Dixon his own advice, which we have copied from a paragraph in which a charge of *ultra crepidam* is ill-naturedly levelled at some of the foremost workers in the science with which he is dealing, and to which their lifetime has been unremittingly devoted—a charge which surely comes ill from one who is purely an amateur, and a young man compared with the veterans at whom he sneers.

Mr. Dixon's style is cumbrous and not always easy to comprehend, while his English is often very ungrammatical. It is only justice to admit that the book, with the *theories* of which we so entirely disagree, contains much interesting information collated and condensed from many sources.

It is refreshing to turn from these airy speculations to the stable ground of pure and unadulterated fact with which the pages of "Heligoland as an Ornithological Observatory" are so lavishly filled. This is the English translation by Mr. Rosenstock of Herr Gätke's celebrated volume published in German in 1890. Ornithological students in England owe their heartiest thanks to the translator, as well as to Mr. Harvie Brown, to the publishers, and to all who have given a forwarding hand to the task of presenting them with this great and important work in their own language. The labours of its venerable and distinguished author are too well known in this country to require us to do more than recommend his book—corrected by the author down to May last—in its new garb. Binding, printing, paper, and illustrations are all that can be desired. In turning over its pages we recognise anew the trustworthy observer, and are reminded of the story told of an old woman in a northern county of Scotland, who, on being taken to task by her minister for invariably paying the closest attention to any stranger who occupied the pulpit, and of as persistently sleeping in unbroken repose throughout his own sermons, replied, "Hoot minister! wha's to ken fat kin' o' doctrine they youngsters may be gi'in'; we a' ken fine that we can lippen to yoursel'." Herr Gatke's book can be perfectly "lippen"-ed to. It is divided into three parts, the first of which—on the migration of birds—is perhaps the most important and interesting. This subject is discussed in nine chapters, dealing with the course of migration in Heligoland; the direction, altitude and velocity of the migration flight; the meteorological conditions influencing it; the order of migration; exceptional phenomena; what guides the birds, and the cause of the movement. In regard to the last, we quote the conviction of this patient observer and recorder after fifty years' experience, "that what at present has been ascertained in reference to the migration of birds furnishes us with no clue, by the aid of which we are enabled to penetrate the depths of this wondrous mystery." The second part deals with changes which he has observed to occur in the colour of the plumage of birds without moulting. This subject has also been studied by Mr. Ogilvie-Grant, of the British Museum, who has not only corroborated the truth of Herr Gatke's observations, but thrown much new light on the subject. The final section of the book gives an account of the birds observed in Heligoland, which number 398. The volume is illustrated by a number of charming vignettes, and by two excellent portraits of Herr Gätke.

The latest addition to the naturalist's library, edited by Dr. R. B. Sharpe, and published by Messrs. Allen and Co., of Waterloo Place, is a "Hand-book to the Game-birds," by Mr. W. R. Ogilvie-Grant, who is well known to be an authority on this group. This is the first of two volumes, and contains an account of the sand-grouse, partridges and pheasants. The second volume (which will be issued shortly) will deal with the American partridges, the megapodes, curassows and hemipodes. The hand-book is founded on the author's British Museum catalogue of the group (vol. xxii.), and is one of the best yet issued of the valuable series to which it belongs. So far as published, the volumes of Allen's Naturalist's Library are each of them concise monographs of the groups they relate to, well illustrated and published at a very low price. The aim of the author has been to treat the subject in such a way that it may not only be useful as a scientific work of reference, but also as a handy book for sportsmen and field naturalists. With its aid they should be able not only to identify the birds they shoot with as little trouble as possible, but also to find out what is known concerning the life-history of each species. The work will be specially valuable to the museum curator; indeed, it is the only handy and up-to-date monograph of the families it describes. This volume contains twenty-one full-page coloured illustrations, some of which are republished from Jardine's Naturalist's Library; the majority, however, have been specially drawn for it by Mr. Keulemans. It is to be regretted that Messrs. Allen do not see it to their advantage to dispense with the

antiquated figures of the former edition, for when they are placed beside Mr. Keulemans' beautiful plates, the contrast is too striking not to call forth unfavourable remark. The birds from the hand of that artist seem transported fresh from the heaths and the hills; the others look like worn museum specimens. A special feature in Mr. Ogilvie-Grant's hand-book, is the full account given of the various phases of the moult in the grouse, partridge and blackcock, and of the curious change of plumage that takes place in these birds without moulting. We are indeed indebted, as observed above, to him more, we believe, than to any other, for the elucidation of these interesting, and to a great extent inexplicable, variations. The account he gives of the plumage-changes in the blackcock (*Lyrurus tetrix*) have never till now been so fully described. We understand that the description of both male and female of every species has been carefully made from the actual skins, and checked with the specimens, in proof. This is sufficient to establish the accuracy and value of Mr. Ogilvie-Grant's work. The only doubtful statement we have detected is on p. 189, where the author has stated, following the authority of Sir Walter Buller, that the New Zealand quail, now extinct in that colony, still exists on the Kermadec Islands. We are inclined to believe that its discovery on the latter island was a mistake, and that this interesting bird is now absolutely exterminated.

"The Land-birds and Game-birds of New England" is a new edition of this local fauna published some nineteen years ago. Its author is the late Mr. Henry D. Minot, who, as we learn from a biographical notice which prefaces the book, had from early childhood showed a great fondness for nature, and who, devoting himself to the study of birds, had completed the manuscript of this volume of over 400 closely-printed pages in his seventeenth year. This new edition issues from the press under the care of the distinguished ornithologist, Mr. W. Brewster, who says that the book was well received on its appearance, sold rapidly, and soon became out of print. Mr. Minot adopted the profession of a railroad engineer, and for fifteen years lived in the hope of adding to, and correcting his published observations. His duties, however, prevented him from accomplishing this task, and his career terminated in 1890 by his being killed in a railway collision. Written by a youth of seventeen, as the editor observes, "with, as I am assured, almost no outside help of either a literary or scientific kind, it is a remarkable and interesting book, for most of the [bird] biographies relate to his own experiences or impressions." The book is certainly worth republishing. The original text has been left almost untouched, and a few notes found in Mr. Minot's annotated copy are inserted at the foot of the pages. As could not but happen in one so young, there are not a few errors, both of fact and deduction; but the "editorial touches" of Mr. Brewster have safeguarded the reader against being misled, and given to the book much of the value it now possesses. Mr. Minot was a keen observer, and the [illegible] apart from what it possesses as a local fauna, and from Mr. Brewster's annotations, lies in his [illegible] of the habits of the New England birds. [illegible] much accurate and [illegible], recorded in a pleasant and easy style. In speaking of the quail (*Colinus virginianus*), he racily describes the unsuccessful pursuit of a covey by a young "gunner," and concludes: "Now the lad returns home, and explains his ill-luck by an extraordinary theory, read of in books, and verified by his own experience, that our Quail have a wonderful power of retaining their scent. The only sound argument to prove this statement is that our game-birds, when very young, by a thoughtful provision of nature, emit little or no scent." In later years the author added this note. ". . . When game-birds drop suddenly to the ground and remain motionless, the dog does not perceive them. Quail most frequently alight in this way, but as soon as they begin to move, the effluvium escapes and is disseminated." Mr. Brewster adds his "editorial touch" to the following effect: "The question cannot be settled in this summary manner, for the writer overlooks the important fact that the habit of retaining scent is not common to all the quail of any one locality or region. On the contrary, it is peculiar to certain individual or bevies who invariably practise it when pursued by sportsmen. Yet these individuals do not drop more suddenly, nor remain more motionless, than the less fortunate birds which the dogs easily find and point." Thus author and editor.

The illustrations consist of woodcuts in outline, but though "drawn from nature," are of no practical use, and might have been omitted with advantage. The book is well printed, and has, as frontispiece, a portrait "prepared and engraved by Mr. A. F. Jaccaci as a personal tribute" to the talented but unfortunate author.

In "Wild England of To-Day," by Mr. Cornish, we have a collection of essays republished from different journals, but chiefly from the *Spectator*, describing the life in various "wild," secluded or thinly populated districts of the country "ranging from the southern cliffs to the Yorkshire fen." Although we find such subjects discussed as "salmon-netting at Christchurch," "trout-breeding," and "the deer in Richmond Park," the majority of the papers are devoted to bird-subjects, and thus come lawfully within the scope of this article. The whole of the sketches, while quite popularly written, are scientifically accurate, without being or pretending to be permanent contributions to science. Charmingly indited, they remind one of the style and flavour of the late Richard Jefferies' psalms in praise of nature. The book is adorned by a number of full-page illustrations of exceptional excellence, from photographs and from drawings specially made for it, of which the "Peewit's Nest," by J. W. Oakes, A.R.A., deserves special mention as an exquisite little picture.

The latest addition to the attractively bound "Fur and Feather" series, whose volumes form such pleasant journeying companions, is "The Pheasant." The Rev. H. A. Macpherson treats, as he does in several of its predecessors, of the natural history of the bird. He discusses concisely its acclimatisation from the earliest times, its geographical distribution and its nesting habits, while under the heading of "Freaks and Oddities" he describes its plumage changes and its cross breeding. This section concludes with two chatty chapters on "Old World Fowling" and "Poaching in the Nineteenth Century."

Mr. Stuart-Wortley discourses with authority on how to shoot—slaughter?—this tame "Byrd of singular beauty," when driven in bevies slowly and with not a little persuasion just sufficiently far away to "home," on being flushed, at a proper altitude over the guns, which are thickly stationed in hiding to rain a murderous hail on them. The shooting of the wild-bred bird is, however, nobler sport. "Nothing strikes one more in Norfolk," says Mr. Stuart-Wortley, "especially in the heath district, than the prevalence of pheasants everywhere . . . and it adds greatly to the charm of a partridge drive when it is varied by a few rocketing pheasants out of the belt you are standing by, or when they rise high off the heath and come over with the partridges, and quite as fast. . . . The late October days in Norfolk and Suffolk, especially where there is heath, are among the most fascinating to be got in England."

Mr. Innes-Shand plays on our salivary glands by extolling the excellence of the bird "when she is in the dish," roast and with bread-sauce, and in many a fascinating style besides that "sublimest form of art . . . the *faisan à la Sainte-Alliance*." Altogether "The Pheasant" is, as remarked above, a delightful *compagnon de voyage*, and will be found in many a portmanteau in the late October days. The ten well-produced full-page plates add much to the attractiveness of the volume.

OUR BOOK SHELF.

The Elements of Botany. By Francis Darwin, M.A., M.B., F.R.S., Fellow of Christ's College, Cambridge, and Reader of Botany in the University. (Cambridge: University Press, 1895.)

IN this little book the elements of botany are presented in a more refreshing form than is too often the case. The author has chosen to emphasise certain principles and phenomena of morphological or of physiological importance, rather than to crowd his pages with vast numbers of facts. Various plants are requisitioned to serve as illustrations of the different subjects under treatment; and thus the student will certainly acquire a clearer and more general conception of what, for instance, a flowering plant is, and how it lives, than would have been possible had only one example been selected as a type, even though this had been far more exhaustively dealt with.

There are some matters, however, in which it may be doubted whether the method of treatment adopted will commend itself equally to most botanists. Thus, although Mr. Darwin says that he advisedly puts the doctrine of alternation of generations into the background, many will doubtless regret his decision. It is true that without the introduction of a few more intermediate types, the question would possess, as the author says, but little interest for the elementary student. But in view of the great importance, both of the facts and of the comparisons based upon them, one cannot help wishing that the general bearings of the question could have been indicated somewhat more fully.

A second matter is the employment of the term *bark* in the popular, as opposed to its more technical, sense. Botanists have come to attach a special and restricted meaning to the term; and though it is no doubt highly improper to pirate English words, still this is done in every technical department, and thus, in spite of its admitted inconvenience to the beginner, we think the balance of advantage is in favour of the retention of the appropriated word in its restricted significance.

But these are cases in which there is room for difference of opinion; there will be none at all on the question as to the merits of Mr. Darwin's book considered as a whole. It is an admirable work which both teacher and student will cordially and deservedly welcome.

The Book of British Hawk-Moths, a Popular and Practical Handbook for Lepidopterists. By W. J. Lucas. With illustrations from Nature by the Author. (London: L. Upcott Gill, 1895.)

THERE is a great flood of books on the larger and more showy British *Lepidoptera* issuing from the press at the present time; but so long as the information which they contain is fairly accurate, and they place on record a portion of the floating information derived from periodicals or personal observation, we do not see that the fact is to be regretted. At least it is a sign that an intelligent interest in entomology is now taken by a large number of persons who are not entomologists or collectors themselves; for we do not believe that there is a sufficiently large number of entomologists to buy up the large editions of popular books which are now offered to them: they must appeal to a considerable number of outsiders as well.

The book before us is restricted to a very small group of British moths, the *Sphingidæ* proper, numbering only seventeen species, several of which are possibly only casual visitors rather than permanent residents. Consequently, the author has been able to treat of the subject in considerable detail, though a good deal of the introductory part of the book deals with the collecting and preserving of *Lepidoptera*, rather from a general point of view, than as specially applicable to *Sphingidæ*. The illustrations consist of folding plain plates, representing the larva, pupa, and imago of each species, the earlier stages, when not observed by the author himself, being usually copied from Buckler's work on larvæ. There are also occasional woodcuts in the text. The letterpress is pleasantly, though sometimes hastily, written, and is fairly complete and up to date; and most of the illustrations are good. On the last plate, the names of the two bee hawk-moths appear to have been reversed, probably by a printer's error. The information given is, we believe, accurate; but every entomologist will be able to supplement it according to his own experience. Thus, it might have been stated that *Smerinthus tiliæ* (the lime hawk-moth) is one of the commonest of the *Sphingidæ* in the suburbs of London. *Sphinx pinastri* (the pine hawk-moth) is mentioned as sometimes found at rest on the trunks of pine trees. So it is; but it will also rest on other trees, and on the continent it is often found resting on the trunks of the poplars which often fringe the roads in the neighbourhood of pine forests.

W. F. K.

Biology Notes. Vol. i. Edited by David Houston, F.L.S. Pp. 290. (Chelmsford: Technical Laboratories, 1895.)

THIS volume is a collection of bulletins published monthly by the Technical Instruction Committee of the Essex County Council, as an aid to the teaching of biology. It contains information bearing upon the applications of biology to the industrial pursuits of the county, and notes of interest to biological students. Among the subjects of short articles are ergot and its physiological effects, bracken poisoning of cattle, biological aspects of dairying, injurious insects, diseases of cultivated plants, zoology on the Essex coast, and spraying experiments; and there are also included in the volume several detailed syllabuses of courses of practical instruction in vegetable and animal biology. The "Notes" are well illustrated, and must be of great assistance to the students in the classes controlled by the Essex County Council. Other County Councils would do well to issue monthly bulletins of the kind collected in this volume.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The University of London.

I HAVE been away from home, and have only now seen Mr. Thiselton-Dyer's letter of August 23.

My previous letters were, I thought, quite clear: but as he asks me to do so, I write to explain that my two statements which he quotes, viz.: (1) "I am not asking that any privilege which they do not at present possess should be conferred upon my constituents, but only supporting what is now their legal right. . . . This right I know they highly value"; and (2) "It is the law at present," had reference to the present right of veto possessed by Convocation.

As regards the vote being taken as at a senatorial election, so far from stating that this was at present the law, the very terms of my letter implied that it was a change.

Whether it would be "radical" or "revolutionary" is, of course, a matter of opinion, but I certainly did not make the suggestion with the object attributed to me; nor do I share my friend's opinion that the graduates would take a course which, to quote his words, "would destroy the prospects of Academic study in London." JOHN LUBBOCK.

High Elms, October 8.

Sir Robert Ball, and "The Cause of an Ice Age."

MR. JAMES GEIKIE has recently brought out another edition of his "Ice Age," a well-known and influential work. In this book he quotes freely from Sir Robert Ball's "The Cause of an Ice Age," which appeared in 1891, and which was remarkable as the first work written by a professed astronomer in which an astronomical explanation of an Ice age was put forward and defended. As the influence of these books upon popular opinion, and even perhaps upon some scientific men, may prove very misleading and mischievous, perhaps you will allow me a little space in which to discuss Sir Robert Ball's work.

The book was preceded by much advertisement, in which we were told not only that it contained an entirely new view of the subject, but that an astronomical basis of the Ice theory was at last securely established.

When the book itself was published, it appeared also that the new matter in it consisted of "a law, hitherto unsuspected, regulating the distribution of heat between summer and winter in either hemisphere." Thus on page 113 the author says: "*I discovered* the law of distribution of sun heat on a hemisphere between the two seasons into which the year is divided by the equinoxes." Again he says: "*I enumerated and proved* that law of the distribution of sun heat between the two seasons, which I have already referred to as the cardinal features of this little book" (*op. cit.* 113.) Again, in the appendix he says: "The following is the calculation often referred to in this book, and in which *for the first time*, so far as I know, the astronomical facts relating to Ice ages have been correctly given." Lastly, he says: "If it should prove that the facts which these numbers imply have not been given by any previous writer, *then their announcement is the novelty in this book, the one central feature by which it is to be judged.*" Sir Robert Ball afterwards speaks slightingly of Herschel and Croll for having ignored this law.

It was very soon pointed out in a review of his book that this particular law which Sir R. Ball claimed to have discovered had been already enunciated and published by Wiener.

The fact might easily have escaped any one else but a writer who was himself a mathematician writing expressly on this very point, which was the justification of his book. Let that pass, however.

It seems to some of us that when the Astronomer Royal for Ireland had had this pointed out to him, he ought at once to have written to the scientific papers correcting his mistake, and doing justice to the real discoverer of the law, and that the book itself should not have been issued again without this correction appearing in it, for the publication of the supposed discovery was the *raison d'être* of the book.

Nothing of the kind has happened, however, and the only reference that I know of the mistake by its author is in an appendix to a book published in 1893, entitled "The Story of the Sun," in which no reference whatever is made to the claims set up in 1891, but the law in question is simply referred to as "Wiener's law," as if everybody in the world must know that Wiener and not Ball had discovered it. Meanwhile, "The Cause of an Ice Age" is not cancelled or withdrawn or corrected, but is being continually issued with all its exploded claims.

What I have just written refers merely to a claim to have discovered a law which was discovered by some one else, and to the amenities which generally regulate our conduct when we are shown in such a case to have done another man an injustice. But this is a very small matter. A much more important matter remains.

The law which Sir R. Ball claimed to have discovered is an indisputable one. No one doubts it, or could doubt it. What most people who have examined the problem say, however, is not that the law is not a perfectly good one, but that it has nothing whatever to do with the question of an Ice age. The law in question is briefly, that the quantity of heat received by either hemisphere of the earth in summer is to that it receives in winter in the ratio of 63 to 37. This is an invariable ratio, true at all times, and true under all conditions of eccentricity of the orbit. It never varies. It was the same millions of years ago, so far as we know, as it is now, and so it will remain. It is therefore a *constant* factor in the problem, and being a *constant* factor it cannot be the cause of *variability* of climate. If, as we are told in the book over and over again, this particular proportion is the cause of an Ice age, we must be living in an Ice age now, and we must always have been in an Ice age. Therefore the law in question was not only not new, but it is an absolutely irrelevant law so far as the problem at issue is concerned. Whether the particular numerical ratio was present to the minds of Herschel and of Croll when they wrote on the problem, is quite immaterial; and being so, the whole *raison d'être* of Sir R. Ball's book is gone, and so far as we know there is not a single material factor of the problem discussed by Sir Robert Ball which was not present to Croll when he wrote "Climate and Time" and his other works.

Lastly, Sir Robert Ball, following in the wake of Croll, has subjected the various facts and conditions, both astronomical and meteorological, which in his view induced an Ice age to analysis, and has reached certain conclusions which he has emphasised in his later work, "The Story of the Sun." This analysis has been criticised and examined by more than one person, but with especial closeness of reasoning and conclusiveness by one of Sir R. Ball's own pupils, a distinguished Fellow of Trinity College, Dublin, Mr. Culverwell. His criticisms have appeared in NATURE and in the *Geological Magazine*.

In the view of those who have read these criticisms, they are simply crushing. No more complete and acute dissection and destruction of a scientific argument has appeared for many years.

This criticism was originally read at the British Association, in the presence of Sir R. Ball himself, who made no attempt whatever to answer it, but (mistaking his audience) merely gave vent to some jocular remarks. The Lowndean Professor at Cambridge cannot turn the flank of serious criticism by ill-timed jokes. Since then he has not, so far as I know, answered his critics in any way, or tried to justify his riddled arguments, and the books in which they are contained are being sold, and their conclusions are being quoted as if they were sound instead of being absolutely untenable.

If Sir R. Ball were an ordinary person, a free lance in literature and science, he might say anything and publish anything with impunity, and might refuse to answer criticism from any quarter; but he was once Astronomer Royal for Ireland. He now fills the chair at Cambridge once occupied by Adams. He cannot write without in some way committing that chair and that University by his opinions; and his principal critic is not an obscure scribbler, but a mathematician as accomplished as himself. Is it right or decent that, under these circumstances, he should continue to publish, with his name on the title-pages, works such as those I have described? Ought he not either to at once confess his mistakes, to answer his critics; or if he cannot do this, to withdraw books which have done some harm to thoughtless people, which have brought no credit to the chair he fills, nor to the University of which he is a Professor; and which have given rise to a good deal of angry comment among those who do not understand a man of science, of real distinction, remaining, for a day longer than he can help, the foster-father of what has been shown to be wrong either in fact or in argument?

I do not think Sir John Lubbock can know the facts of the case, or he would not permit his name to appear as the godparent of a book thus flyblown; nor should its publishers continue to issue it, and this not because the book contains mistakes—all books do that—but because its mistakes have been pointed out, and because its author is a great deal more than Sir Robert Ball, and cannot therefore escape the penalty of such a position.

The Athenæum Club, October 4. HENRY H. HOWORTH.

MacCullagh's Theory of Double Refraction.

An attempt has recently been made by Mr. Larmor to resuscitate MacCullagh's dynamical theory of double refraction (Brit. Assoc. Rep., 1893; *Phil. Trans.*, 1894, A, part ii.), but on examination this theory appears to me to infringe one of the fundamental principles of dynamics, viz. the principle of angular momentum.

Whatever the constitution of the medium may be, the forces which act upon any element consist of two distinct classes: (1) forces due to the action of contiguous parts of the medium; (2) forces arising from causes external to the element. The forces comprised in the first class are usually termed stresses; they act upon the surface of the element, and are completely specified by the nine quantities X_x, X_y, &c. The forces comprised in the second class act upon each element of mass, and arise from attraction or repulsion due to external causes or to the action of the medium upon itself. These forces, from whatever cause they may arise, are capable of being compounded into a single force along a line through the centre of inertia of the element, and a couple about some axis through this point. In ordinary gravitating matter the couple vanishes.

The equations of motion of the element in terms of the stresses and the *force constituent* of external action are the analytical expressions for the principle of linear momentum; but this principle is not sufficient to determine the motion of the medium—it is further necessary to satisfy the principle of angular momentum, and any theory which violates the latter principle is dynamically unsound. Now the principle of angular momentum requires that three relations of the form $X_y = Y_x$ should exist between the six shearing stresses, thereby reducing their number from six to three, except in the following two special cases. The first case occurs when the medium, previously to being disturbed by the passage of a wave of light, is *not at rest*, but possesses an independent angular momentum; that is to say, the medium is what has been termed a *gyrostatic* one. The second case occurs when the resultant of the external forces which act upon the element consists of a *couple* as well as a force. In the first case the kinetic energy of the disturbed motion of an element will not be proportional to the square of its velocity of translation, but will contain a term depending on the gyrostatic momentum: whilst in the second case the potential energy must necessarily contain a term due to external action.

Mr. Larmor assumes that the kinetic energy of an element is proportional to the square of its velocity of translation, so that the medium he considers is not a gyrostatic one; whilst the potential energy is supposed to be a quadratic function of the rotations, and he obtains his equations of motion by means of the principle of least action. Now, as we have pointed out, the potential energy of an element *may* consist of two distinct parts, viz. one due to deformation, and the other due to the action of external causes; and it is quite legitimate to assume *by way of trial* that the former part contains rotational terms. But it is well known that a quadratic expression which contains rotational terms will not satisfy the conjugate relation between the six shearing stresses, and consequently the principle of angular momentum will be violated, unless every element of the medium is under the influence of some system of forces, of the kind belonging to the second class, the couple constituent of whose resultant *does not vanish*. The potential energy ought therefore to be of the form $W + V$, where W is the portion due to deformation, whilst V is the portion due to external causes which supplies the couple which is necessary in order to prevent the principle of angular momentum being violated; and unless Mr. Larmor is able to surmount this difficulty, I am at a loss to understand how his paper is an improvement upon theories which are at any rate *dynamically sound*, whatever other imperfections they may possess. The question is one which cannot be disposed of by pages of vague and obscure generalities, but requires a detailed and careful mathematical investigation for its elucidation.

A. B. BASSET.

Holyport, Berks, October 3.

The Southern Carboniferous Flora.

So far as I am aware, Dr. Kurtz's paper on the newly discovered Carboniferous Flora in Argentina had not been noticed in print in this country until the appearance of the number of NATURE for September 26, which contained a note (p. 523) giving a brief abstract from the translation published in the Records of the Geological Survey of India. The circumstance that the original paper, which appeared nearly a year ago, was in Spanish, may have caused its being overlooked.

The subject of the ancient Southern floras is naturally unfamiliar to most European geologists, and I hope I may be allowed to point out why the present discovery is important. It completes a mass of evidence gradually accumulated. It is, of course, well known that several successive floras of Upper Palæozoic and Lower and Middle Mesozoic Age have been found associated with beds mainly of freshwater origin, some of which combine valuable coal seams, in India, Australia, and South Africa. The most ancient of these beds in Australia and South Africa contain certain plants, amongst them a *Lepidodendron*, allied to the ordinary Carboniferous flora of Europe and North America. From the upper beds in all the three regions named, Ferns, Cycads, and a few other plants have been obtained that are related to the Rhætic and Jurassic types found in European rocks. Between the upper and lower plant-bearing strata in South Africa and Australia, and beneath the upper series in India, are found beds, with coal seams in places, containing by far the most remarkable flora of the whole, the Glossopteris-flora, as it has been called. The particular interest attaching to this flora is mainly due to two circumstances. (1) It is clearly Upper Palæozoic, for in Australia the coal measures containing it are interstratified with marine beds abounding in carboniferous fossils, and yet it differs radically from any known European or North American flora of that age. (2) The basal beds, in India, Australia, and South Africa, are boulder beds, resembling the Pleistocene glacial boulder clay more than they do any other formation.

Now in Argentina the occurrence of the Southern Jurassic or Rhætic flora has been known for some years, and Prof. Derby has called attention to the presence in Southern Brazil of a great boulder bed, that very probably corresponds in character and geological position to the Talchir beds of India and the Dwyka beds of South Africa. More recently traces of the ancient Lepidodendron flora have been discovered in Argentina, and some additions to that flora are described in Dr. Kurtz's paper. But the important announcement in this paper is the discovery in Argentina of three Indian lower Gondwána plants, *Neuropteridium validum*, *Gangamopteris cyclopteroides*, and *Noeggerathiopsis hislopi*, all three associated in India with the Karharbári coal-seams near the base of the Lower Gondwána. Two of the species are also found or represented by closely allied forms in Australia and South Africa. In Argentina, as in India, Australia, and South Africa, there is a remarkable absence in this particular flora of forms characteristic of the Upper Palæozoic of Europe, no representative of Lepidodendron or Sigillaria occurs, and the Ferns, Cycads, and Equisetaceæ that constitute the flora are related to European Mesozoic types.

It is difficult to understand how two floras differing from each other far more widely than do any two continental floras living on the earth's surface at the present day, can have coexisted unless there was, for a long period of geological time, a great southern continent—the Gondwána-land of Suess—isolated by a wide sea, probably an ocean, from the land that occupied in Carboniferous and Permian days so wide an area in the northern hemisphere. The importance of the new discovery is the immense extension that it gives to Gondwana-land, and the proof it affords that the region with its flora extended to the western hemisphere, and included a part, at all events, of South America. This appears to indicate that a considerable area now occupied by ocean in the southern hemisphere was land in the Carboniferous period. Further research is needed to show whether the various tracts of Gondwana-land were connected by a South Polar land area.

W. T. BLANFORD.

October 4.

About a certain Class of Curved Lines in Space of [illegible] Manifoldness.

The class of curves [illegible] considered is defined by the following [illegible]: A curve of that class situated in plane space of n [manifol]dness is cut by a S_{n-1} in n different [or] coinciding [poin]ts. In the plane it is therefore a conic, and in space a [illegible].

[If th]rough $n-1$ of its points a pencil of S_{n-1} is drawn, then [each] element of that pencil cuts out of the curve [one] additional [point], and has with a straight line one point in common. The [coordinates] of the curve must therefore be expressible as [rational] functions of one parameter. If any fixed pyramid A_1, A_2 . . . A_{n+1} is [selected] as pyramid of reference, then any [poin]t P of the curve

$$(\Sigma\chi) \;.\; P = \chi_1 A_1 + \;.\;.\;.\; + \chi_{n+1} A_{n+1},$$

[supp]ose χ to be the homogeneous coordinates of P; and it [follo]ws

$$\chi_1 = R_1(\lambda, \mu) \;.\;.\;.\; \chi_{n+1} = R_{n+1}(\lambda, \mu).$$

[Her]e the R are homogeneous and integer functions of the [λ], μ. To ensure that a S_{n-1} has n points exactly with the [cur]ve in common, necessitates that the degree of the R is $= n$.

It follows from the definition that no S_k can have more than [$k+$]1 points in common with the curve (unless the curve is [who]lly contained in the S_k), as otherwise through this S_k and [$n-k-1$] additional points belonging to the curve a S_{n-1} might be [const]ructed, having more than n points in common with the [curve].

The curve is uniquely determined by any $n+3$ of its points; [and] between any $n+4$ of its points a certain condition is fulfilled (from which for $n=2$ the well-known Chasles and Pascal theorems for conics are easily deducible). To construct [thi]s condition and verify this proposition, let us return to the [art]icle entitled "Metrical Relations," &c., of NATURE, August [8]. There it was pointed out that a point and a S_{n-1} may have [a pe]culiar situation in regard to a pyramid of n manifoldness, by [vir]tue of which to each point of the S_n corresponds one S_{n-1}, [and vice vers]a. It is not difficult to verify that when the co[ordinat]es of the point in regard to the pyramid are

$$a_1 \;.\;.\;.\; a_{n+1},$$

[the]n the coordinates [χ] of the points of the S_{n-1} satisfy the [equatio]n

$$\frac{\chi_1}{a_1} + \frac{\chi_2}{a_2} \;.\;.\;.\; + \frac{\chi_{n+1}}{a_{n+1}} = 0.$$

[If] point and S_{n-1} have that relation to a pyramid, then they [ma]y be called pole and polar to it. It will be remembered that [the co]nstruction of pole to polar, and *vice versa*, is a purely [projecti]ve one, by means of cuts of plane spaces, &c. The [rel]ation of $n+4$ points of the curve to each other is now, [that] the polars of any three with regard to the pyramid of the [oth]er $n+1$ have a S_{n-2} in common.

[Indeed], let A_1 . . . A_{n+1} be $n+1$ points of the curve, and [illegible] the other points, also

$$\Sigma\chi \;.\; P = \chi_1 A_1 + \;.\;.\;.\; + \chi_{n+1} A_{n+1} \text{ and } \chi = R(\lambda, \mu).$$

[Th]en, A_1 being a point of the curve, R_2 . . . R_{n+1} must have a [common] [illegible] point; and the same is true for R_1 R_3 . . . R_{n+1}; [illegible] R_1 . R_2 . . . R_n [illegible]. It is therefore easily seen that the [coordinates] of P may be put into the form

$$\chi_i = \frac{1}{\gamma_i\lambda + \delta_i\mu}, \text{ where } \gamma_i \text{ and } \delta_i \text{ are constants.}$$

The polars of P form, therefore, a pencil; that is, they have a [illegible] in common.

[illegible] are projected from any one of its [illegible] S_{n-1}, they form a curve of the class considered in [illegible] verified from the representation of the co[ordinates] by [a] parameter. For $n=1$ the curve becomes a [illegible] form a homographic range with that [illegible] points are the representatives of the [illegible]. [illegible] four points of the [illegible] S_{n-1} of [illegible].

[illegible] always into straight [illegible]. [illegible] It is also obvious, that each [illegible] a conic and [illegible] point in common that is not situated [in] the same plane), or into three straight lines, of which one has one point in common with each of the other two.

In each point of the curve there is one straight line, that has two coinciding points in common with the curve, and one plane, that has three points of intersection which all coincide, &c. They may be called tangent lines, planes, &c., of the curve. Cut the curve by a S_{n-1}. If the n points of intersection are distinct, draw the n tangent S_{n-1} through them; and if only $n-2$ are distinct, and 2 coincide, draw the $n-2$ tangent S_{n-1}, and the one tangent S_{n-2}; and so on.

The point of intersection of these plane spaces may be called the *pole* of the original S_{n-1} to the curve; and this one, the *polar* of that point. The polar of any point of the polar passes the pole. Let the pyramid of reference be chosen so that the equation of the curve is

$$\chi_1 = \lambda^n \quad \chi_2 = \lambda^{n-1}\mu \;.\;.\;.\; \chi_{n+1} = \mu^n.$$

The S_{n-1} may satisfy the equation

$$p_1\chi_1 + \;.\;.\;.\; + p_{n+1}\chi_{n+1} = 0.$$

The n points of intersection are then given by

$$p_1\lambda^n + \;.\;.\;.\; + p_{n+1}\mu^n = 0.$$

Their roots may be

$$\lambda : \mu = \alpha_1, \; \alpha_2 \;.\;.\;.\; \alpha_n.$$

Through $\chi_1 = \alpha_i^n$, $\chi_2 = \alpha_i^{n-1}$ . . . the tangent S_{n-1} (whose coordinates may be ξ) $a_1\xi_1 + \;.\;.\;.\; + a_{n+1}\xi_{n+1} = 0$ will be such that

$$a_1 = 1 \quad a_2 = n\beta \quad a_3 = (n)_2\beta^2 \;.\;.\;.\; a_{n+1} = \beta^n,$$

where β is a parameter, whose value is found $= -\alpha$. The point of intersection of the n S_{n-1}, whose equations are

$$\xi_1 - n \,.\, \alpha_i \quad \xi_2 + (n)_2 \,.\, \alpha_i^2 \quad \xi_3 - \;.\; \pm \alpha_i^n \xi_{n+1} = 0$$

is obviously

$$\xi_{n+1} = p_1 \quad \xi_n = -\frac{p_2}{n}$$

$$\xi_{n-1} = \frac{p_3}{(n)_2}, \;\; \&c.$$

(on account of the equation satisfied by the α).

If ξ_i is any point, and χ_i any point on its polar, the equation exists

$$\xi_{n+1}\chi_1 - n\xi_n\chi_2 + (n)_2\xi_{n-1}\chi_3 - \;.\;.\;.\; = 0,$$

which is symmetrical, and therefore proves the proposition.

The polar to a line joining two points is the cut of their polars; and so generally. It is therefore possible to speak of the polar, or pole, of any plane space, in regard to the curve. The two are united only when the two sets of coordinates are equal, that is, when they satisfy a condition of the second degree. Pole and polar cut a straight line in involution, as immediately follows from the symmetry of the equation connecting them. The double points of the involution are the points in which the straight line cuts that surface of the second order.

Much more could be said concerning this class of curves, the properties of which are so much like those of the conics; but I hope that what has already been mentioned will be found sufficient to interest mathematicians in their existence.

London, September 6. EMANUEL LASKER.

The Freezing Point of Silver.

THE subject of high temperature thermometry has recently attracted considerable attention, and on account of the ease with which silver can be obtained in a pure state, coupled with its great thermal conductivity, the freezing point of this metal has been suggested as a standard temperature. We therefore wish to call attention to an error into which we believe M. le Chatelier has fallen with regard to this constant. In the *Zeitschrift für Physikalische Chemie*, Band viii. p. 186, he says that the melting point of silver can be lowered by as much as 30° through the absorption of hydrogen; again, in the *Comptes rendus* for August 12, 1895, he states that the melting point of this metal is lowered by a reducing atmosphere. He therefore recommends that when the melting point of silver is used as a fixed point in calibrating pyrometers, the experiment should be performed in an *oxidising* atmosphere. This conclusion is contradicted by Prof. Callendar's experiments and by our own, for in the *Phil. Mag.*, vol. xxxiii. p. 220, Callendar shows that the freezing point of silver is lowered and rendered irregular by an oxidising atmosphere; and our own results confirm this

conclusion. But serious doubt having been raised on this point by so high an authority as M. le Chatelier, we have thought it right to make further experiments.

These experiments convince us that the freezing point of molten silver is lowered and rendered variable when the surface is exposed to the air. We also find that by blowing oxygen through the molten metal, the absorption of this gas is sufficiently great to lower the freezing point 20°. Moreover, when the oxygen is removed by the action of either carbon, coal gas, or hydrogen, a constant maximum freezing point is reached. Further, if the atmosphere of hydrogen, or coal gas, be replaced by carbon dioxide, there is no change in the freezing point, whilst if nitrogen be used to sweep out the hydrogen, there is a slight fall. In neither case does the removal of the hydrogen bring about a rise, as should be the case on M. le Chatelier's hypothesis.

Another strong reason for believing that the true freezing point of silver can only be obtained in a reducing atmosphere, is to be found in the remarkable constancy with which a considerable mass of pure silver maintains its temperature from the moment that freezing commences until the whole is solid, provided it has not been exposed to the action of free oxygen. It is also noteworthy that in a reducing atmosphere the melting and freezing points are identical.

Impure substances do not as a rule behave in this way, and hence it is improbable that the silver can contain dissolved hydrogen. In an oxidising atmosphere the freezing point is less sharply marked, and the silver behaves as if it were impure.

These are our reasons for venturing to differ from M. le Chatelier, and we hope that he will further examine the question.

Cambridge, October 12. C. T. Heycock.
F. H. Neville.

Plant-Animal Symbiosis.

In your issue of August 22, 1895, Mr. Schwarz describes his finding in South Africa some ants inhabiting the thorns of a mimosa tree, by which he evidently means a species of *Acacia*. This symbiosis is well known out here, and probably also in Europe, as will be seen by a reference to Schimper's "Wechselbeziehungen zwischen Pflanzen und Ameisen im tropischen Amerika," p. 48. I first observed ants inhabiting the thorns of *Acacia horrida* in the neighbourhood of Grahamstown about six years ago. I also found them near Port Alfred. As far as my repeated observations go, the partnership between the ants and the trees is a very one-sided one.

The former receive shelter and food from the trees, whereas I have failed to find that the latter derive any advantage from it. This last conclusion is not surprising, as, firstly, amongst the "mimosa"-scrub near Grahamstown, one only finds here and there a tree the thorns of which are inhabited by ants, and as, secondly, in some years *all* individuals of *Acacia horrida* are completely denuded of their foliage over wide areas by caterpillars. Moreover the ants (of which I found two different kinds) are, as Mr. Schwarz rightly observes, not at all aggressive, whereas Belt showed that the little ants living in the hollow thorns of *Acacia sphaerocephala* in Central America are very pugnacious, and protect the plant against browsing mammalia and insect enemies.

The two cases are, therefore, very different from one another.

S. Schonland.

Albany Museum, Grahamstown, South Africa,
September 16.

The Recent Dry Weather.

With reference to the recent remarkable weather, both at the commencement of the year and during September, it is worth while calling attention to the climatological period of about thirty-five years, which Prof. Bruckner, of Berne, pointed out as existing relatively to the years or groups of years characterised by marked cold or heat, as mentioned in vol. xliii. p. 163 of Nature. He therein indicated the years 1700, 1740, 1780, 1815, 1850, and 1880 as centres of cold periods, while the years 1720, 1760, 1795, 1830, 1860 (and now 1895) appear as centres of warm, dry periods. The coincidence for the present year is certainly remarkable, and merits attention as to the causes which underlie these periodic fluctuations of weather.

Dublin, October 11. J. P. O'Reilly.

The Genus "Testacella."

In Nature for last year the writer gave a list of the localities for *Testacella scutulum* which had come under his notice. With a view to making this list more complete, and to obtaining a more definite idea of the distribution of the various species of the genus in the British Isles, the writer would be greatly indebted to any reader of Nature who could forward to him, localised specimens of *Testacella*, alive, or preserved in alcohol, the present month being a likely one for the coming above ground of these slugs, which should now be found under logs and stones in the neighbourhood of rich garden soil.

Wilfred Mark Webb.

"Holmesdale," Brentwood, Essex.

The B.A. Committee on Coast Erosion.

In the reference, in your number of Oct. 3, to "Geology at the British Association," the statement as to the Coast Erosion Committee, in their final report, recommending a "Departmental Committee of the House of Commons," to inquire into the subject, is taken from the "first proof" of the report, which was drafted by myself as surviving Secretary. The suggestion has *not* been adopted by the majority of the Committee, who considered their duty did not extend to drawing up and formulating recommendations. This termination I regret, as when the Association adopted my suggestion in 1881, to appoint this Committee, I hoped it would have had a practical outcome, leading to the conservation of our coasts.

Charles E. De Rance.

A Substitute for Sulphuretted Hydrogen.

In your Notes of February 14 last, you state that ammonium thio-acetate has been found to be a satisfactory substitute for sulphuretted hydrogen in chemical analysis. Can any of your readers tell me where I can obtain it? I cannot find it in catalogues of chemical manufacturers. Rusticus.

THE GRAPHICS OF PIANO TOUCH.

Much trouble has been taken in order to construct an apparatus that will reproduce graphically the effects of touch in keyed musical instruments. The experiments are most easily made with the piano, and have therefore been tried on that instrument.

Recently a most interesting article appeared in the *Revue Scientifique*, written by MM. Binet and Courtier, who have studied this subject closely, and have made many experiments with their apparatus. They have treated the matter very fully in their article, of which the following is a *résumé*:

When a certain point of perfection has been attained in piano playing, it becomes very hard to distinguish inequality of touch; yet, owing to the varying strength of the fingers, it is only with much practice that perfect equality is possible. As will be seen further on, involuntary movements and irregularities, scarcely perceptible to the ear, are shown by the graphical method.

The apparatus (Fig. 1) is quite simple in construction, and consists chiefly of an india-rubber tube, placed under the key-board, united at its two extremities by a registering drum, also of india-rubber. When the notes of the piano are played, the pressure on the tube causes a wave of air to be sent through it into the drum, upon which is attached a pen that in the ordinary way is made to record its movement on a moving roll of paper. The wave makes the drum vibrate, which in its turn jerks the pen, thus causing irregular marks to be left on the paper. The board on which the tube rests is regulated by means of wedges adjusted by a screw, the board being either lowered or raised. When raised it almost reaches the notes of the piano, and in this case the registering action takes place; but if it is lowered, the whole apparatus is disconnected from the key board.

When no notes are being played, and the registering drum is connected, *i.e.* the board is raised, merely a straight line is drawn. In Fig. 2, first *a* is struck, then two notes with *b*, then three notes with *c*, and so on. It

is difficult to tell whether the mark made for each additional note is the same length, for when three notes are struck they may not each be struck with the same force. In the second case (Fig. 2) one note is struck, held down and another struck, and so on, the previous notes always being kept down. The effect produced is

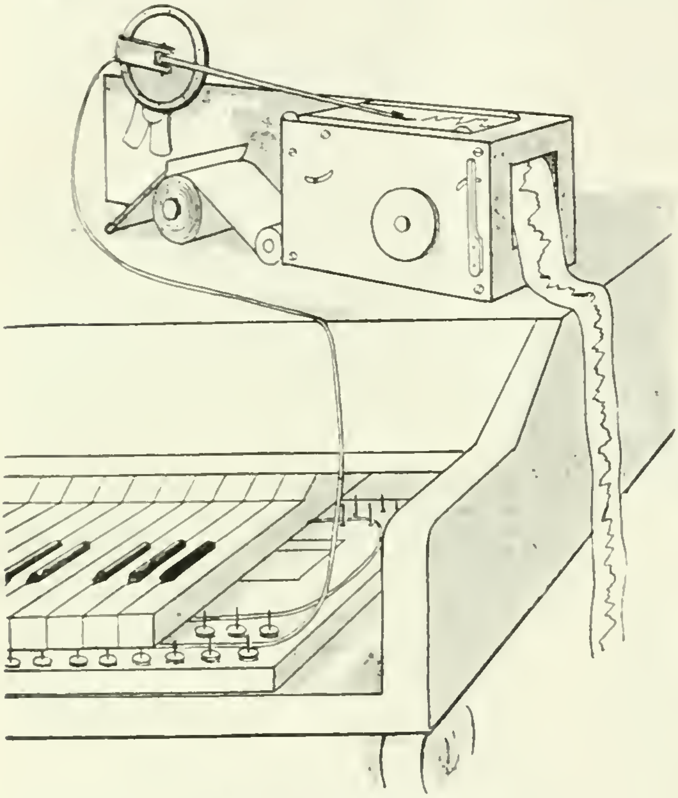


FIG. 1.—Illustration of the apparatus.

curiously like a flight of stairs, but the height of each stair is not absolutely equal. This proves that the apparatus is sufficiently sensitive to show, by the height of the lines, the intensity with which a note is struck.

With regard to *time*, it is reproduced with the utmost precision, and it is in order to guarantee accuracy that the ... connected to the drum at both ends, otherwise the notes situated near the end which was not connected, would be farther removed from the drum than the others, and this would cause a delay in their being registered.

The graphical *form* ought theoretically to be an imitation of the movement played, but this result is not often achieved. When very quick passages are being played, the strong wave of air shakes the drum so forcibly, that

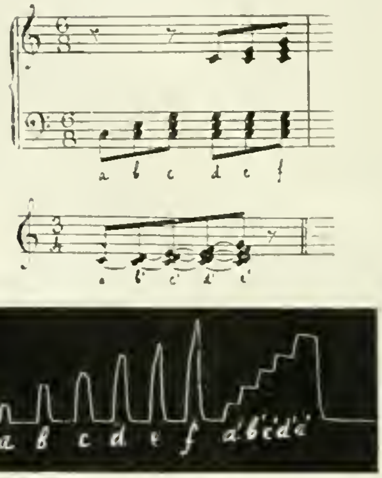


FIG. 2.—Effects produced: *a*, in striking one note, and *b*, *c*, *d*, *e*, *f*, in striking chords of two up to six notes; in the second case, *a'*, *b'*, *c'*, *d'*, *e'*, in playing five successive notes, and keeping them down.

FIG. 3.—A represents effect without the insertion of the diaphragm, B the effect with the diaphragm.

the pen ceases to act properly. Much trouble has been taken to devise a way of lessening the force of the wave; amongst other materials, cotton has been inserted into the tube, but these experiments were not entirely successful. It has been found that placing a diaphragm with a small hole in the tube, lessens the force of the wave of air very considerably, and to a certain extent prevents the drum vibrating too strongly.

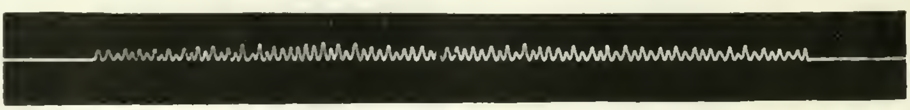


FIG. 4.—Shake executed with first and second finger.

FIG. 5.—Effects produced by an irregular shake.

The effect produced without and with the diaphragm will be seen in Fig. 3.

Figs. 4 and 5 illustrate shakes, and show clearly the importance of equality of touch: they show, too, how precisely the apparatus reproduces any irregularity.

Many questions have to be considered with regard to quick playing, but one of the most striking features is that the more quickly the notes are played, the more the force of the movement diminishes, until finally a certain stage is reached, when the amplitude ceases to vary.

Let us now consider the advantages of the instrument; they are threefold.

(1) Dealing with its advantage from the psychological point of view, it is found that the voluntary movements of the pianist can be observed without putting him to any restraint or embarrassment, for the small tube does not affect the resistance of the notes, nor is the exterior of the piano altered.

(2) For teaching purposes the device has been of great use. The record on the roll of paper shows the faults so precisely, that although they are scarcely perceptible to the ear, there is no denying their existence.

(3) We are well aware that written music cannot show every slight change in the time the composer might desire. By applying the graphical method, this difficulty is eliminated, and the time will be reproduced with the smallest details.

THE NEW METEOROLOGICAL STATION ON MOUNT WELLINGTON.

A VIEW of the new meteorological observatory on Mount Wellington, Tasmania, is shown in the accompanying illustration. As we announced in a previous issue (July 25), the observatory was begun in May last, and it will be to the southern hemisphere what the Ben Nevis and other high-level observatories are to the northern. Mount Wellington is about four miles distant from Hobart, and rises almost directly from the level of the sea. The station is supplied with a "Fortin" mountain barometer, "Richard" barograph and thermograph, dry-wet, and maximum and minimum, thermometers, as well as a "5-inch" gauge with extra deep rim for retaining snow. Similar instruments are in use at the Springs (2495 ft.) and at Hobart, 160 feet above sea-level. Mr. Clement L. Wragge, Superintendent of the Chief Weather Bureau, Brisbane, has organised the stations. Very valuable results, bearing upon the distribution of pressure, temperature and humidity attaching to anticyclonic and cyclonic systems through vertical sections of the atmosphere in the northern and southern hemispheres respectively, will probably be forthcoming when the Mount Wellington and Hobart results appear and are discussed side by side with those obtained at Ben Nevis and Fort William. Except for a few degrees of latitude, Mount Wellington and Hobart are geographically and physiographically almost the very counterparts in the southern hemisphere of Ben Nevis and Fort William in the northern. Mr. Wragge has entirely reorganised the Tasmanian Government Meteorological Service on federal principles in direct connection with the Queensland Weather Service, and he was enabled to perform this work through the courtesy of the Queensland Government, who allowed him as their officer to render federal aid in the cause of science to the sister colony. Mr. H. C. Kingsmill has charge of the Tasmanian section.

The barometer cairn, now a larder, and barometer transferred to house (4166 feet).

The Observatory, Mount Wellington (4166 feet above sea-level).

DR. E. VON REBEUR-PASCHWITZ.

E. VON REBEUR-PASCHWITZ was born in 1861, and died, after an illness of ten years, on the first of the present month. In many ways he always seemed to me to resemble our incarnation of the ideal man of science. He had Darwin's lovable nature, as well as his modesty and utter carelessness of his own fame. But the likeness was closest in the unceasing energy with which he laboured, in spite of the constant suffering that would have made many stronger men feel their life's work was done.

For some time von Rebeur-Paschwitz was a Privat-docent in Astronomy at the University of Halle. His first notable achievement was, I believe, the modification of Zöllner's horizontal pendulum, the two springs by which it was supported being replaced by agate cups resting on fine steel points. The earlier investigations with this instrument were intended to be of an astronomical character, but its wonderful sensitiveness to the pulsations of distant earthquakes soon became apparent, and he was gradually led to give more time to their study, until he became the chief authority on this fascinating branch of seismology. On two occasions he contributed articles to NATURE on this subject (vol. xl. pp. 294-295; vol. li. pp

2 S 211), and, at the request of the Earth Tremors Committee of the British Association, he wrote an admirable summary of his results up to the middle of 1893. As this is readily accessible, it is unnecessary to enlarge upon his achievements here. I will merely add that since that date he has written several papers on earthquake-pulsations in Petermann's *Mittheilungen* and the *Astronomische Nachrichten*. His last memoir, and one of the most valuable, has just been published in Gerland's *Beiträge zur Geophysik*.

For several months before his death, von Rebeur-Paschwitz was occupied with a scheme for the organised study of earthquake-pulsations all over the globe. The suitability of his horizontal pendulum for this purpose had received ample proof, and nothing but the want of health seemed likely to prevent the fulfilment of his plans. These, no doubt, will be carried out by other, if less skilful, hands; but to him will belong a great part of the credit for any results that may be attained. Dying at thirty four, he had done work which most men of twice the age might regard with satisfaction as the fruits of a well-spent life. CHARLES DAVISON.

CHARLES V. RILEY.

CHARLES V. RILEY, M.A., Ph.D., whose death on the 14th ult., in consequence of injuries received in a fall from a bicycle in the streets of Washington, was announced in these columns on October 3, was an Englishman, born at Walton-on-Thames in 1843. He emigrated to the United States at the age of seventeen, and settled, as we learn from the *Garden and Forest*, on a farm in Illinois. Like so many other Americans, who have since made a reputation in science, he served as a soldier in the civil war. Subsequently, after some experience as a journalist, he was appointed State Entomologist of Missouri, a position he occupied nearly ten years. During this period he did excellent work in the investigation of the life-histories of insects injurious to plants, and experiments to discover the most effectual means of destroying them. But one of his earliest papers was on a new genus *Pronuba* of the Tineidae, and the part it plays in the fertilisation of *Yucca*.[1] This was an important and interesting contribution to biological science. In 1878 he accepted the post of Entomologist to the United States Department of Agriculture at Washington, where, in the words of the authority cited above, he practically supervised all the entomological work of the Government until his resignation last year. The valuable results of the investigations and experiments conducted by him and his staff, were in part published in occasional bulletins, of which thirty-two appeared between 1883 and 1894, and partly in the now familiar periodical entitled *Insect Life*, which was established in 1888. Six volumes appeared under his editorship. Dr. Riley was an indefatigable worker, and his organising and administrative qualities were well exemplified in the department which he so successfully developed. W. B. H.

NOTES.

I[illegible] in order to enable the Berlin Academy of [illegible] complete edition of Kant's works, the Govern[illegible] consented to place at its disposal for a [illegible] manuscripts belonging to the University [illegible]

[illegible] *[illegible] Medical Journal*, the New York [illegible] has purchased thirty five acres of land near [illegible] experiment station is to be established. [illegible] stocked with cows, horses, sheep, [illegible] for the production of diphtheria and cancer antitoxins. The situation is healthy, and in the grounds there will be a house in which some of the patients of the Institute will be treated. A new station, to be known as the Pasteur Station, will be established on the Erie Railroad, close at hand.

WE regret to notice the following announcement in *Science*:—"Prof. Ernst Ritter, whose appointment as assistant professor of mathematics in Cornell University was recently announced, died on September 23, of typhoid fever, on his arrival in America from Germany. Ernst Ritter was born at Waltershausen, Germany, on January 9, 1867. He spent twelve years at the Gymnasium at Gotha, and afterwards studied mathematics and natural science under Thomas, at Jena, and under Klein and Schwartz, at Göttingen. In 1890 he passed the Government teacher's examination with the highest distinction, after two years of pedagogical work at Cassel, and at the Wohlerschule in Frankfurt. He took the degree of Ph.D., *summa cum laude*, at Göttingen in 1892. In 1893 he was appointed assistant to Prof. Klein, and began to devote his entire time to mathematics, contributing regularly to mathematical periodicals. Last year he lectured on geometry and the theory of automorphic functions, in which he was an authority. He was appointed to his Cornell professorship last June."

WE learn from the *Journal* of the Franklin Institute that the German Hygienic Association offers a prize of 1200 dols. for a research essay on the efficiency of electric heaters. The programme is as follows: "The heat given out in heating installations by heaters in their various forms and modes of use is to be ascertained. The investigations are to be described in detail in respect to the arrangement of the heaters, the nature of the heating agents, and the observations made; and they are to be illustrated by drawings. The heating values obtained are to be stated in units of heat given off per hour per unit of surface. In the case of heat given out to air, the investigations must be conducted with currents of air at speeds as different as possible. The heaters are to be described in detail as regards form and measurement, and the relation of their heating efficiency to their weight is also to be ascertained." Essays are to be written in German, and sent, with a motto and sealed envelope, to Prof. Konrad Hartmann, Charlottenburg, Fasannstrasse 18, before July 1, 1896. The essay will remain the property of the successful competitor, but he is required to publish it within six months, and to give the prize offerers gratuitously 300 copies. The offerers reserve the right to divide or withhold the prize.

THE display of horseless carriages, held at Tunbridge Wells on Tuesday, under the superintendence of Sir David Salomons, will do something towards the introduction of self-propelling light vehicles in England. Two carriages, fitted with Daimler motors, were shown in operation. One of these, that belonging to Sir David Salomons, weighs 13 cwt., and will run nearly two hundred miles without recharging. The motor has a horsepower of 3¾, and a speed of fifteen miles an hour can be attained on a level road, while on a gradient of one in ten a speed of four miles an hour is reached. A mechanical tricycle, worked by a petroleum motor with electric spark ignition, was shown by MM. de Dion and Bouton, of Paris. The tricycle can run at a rate of fourteen miles an hour, and only needs a fresh supply of benzine after about six hours' work. The exhibition proved the capabilities of auto-mobile carriages to a large number of spectators, and it will probably do something to bring about a change in the present vexatious Highways and Locomotives Act, which at present limits the rate of speed of self-propelled carriages to two miles an hour, and makes it necessary for a man carrying a red flag to precede the carriage as a warning of approaching danger!

[illegible]

The first series of lectures given in connection with the Sunday Lecture Society begins on Sunday afternoon, October 20, in St. George's Hall, Langham Place, at 4 p.m., when Prof. Sir Frederick Pollock, Bart., will lecture on "Tyndall as Worker and Teacher." Lectures will be subsequently given by Dr. C. W. Kimmins, Rev. Stewart Headlam, Prince Kropotkin, Mr. Graham Wallas, Mr. Wyke Bayliss, and Dr. R. D. Roberts.

Facts are always worth recording, and we publish the following note because it contains an interesting fact, which is, moreover, in accordance with other observations. The note came to us from Mr. Mata Prasad, Benares: "It was quite accidentally observed, by a stammering friend of mine, during the months of May and June last, that on moonlight nights he stammered more than on dark nights, and when he slept exposed to the rays of the moon during the month of June, he found that he stammered the most on days succeeding full moons, while a day just after the new moon, and a day before, he had not a single attack of the fit."

The organisms responsible for the production of the Japanese beverage saké are still the subject of comment and investigation. Only a few weeks ago we received a communication from Dr. Jorgensen, in which he claimed to have discovered that the mould known as Aspergillus oryzæ, employed in the preparation of sake, was capable of producing the yeast cells invariably present, and that, therefore, only one organism was responsible for the elaboration of this well-known beverage. Mr. Atkinson, who investigated this subject some years ago in Japan, could find no evidence of the transformation of the mould into yeast cells, and maintained that the mycelium and the ferment were entirely distinct. This view has been quite recently upheld by some experiments published by Messrs. Kosai and Yabe, of Tokio. They have found that in the preparation of saké two distinct organisms are required, the well-known Aspergillus and a species of yeast. These have been carefully isolated and their growth watched in various solutions, with the result that the mould only gave rise to typical mycelium growths, whilst the yeast elaborated only yeast cells, without exhibiting a trace of mould. The authors are now engaged upon carefully identifying this sake-yeast, and state that, as far as their investigations at present go, it resembles the *Saccharomyces cerevisiæ*, with which they are carrying out numerous comparative experiments.

Only those who have much to do with scientific literature know how important, and yet how much neglected, is the art of making references. No apology is needed, therefore, for reprinting in full the following rules abstracted from a paper that appeared in the *British Medical Journal*, 1895, vol. i. p. 875, by Mr. J. B. Bailey, Librarian of the Royal College of Surgeons of England. The rules can be obtained printed on a card, so that an abstractor can always have them before him. (1) The titles of all books and periodical publications should be given in the language in which they are written. (2) References should be taken from the title-pages, and not from the lettering on the backs of books. (3) Where two, or more, vols. are bound together, care should be taken that the reference is made from the right title-page. (4) Where a journal is in more than one series, the number of the series as well as the vol. and date should be given. (5) When an abstract only of a paper is referred to, this fact should be stated, and reference to the original paper given if possible. (6) Journals and Transactions should not be quoted by the date of issue, but by vol., date and page. (7) In books which have two sets of paging, care should be taken to specify exactly the pagination to which reference is made. (8) The name of the editor of a journal should not be used as part of a title unless it be necessary to distinguish between two journals with similar titles. (9) References to papers read before Societies which do not publish any separate reports of their meetings should quote the journal where the paper in question can be found. (10) In abbreviating titles care should be taken that the abbreviation shows exactly what journal is referred to, *e.g.*, *Jnl. Anat. Physiol.* does not make it clear whether an English, French or German book is quoted.

The Smithsonian Institution has recently published a series of directions for collectors, as separate portions of *Bulletin* No. 3 of the U.S. National Museum. The directions for collecting minerals, rocks, and fossils (parts H, I, and K) are written by the curators of the respective departments, and include advice not only on actual collecting, but on preparing, labelling, making sections, &c. Many of the recommendations are novel, and all cannot fail to be helpful to amateur collectors.

The *Bulletin of Miscellaneous Information* of the Royal Gardens, Kew, for September, continues the *Diagnoses Africanæ*, in which, in addition to a large number of new species, two new genera are described: *Cyclocheilon*, Oliv., belonging to the Scrophulariaceæ, and *Phillipsia*, Rolfe, belonging to the Acanthaceæ. An interesting account is given of the history of the rock-garden, based on a list of herbaceous plants cultivated in the Royal Gardens, Kew, issued by the Department.

Under the modest title of "Guide to the Collections of Rocks and Fossils," the Geological Survey of Ireland has published what is really an excellent guide to the geology of Ireland. The authors are Messrs. W. W. Watts and A. McHenry, and the price of the book is ninepence. It opens with a short introduction, explaining the principles on which is based the classification adopted in the Science and Art Museum, Dublin. Two-thirds of the book are taken up with an account of the rocks of Ireland, each of the four provinces being taken in turn. Part iii. begins with a popular account of general Palæontology, which is followed by a description of the fossils exhibited, and this by a catalogue of figured and type specimens in the museum. Finally we have an index of localities for the rocks described, that should be most useful to amateur geologists.

The Observatory of Manila has published an extensive discussion of the typhoons of the year 1894, prepared by the Rev. J. Algué, S.J. The work occupies 176 small folio pages, and is accompanied by a large number of plates showing the tracks of the different storms and concomitant data, and also contains some general considerations respecting the character of these disturbances in the extreme East. A section is devoted to the distribution of the various meteorological elements around the centres of areas of low barometic pressures at Manila during the years 1879-94. The result of this discussion shows that the distance of the cyclonic centre cannot be determined from the reading of the barometer alone; but the author describes an apparatus, which he calls a "cyclonoscope," whereby an aproximate idea of the distance of the vortex may be determined.

W. Engelmann, Leipzig, will shortly publish the collected papers of Prof. W. Roux upon the "Entwickelungsmechanik der Organismen." The work will consist of two volumes, illustrated with lithographic plates, and numerous illustrations in the text.

The sixth part of *Bulletin* No. 9 of the *Minnesota Botanical Studies* (August 1895), is entirely occupied by a very useful "Contribution to the Bibliography of American Algæ," by Miss Josephine E. Tilden. No less than 1544 separate works or papers are enumerated.

The discourse entitled "The Splash of a Drop," delivered by Prof. A. M. Worthington, F.R.S., at the Royal Institution in May 1894, has been published in book form by the Society for Promoting Christian Knowledge, with illustrations of the

beautiful phenomena described. The arrangement employed to obtain photographs of drop-splashes, and some of the results, were shown in NATURE of July 5, 1894.

DR. E. RUDOLPH, who has given much attention to submarine earthquakes and eruptions, has recently contributed a second valuable memoir on "Seebeben" to the *Beiträge zur Geophysik*. It contains accounts of more than two hundred additional shocks, and also a small map of the seismic zone of the Equatorial Atlantic. The memoir concludes with a useful list of questions for the observation of submarine earthquakes.

M. DE FONVIELLE has translated into French Lord Salisbury's Oxford Address to the British Association, and MM. Gauthier-Villars et Fils have just published the translation in their series of Actualites Scientifiques, under the title "Les Limites Actuelles de notre Science." The address is prefaced by a long introduction, in which the translator describes the circumstances under which it was given; and throughout the pages there are numerous notes explanatory of points, the importance of which might be overlooked by French readers.

A NEW volume in the Aide mémoire Series, published jointly by Gauthier-Villars and Masson, is "Polarisation et Saccharimétrie," by D. Sidersky. The volume is a handy aid to the study of polarisation and its numerous applications in analytical chemistry. The first part contains a description of the properties of polarised light, a table of the specific rotatory powers of various optically active substances, and explanations of polarising apparatus. The second part of the book is devoted to the applications of the constant of rotation to the quantitative analysis of sugars, alkaloids, &c., together with a number of tables which will facilitate the practical application of the processes described.

By the recent publication of two numbers of the *Essex Naturalist*, the Essex Field Club has brought their journal up to date. The first number (November–December 1894) includes papers on "Izaak Walton's Association with the Lea," by J. E. Harting, the "Geology of the Lea Valley," by T. V. Holmes, and on "Navestock in Olden Days," by Rev. S. Coode Hore. The second number (January–June 1895), contains a paper, by Prof. Meldola, on the "Eastern Boundary Stones of Waltham Forest," the Presidential address (in which the part played by the Club in the development of technical education in the county is explained), and a series of three papers, by Messrs. T. V. Holmes, E. T. Newton, and W. M. Webb, on the section in brick-earth at Chelmsford in which mammoth remains were recently found.

SEVERAL interesting papers are contained in the part of the *Proceedings* of the Royal Society of Edinburgh, just published (vol. xx. pp. 385–480). In "A Sketch of Lake-Dwelling Research," Dr. Robert Munro shows that over a wide geographical area, extending from Ireland to Bosnia, and from North Germany to Italy, the habit of constructing lake- and marsh dwellings was prevalent in former times. Prof. Sir William Turner, F.R.S., has a paper "On M. Dubois' description of remains found in Java, named by him *Pithecanthropus erectus*; with remarks on so-called transitional forms between Apes and Man." A paper on drops, by Mr. J. B. Hannay, summarises the work of various observers on the formation of drops, and the variation with density and chemical composition of the liquid forming them, and gives the author's own investigations upon the subject. There are also in the *Proceedings* Prof. T. R. Fraser's two papers on "Antivenine," and a paper by Prof. J. C. Ewart, "On the Dorsal Branches of the Cranial and Spinal Nerves of Elasmobranchs."

THE fourth edition, revised and enlarged, of Dr. Carl Günther's "Bakteriologie" has been published by Georg Thieme, Leipzig. We noticed the third edition in March of last year (vol. xlix. p. 455), and the present issue sustains the commendation then given, viz. that "the volume is undoubtedly one of the best introductions to the study of bacteriology which has yet been produced." Another new edition which we welcome is the "Cours Elementaire de Manipulations de Physique," by Prof. A. Witz, published by Gauthier-Villars. The book contains a descriptive course of work covering the fundamental principles and laws of physical science. Each experiment is divided up into four sections, as follows: first, the theory of the experiment is stated; then the apparatus is described; the experimental operations form the subject of another section, and the results of observations are given in the fourth. Though the book is here and there deficient in the details required by students of practical physics, it is altogether a useful companion to the physical laboratory.

THE Catalogue of the Library of the Royal Geographical Society, compiled by Dr. H. R. Mill, and lately published, is a very full and valuable index to the literature of geography. The Catalogue contains the titles of all works in the possession of the Royal Geographical Society published up to the close of 1893. The entries (amounting to as many as 18,000) are arranged in four divisions. The first division, which runs into 521 of the 833 pages, is a general alphabetical author's catalogue; the second comprises collections of voyages and travels, arranged in alphabetical order under authors' names, and containing a brief analysis of the contents of each volume; in the third division, Government, anonymous, and other miscellaneous publications are arranged geographically; while the fourth consists of a list of transactions and periodical publications, arranged in a similar manner according to the place of publication. With such a comprehensive classification, it is easy to find the works of each author, and to refer to the literature concerning different divisions of the earth. A valuable supplement to the Catalogue will be the subject index now being prepared, and in which the principal contents of all the geographical books and periodicals belonging to the Society will be classified.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*, ♀) from India, presented by the Rev. Sidney Vatcher; a Crested Porcupine (*Hystrix cristata*) from East Africa, presented by Captain B. L. Sclater; three Common Rheas (*Rhea americana*) from South America, presented by Mr. Robert Gunther; four Rhomb-marked Snakes (*Psammophylax rhombeatus*), three Crossed Snakes (*Psammophis crucifer*), two Rough-keeled Snakes (*Dasypeltis scabra*), a Smooth-bellied Snake (*Homalosoma lutrix*), a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by Mr. J. E. Matcham; a Bonnet Monkey (*Macacus sinicus*) from India, a Yellow Baboon (*Cynocephalus babouin*) from West Africa, a Rose Hill Parrakeet, (*Platycercus eximius*) from Australia, deposited; three Prevost's Squirrels (*Sciurus prevosti*) from Malacca, purchased.

OUR ASTRONOMICAL COLUMN.

THE OBSERVATORY ON MONT BLANC.—Two causes combined to induce Dr. Janssen to undertake his recent ascent of Mont Blanc. First, he was anxious to be convinced of the perfect safety of the new telescope which has been conveyed to the observatory; and second, the meteorograph had ceased to perform its various important duties (*Comptes rendus*, October 7). It is intended to mount the telescope, which has an aperture of thirteen inches, with its axis parallel to that of the earth, and a mirror nearly twenty-four inches in diameter will be employed to reflect the light of the heavenly bodies into the telescope; the mirror and telescope will have a common movement, so that the relative positions of the stars will not change on account of the diurnal motion. The meteorograph was found to be some-

what unstable, but arrangements have been made by which it is hoped that the records may be continued. A slight movement of the observatory towards Chamounix was noted, but it is expected that future displacements will be insignificant; and, in any case, the means are at hand to restore it to its original position. The practicability of the establishment of observatories on snow-clad mountains is therefore no longer to be questioned, and the multiplication of such institutions as that on Mont Blanc will no doubt contribute largely to our knowledge both in meteorology and astronomy.

It is characteristic of Dr. Janssen that he should take advantage of the opportunity of observing the aqueous bands in the solar spectrum. The air above him being very rare and also extremely dry, he found that when observing sunlight in its totality the bands at C and D were absolutely invisible, while the group at α was so pale that its presence could scarcely be determined. Dr. Janssen already regards it as certain that there is neither oxygen nor aqueous vapour in the solar envelopes, but the question is so important that too many observations cannot be made. To carry the observations a step further, it will be necessary, under analogous atmospheric conditions, to compare very carefully the centre of the sun's disc with the edge, to see if there is any augmentation of the α group as the limb is approached, this group being especially sensitive to variations in the amount of absorbing vapour.

EPHEMERIS FOR FAYE'S COMET.—The following ephemeris, for Berlin midnight, is given by F. Engström in *Astr. Nach.* No. 3313:

	R. A. h. m. s.	Decl. ° ′
Oct. 17	21 11 9	−4 20·6
19	12 10	31·7
21	13 18	42·1
23	14 32	51·0
25	15 54	5 1·0
27	17 23	9·5
29	18 59	17·3
31	20 42	24·5
Nov. 2	22 32	31·0
4	24 28	36·7
6	26 30	41·6
8	28 38	45·8
10	30 52	49·3
12	33 13	52·2
14	21 35 39	−5 54·4

The calculated brightness is practically constant throughout the above period. Perihelion passage will not occur until March 19, 1896.

VISIBILITY OF THE DARK SIDE OF VENUS.—Various theories have been advanced at different times to account for the visibility of the hemisphere of Venus which is not illuminated by the sun, but there is no general agreement as to which is the most probable. Still another explanation is offered by M. Camille Flammarion, and it has the merit of being based on careful observations made at Juvisy during August and September of the present year (*Bull. Soc. Ast. de France*, October). The planet was frequently observed in full sunshine by M. Flammarion and his assistants, and the observations appear to put the matter in quite a new light. To these observers it has several times seemed that the interior of the crescent of Venus was darker than the sky, even on the day of inferior conjunction. That this appearance was not simply an effect of contrast produced by the luminous crescent is shown by the fact that no such darkening was apparent at the exterior edge of the crescent, and again by the visibility of the obscure hemisphere when the luminous part was artificially eclipsed. The colour of the unilluminated area was slightly violet in all the varied conditions of observation. M. Flammarion considers that the observations can be best accounted for by supposing that Venus is projected on a somewhat lighter background, such as might be furnished by the zodiacal light, or an extended solar atmosphere. The violet tint which was noted may have been due to the considerable refraction of the sun's rays by the atmosphere of the planet, the reddish tinge thus produced on the planet appearing purple when seen through our own blue sky.

In the same article, M. Flammarion gives some interesting facts relating to the history of the phenomenon, and some calculations which indicate that "earth-shine" is insufficient to account for it. Under the most favourable conditions, the terrestrial light received by Venus is 12,000 times feebler than that received by the moon, and 822 times less intense than the light we receive from the full moon.

THE MELBOURNE OBSERVATORY.—The twenty-ninth report of the Government Astronomer, Mr. R. L. J. Ellery, on the work of the Melbourne Observatory during the year ending at the beginning of last June, has just come to hand. Meridian observations, the daily photography of the sun, magnetic and meteorological observations, have been carried on as heretofore. The number of plates secured, in connection with the photographic chart and catalogue, up to June 1, was 1080. Preliminary measures have been made of 238 plates to obtain the positions where possible, of five stars on each plate, to be used for the determination of the constants of the plates. Mr. Ellery refers to the important change in time-reckoning made in February last by the introduction of zone or standard time in all the Australian colonies. By the zone system, Eastern Australian time, which covers Queensland, New South Wales, Victoria, and Tasmania, conforms to that of the 150th meridian; and this makes Melbourne exactly ten hours in advance of Greenwich time, instead of 9h. 30m. 54s., which is the true difference of longitude. The retirement of Mr. Ellery from his post as Government Astronomer has already been noted in these columns. Mr. Ellery has built up the Melbourne Observatory from its very small beginning in 1853 to its present recognised position among the national observatories of the world; and we are glad to see that the Government has appointed him a member of the Board of Visitors, so that he has not entirely severed his connection with the observatory. He has been succeeded in the directorship by the chief assistant, Mr. P. Baracchi, whose pendulum observations are well known to students of terrestrial physics.

A NEW OBSERVATORY.—The New York *Nation* notes a new departure at the University of Pennsylvania, by the addition of an astronomical observatory. The observatory has already been commenced, and, when completed, it is designed to furnish better facilities, not only for instruction, but for original research as well. The new edifice is two miles from the limits of Philadelphia, and about five miles from the university buildings. The instruments are an eighteen-inch equatorial, with spectroscope attachment, by Brashear, and a meridian circle and zenith telescope, each of four inches aperture, also by Brashear. The mountings are by Warner and Swasey. This institution will be known as the Flower Observatory, and its director is Prof. C. L. Doolittle, formerly of the Lehigh University.

THE INTERNATIONAL CONGRESS OF PHYSIOLOGISTS AT BERN.[1]

II.

THURSDAY morning, September 12.—Presidents: Profs. Dastre and Wedensky. Prof. Arloing (Lyons) gave the result of his researches on the persistence of electric irritability in the peripheral ends of divided nerves. The author found that the length of time for which electric irritability was retained varied with the species of animal, and also with the individual, and further that it was different both for different nerves and for the different kinds of fibres in compound nerves, such as the vagus. For spinal nerves the irritability lasted from four to five days in dogs, and from eight to ten days in horses. In one ass the author obtained cardiac inhibition with a rise of blood pressure, upon stimulating the peripheral end of the vagus fifty-seven days after section: this result he attributed to a tetanus of the myocardium.

Dr. Arthus (Paris) defended the view that the salts of calcium are necessary to the coagulation of the blood, against that of Alex. Schmidt, who does not believe their *rôle* to be an essential one. He further discussed the action of neutral solutions of the oxalates, fluorides, &c., in rendering the blood incoagulable. He disagreed with Schmidt, who holds that they act specifically, and maintained that their effect is due solely to the fact that they precipitate the calcium salts. Arthus repeated Schmidt's experiments, and was unable to confirm his results.

Prof. v. Kries (Freiburg) discussed the phenomena of colour vision in eyes adapted for darkness.

Prof. Gamgee (Lausanne) gave the result of his researches on the violet and ultra-violet spectrum of hæmoglobin and its derivatives. He exhibited photographs which showed the

[1] Continued from p. 580.

[illegible] in the spectra of [illegible] of CO and NO [illegible], which band was shown to be displaced towards the less [illegible] end of the spectrum. This very remarkable [illegible] he showed to be due to the hæmatin [illegible] of the [illegible] molecule, and to be independent of the [illegible]. Prof. [illegible] described and showed photographs of the spectrum of [illegible], a pigment obtained from the feathers of certain birds. This substance, containing 6·0 per cent. of copper, gives a spectrum identical with that of hæmoglobin. A demonstration [illegible] at which the spectra were shown.

Prof. Burdon Sanderson (Oxford) gave a demonstration, illustrated by slides, of electrometric photographic curves, and [illegible] the following propositions.

Proposition A.—There are two kinds of electrical response [illegible] excitation of a muscle by its nerve. I. Diminution of the E.M.F. of the previously existing [illegible] current. II. The excitation wave. The first (I.) is evoked when the nerve is stimulated by equal alternating currents of great frequency, (b) by the constant current, (c) by salt crystals, &c. The second (II.) [illegible] in uninjured muscle (a) by single electrical and [illegible] momentary excitations, (b) by rhythmically repeated momentary excitations (true tetanus). I. and II. exist together in rhythmical excitation of injured muscle.

Proposition B.—In the reflex spasm of strychnine each phase of excitation resembles I. The strychnine spasm is no true tetanus.

Dr. Waller (London), gave the results of his researches on the influence of chemical reagents on the electrical excitability of isolated nerve. Excised frogs' nerves were rhythmically excited once a minute during one-eighth of a minute. The current of action was recorded by a galvanograph. The nerve was enclosed in a gas chamber to study the effects of gases, and dipped for one minute in decimolecular solutions for salts, and 1 per cent. solutions for alkaloids. By means of this method Dr. Waller was enabled to study the action of a large number of bodies, the results of which he gave; his researches also extended to the study of physiological antagonism.

Dr. Epstein (Bern) showed that the increase in the acuteness of vision, which occurs under the influence of auditory impressions, can be experimentally demonstrated. The author described the apparatus. He further expressed the opinion that the centre for this process lay not in the cortex, but in the superior corpora quadrigemina. He regarded it as essentially dependent upon [illegible] reflex sensibility of the retina, the optic nerve containing the efferent fibres; in favour of this view, he quoted an experiment in which by faradic excitation of the nerve from the cochlea he obtained eye movements, and increased conjunctival reflex.

Tuesday Afternoon.—Presidents, Profs. Nitzau and Fredericque. Prof. Rutherford (Edinburgh) gave an account of his researches on the structure and contraction of striped muscular fibre. His observations were mostly made upon crabs' muscles fixed in a 4 per cent. solution of formaldehyde, and then stained with [illegible] or [illegible]. He regards the fibrils as the essential elements of the sarcous substance, each fibril has in its whole length a [illegible]. He regards the fibril as composed of a series of segments: (1) Bowman's element, (2) the intermediate segment placed midway between successive Bowman's elements, and (3) the clear segment placed between (1) and (2). The intermediate segment is tripartite, and consists of Dobie's [illegible], forming a node in its equator, and Flogel's element N [illegible] on each side of it. During contraction, the [illegible] consists in the shortening of the interval between [illegible] ends of Bowman's elements. This stage of the contraction probably results from the absorption of fluid by [illegible]. In the next stage Bowman's elements [illegible] real contraction of their tissue, [illegible] to their ends, which become swollen.

[illegible] gave the results of their researches [illegible] of the posterior roots of the spinal nerves from the [illegible] to the second dorsal. [illegible]

[illegible] read a paper on [illegible]. The author [illegible]

[illegible] showed an apparatus for the [illegible]

[illegible] read a paper on the

fibre in fever. In fever the red blood-cells are diminished; tepid baths but not antipyretics bring the number again to the normal. In artificial fever (rabbits raised to a temperature of 50° C.) the red blood-cells are also greatly diminished in the systemic veins, but are increased in the liver.

Dr. Gley (Paris) discussed the action of the intra-vascular injection of solutions of peptones in rendering the blood incoagulable. He showed that these bodies did not possess this action after the ligature of the lymphatics coming from the liver. From this experiment the author inferred that the substance which renders the blood incoagulable, and is produced in the organism under the influence of the peptones, is secreted by the hepatic cells.

Dr. I. V. Uxhill (Heidelberg) showed an apparatus for the rapid mechanical stimulation of the nerve of a muscle-nerve preparation.

Dr. Schenk (Würzburg) read a paper on the innervation of the iris. The author's communication chiefly concerned the observation of Dogel, that upon stimulation of the cervical sympathetic in cats, dogs and rabbits, in addition to the dilatation of the pupil on the same side, a contraction of the pupil on the other side occurred. Schenk regarded this phenomenon as a consensual pupil reflex (consensuelle Pupilla Reflex). In Dogel's experiments the eye on the same side as the stimulated sympathetic was exposed to the light; hence during stimulation more light entered the pupil, and this caused contraction of the pupil of the opposite side. When the eye on the stimulated side was shaded from the light, the contraction of the other pupil did not take place. This explanation would not hold for rabbits, as in them the consensual pupil reflex is absent; the author was, however, in the case of rabbits, unable to repeat Dogel's results.

Dr. Leathes (London) read a paper on the osmotic changes between the blood and the tissues. The author gave the result of his experiments on the influence of strong solutions of cane-sugar and dextrose, and of iso-, hypo-, and hyper-tonic solutions of NaCl in causing the passage of fluid from the blood into the tissues, or *vice versâ*. The author further discussed the osmotic pressure of the lymph in the thoracic duct, which he found $\frac{1}{100}$ to $\frac{1}{50}$ higher than that of the blood.

Friday Morning, September 13.—Presidents, Profs. Rosenthal and Langley. Prof. Wedensky (St. Petersburg) read a paper on the exciting and inhibitory action of electric tetanisation on the nerve-muscle apparatus. The author showed that, if induced currents of great frequency and intensity be applied to the sciatic nerve, the gastrocnemius contracts strongly but soon relaxes; if at this time the intensity of the exciting currents be diminished until they become moderate, a very strong (optimum) contraction of the muscle takes place. Further, if when the muscle is in a condition of relaxation produced by the application of strong and frequent induced currents to the nerve, one diminishes the frequency, a strong tetanic contraction can be reproduced, which at once disappears upon again increasing the frequency. There is for each stage of tetanisation an optimum of frequency. The relaxation of a muscle, under the application of strong and frequent induced currents to its nerve, is caused by the nerve-endings entering into a condition of inhibition. This can be demonstrated by applying to the muscle moderately strong induced currents; no effect is produced until the strong and frequent tetanisation of the nerve is discontinued.

Dr. Luscher (Bern) read a paper on the nervous mechanism of swallowing. The author's experiments were made upon rabbits. He showed that the three branches of the recurrent laryngeal overlapped in their distribution to the œsophagus, *i.e.* each branch supplied some of the region supplied by the other. After division of the recurrent laryngeal stimulation of the central end of the superior laryngeal did not produce swallowing. Upon stimulation of the central end of the divided recurrent laryngeal (the nerve of the other side being intact) a faint act of swallowing was produced. Stimulation of the central end of the vagus only gave rise to swallowing when the recurrent laryngeal was intact.

Prof. Bowditch (Boston) showed an apparatus to demonstrate the mechanism of the ankle-joint. By the introduction of a spring balance into the cord representing the gastrocnemius muscle, and the application of a weight, he could determine the relation between power and weight for the action of this muscle.

Dr. Waller (London) read a paper on the photo-electric [illegible] of the retina.

Prof. Hensen (Kiel) gave a demonstration on an acoustic apparatus, the result of which was to show that the view of Helmholtz, that the vowels owe their special quality to overtones produced in the mouth and adjoining cavities, requires modification: this, in the author's opinion, is impossible.

Friday Afternoon.—Presidents, Profs. Richet and Cybulsky. Dr. Sherrington (London) gave a demonstration on eye movements.

Dr. Lanz (Bern) read a paper on the effect of removal of the thyroid, and of thyroid-feeding in normal animals. Among many interesting results, the author found that thyroidectomised hens either lost their power of laying eggs, or laid very small and ill-formed ones. On the other hand, hens fed with thyroids (30 grms. per diem) had their egg-laying power greatly increased. In some animals the author found that the administration of large quantities of thyroid gland caused an arrest of growth.

Dr. Phisalix (Paris) showed that the blood of the salamander rendered animals immune to curare. This immunity in the case of the frog and pigeon lasts several days.

Prof. Mosso (Turin) read a paper on the effect of rarefied air upon man and apes. The author's researches on man were made on Mount Rosa, at a height of 5000 metres. The author showed that at this altitude the respiratory exchange is diminished; his observations were made under conditions of absolute rest, mostly during sleep. In the explanation of these phenomena the author thinks more attention should be paid to the diminution of CO_2. He describes them under the name of Akapnia (καπνος = smoke). Mosso further described an experiment which he made upon a monkey. He subjected this animal to an atmosphere of pure O at a low pressure; he observed under these conditions the phenomena of mountain-sickness (*Bergkrankheit*) even when the pressure of the O exceeded the partial pressure of this gas in the atmosphere under ordinary circumstances. The author concluded that the two main factors which come into play at high altitudes are (1) the diminution of CO_2 in arterial blood; (2) the physical effect of low pressure on the nervous system.

F. W. Tunnicliffe.

CORRESPONDING SOCIETIES OF THE BRITISH ASSOCIATION.

The first meeting of the Conference took place on Thursday, September 12, the second on Tuesday, September 17, at the Co-operative Hall, at 3.30 p.m.

At the first meeting, the Corresponding Societies Committee was represented by Mr. G. J. Symons (Chairman), Prof. R. Meldola, Mr. J. Hopkinson and Mr. T. V. Holmes (Secretary). The Chairman opened the proceedings with an address.

On the conclusion of the address, Mr. T. V. Holmes made a few remarks with regard to the list of papers read before the various Corresponding Societies, and appended to the Report of the Corresponding Societies Committee. He hoped that the Secretaries of the Corresponding Societies, in preparing their lists, would be careful to group papers, which from their titles might belong to either of two Sections, with that to which they had most affinity. It was also most desirable that the names of papers sent in should not turn out to be mere popular lectures, but should contain something original. It had sometimes happened that on wishing to refer to some paper on the list sent in by some Society, in order to ascertain its true character, it could not be found on their shelves at Burlington House. In future no paper could be placed on the list published by the British Association unless it was on their book-shelves.

Captain Elwes (Dorset) laid upon the table a paper on the rainfall in the county of Dorset, which had been compiled by a member of the Dorset Natural History and Antiquarian Field Club, Mr. Eaton. It was a most careful piece of work, and was illustrated by maps and diagrams.

Mr. Hopkinson said that about twenty years ago he began to note the rainfall of Hertfordshire with about twenty observers. Last year the record he published contained the monthly returns from forty observers. He trusted that delegates would preserve any early meteorological records they might find.

Mr. De Rance remarked that the increasing usefulness of local societies was shown by the fact that two British Association Committees had ceased to exist, that on coast erosion, and that on the circulation of underground waters, on account of the admirable way in which their work had been taken up by the local societies.

His Honour Deemster Gill said that the subject of coast erosion had been taken up by a Committee of the Legislature of the Isle of Man, but their investigations were not yet complete. They had found that for some twenty miles on the west, the north-west and the north, there had been a destruction of land of about twenty acres to the mile within the last fifty or sixty years. The meteorology of the Isle of Man was also being well looked after.

Mr. Sowerbutts asked whether it was desirable that the Manchester Geographical Society should collect the results of observations at their local observatories, and forward them to the Meteorological Society; and the Chairman replied in the affirmative.

Capt. Elwes hoped that local societies might be induced to co-operate for the discovery of flint implements, and the formulation of the results attained.

Mr. Osmund W. Jeffs, Secretary to the British Association Committee for the collection and Preservation of geological Photographs, said that the photographs collected would be placed in the Museum of Practical Geology, Jermyn Street, London. The first part of the collection, 800 photographs, had already been placed there. It was proposed to go on collecting, as many parts of the British Isles were quite unrepresented.

Mr. De Rance thought that it would be a good thing if each society would issue a circular and send it to other local societies, so that it might be known what photographs had been taken in each locality.

Mr. J. B. Murdoch (Glasgow) thought that in too many of their investigations Scotland was excluded. He mentioned, as an instance, the British Association Committee for recording the position, &c., of erratic blocks of England, Wales and Ireland.

Some discussion arose on this point, in which Mr. De Rance, Mr. Sowerbutts and Mr. G. P. Hughes took part. Then the Chairman said that he believed Scotland had been omitted in that instance because the Royal Society of Edinburgh had been working at the subject before the formation of the British Association Committee.

Mr. Murdoch replied that it was true that a Boulder Committee had existed in Scotland, but its director, Mr. Milne Holme, was dead, and had been unable to get about the country for some time before his death. The eight yearly reports issued by his Committee were very valuable, but for some time the work had been practically at a standstill.

The Chairman remarked that in that case it was most desirable that Scotland should be included by the Erratic Blocks Committee.

Deemster Gill said that the boulders of the Isle of Man were being noted by the Isle of Man Natural History and Antiquarian Society.

Prof. Meldola moved, and Mr. Hopkinson seconded, a motion in favour of an application to the General Committee for a grant of £30 to enable the Corresponding Societies Committee to carry on its work. This was carried, and the meeting ended.

At the second meeting, on Tuesday, September 17, the Corresponding Societies Committee was represented by Dr. Garson (in the chair), Mr. Hopkinson, Mr. Symons, and Mr. T. V. Holmes (Secretary).

The Chairman said that it was usual at their second meeting to consider the recommendations from the various Sections respecting work in which it was thought the Corresponding Societies might usefully co-operate.

Section A.

Mr. White Wallis, representing Section A, said that the Committees for investigating earth tremors and seismological phenomena in Japan had been merged into one, with the title of "Committee for Seismological Observations." The Committee for the application of photography to meteorology had been reappointed, and so had the Underground Temperature Committee. The Meteorological Photographs Committee was simply desirous to obtain photographs of lightning, rainbows, halos, &c.

The Rev. J. O. Bevan inquired whether the meteorological work formerly carried on at Stonyhurst by Father Perry was still going on. Mr. Sowerbutts answered that it was, and Mr. White Wallis said that he would note the suggestion that they should communicate with Stonyhurst. He added, in answer to questions, that instruments for noting earth tremors were unaffected by vibrations from passing waggons, trains, &c.

Section C.

Mr. A. S. Reid, representing Section C, stated that Mr. Osmund Jeffs had consented to retain the post of secretary to the Geological Photographs Committee for another year, as Mr. W. W. Watts had agreed to act as co-secretary during that time, and afterwards to become sole secretary. The Erratic Blocks Committee had altered its title so as to include Scotland.

Mr. Murdoch hoped that the Earth Tremors Committee might include Scotland in its sphere of action. It was then a purely English Committee.

Mr. M. B. Slater thought that an exchange of local geological photographs among the various Corresponding Societies would be a good thing. Some discussion then took place on the practical difficulties likely to arise from an interchange, such as the burden likely to be laid upon the shoulders of the amateur photographer, &c. Mr. Hopkinson thought that copies should be obtainable at the Jermyn-street Museum at a small fixed charge, and Mr. Reid mentioned a plan suggested by Mr. Gray of Belfast. At that town a photographer had been appointed who received the negatives taken by various members of the local societies, and furnished as many copies as were required at a small fixed charge.

Section E.

Mr. Sowerbutts said that the Committee of Section E had asked the Council of the British Association to permit them to have a Committee for the purpose of making an inquiry into the condition of the teaching of geography in Great Britain in all schools, especially secondary schools, and to report next year. It was probable that the Corresponding Societies might be asked to furnish certain information, and he hoped their secretaries would reply as promptly as possible.

The Rev. J. O. Bevan thought that the statements made in the report of the Conference of Delegates at Nottingham, that in some county, unnamed, "children attending schools were not taught geography in any way," and that geography was absolutely ignored in secondary schools, were decidedly erroneous, though in some primary schools it was not taught except in connection with reading. The Royal Geographical Society had instituted examinations in geography in secondary schools, and gave gold medals and other prizes.

Section H.

Mr. Hartland said that he was there owing to the very sad bereavement sustained recently by Mr. Brabrook, the Chairman of the Ethnographical Survey Committee, who was consequently unable to attend. The Ethnographical Survey was a matter in which the Corresponding Societies were especially capable of rendering valuable assistance. They had hitherto, however, met with but little response from the local societies. The work had so many branches that some of them could scarcely fail to interest their more active members. If the Committee obtained the grant for which they asked, they proposed to begin work in Galway, and he hoped to report progress at the next meeting. He would be glad if meanwhile the Corresponding Societies would circulate their schedules, and bring the Survey under the notice of their members.

Mr. M. B. Slater mentioned the work done in the neighbourhood of Malton by a sub-committee, of which Dr. Colby was chairman; and Mr. Hartland remarked that the Malton Naturalists' Society was one of those which had responded to their circular.

The Chairman noted the great variety of the work of the Ethnographical Survey, which included questions of physical characteristics, folk-lore, linguistic differences, place-names, traditions, &c. Satisfactory work had been done around Ipswich.

Mr. Hartland wished also to mention the preservation of ancient monuments. He had just received a letter from the Secretary of a local committee in Pembrokeshire, mentioning the recent discovery there of some ancient stones and some pit dwellings.

Mr. Hopkinson thought that the measurements asked for were very elaborate, and the questions were considered inquisitorial. He was sure that a simpler system would be found to answer better in practice, as then more societies or persons would be found willing to undertake the work.

Mr. Hartland hoped that members who objected to the [illegible] would take up the subjects of dialect, folk-lore, or prehistoric monuments. Though they hoped to be able to secure the elaborate measurements in some cases, they were glad to get such measurements as could be procured. They did not consider their standard as of universal obligation.

The Chairman wished to say a few words about another Committee, that concerned with the measurement of school children. Many schools had been doing good work in this way, but unfortunately there had been no uniform system, so that the results obtained at one school could not be compared with those at another. The Committee had drawn up a system which he hoped would prove acceptable to the various schools.

Dr. Brett (Hertfordshire) said that since the York meeting of the British Association, fifteen years ago, it had been his custom as a medical man to record the height, weight, colour of hair and eyes, &c., of children. He had up to that time made about three thousand observations, but had not yet been able to put his records into shape.

The Rev. J. O. Bevan spoke of the desirability of expediting the archæological survey of the kingdom, which had been begun a few years ago. He was then at work at the map of Herefordshire, which was nearly ready for publication. He was surprised that the work had not been taken up more energetically by properly qualified persons in the different districts.

THE AFFILIATED SOCIETIES OF THE AMERICAN ASSOCIATION.

A FEATURE of the meetings of the American Association for the Advancement of Science is the number of affiliated societies which meet at nearly the same time and place, though having no organic connection with it. One disadvantage of this is that the Sections of the Association do not get many of the important papers read before the affiliated societies; in fact, these societies seem almost to take the place of the Sections, and they certainly tend to put the Association in a secondary position. As a large number of the papers were more of local than of general interest, we confine ourselves to a brief statement of the societies which met at Springfield during the recent meeting of the American Association, and of a few of the subjects considered.

The Society for the Promotion of Agricultural Science discussed several papers on spraying as a prevention of the attacks of various insect pests and fungi, and also on cereal culture in the United States. At the end of the proceedings, Mr. R. Lazenby was elected President of the Society.

The attention of the Association of Economic Entomologists was largely directed to the results of experimenting with insecticides, and the methods of placing the knowledge before all agriculturists. A resolution was passed asking the Massachusetts authorities to support the work of the Gipsy Moth Commission. Another resolution was adopted asking that the publication of "Insect Life" by the Department of Agriculture be resumed. The officers for the ensuing year are: President, Prof. C. H. Fernald; first vice-president, Prof. F. M. Webster; second, Prof. Herbert O. Ames; secretary, C. L. Marlatt.

The session of the American Forestry Association was a very successful one. In the course of a short communication, Baron Herman pointed out that Germany has comparatively the most forests in well-regulated administration of all the countries of the world; that is, one-fourth of its whole area is covered with them (all under long and careful management). There is scarcely one tree in the whole of the fatherland which is not known personally to a forest officer, and which has not been sown or planted with more or less great care and labour. The whole area of wooded land is almost equally divided between State, community, and private persons. And it is thought that this is a very good state of affairs, the commonwealth being in that way well interested in its parts as well as in the whole, in the affairs connected with the forest growth. This of course influences legislation, and although laws concerning the forests are not passed in the Reichstag, but in the Parliaments of the individual States, there is scarcely a part of Germany where one is allowed to cut down a forest, and not plant it again, without the permission of the Department of Forestry. The forests are managed by hundreds of forest officers, and these are educated at special colleges for forestry, there being no less than eleven in Germany. The theoretical study at these colleges lasts generally four years, not counting the time a young man has to spend in learning practical work in the woods. This comparatively long time a man wants, for his training shows how very much the science of forestry has been developed in

its different branches in Germany. After a man has passed his examinations he may often have to wait for years and years before he gets an appointment; but the love of the woods, the poetry which time has woven around the solitary *forsthaus* amidst the trees and animals of the woods is so great they do not mind waiting a long time. In conclusion, Baron Herman said he was in America to see what trees could be transplanted with success to Germany.

After a paper on the present condition of the forests of America, the following resolutions were adopted, among others:—

"That the American Forestry Association join with the New York Chamber of Commerce and Board of Trade in hearty advocacy of the establishment of a forestry commission of three members to make a thorough investigation of the public forest lands, and to make recommendations concerning their disposition and treatment, and the executive committee is hereby directed to represent the Association in support of such legislation."

"That the American Forestry Association recognising that a practical advance in rational forestry methods requires the services of men trained in forestry practice, endorse the legislation proposed in the last congress, and expresses the hope that the same will be enacted during the coming congress."

"That the knowledge and extent and conditions of our forest resources is a necessary basis for intelligent forest legislation, and that therefore the American Forestry Association recommends the co-operation of various government departments as far as practicable in ascertaining their areas and conditions, and especially recommends that both a topographical and forestal survey of the national forest reservations be instituted."

Sixteen papers were read before the American Mathematical Society, and two topics were discussed, viz. (1) a general subject catalogue or index of mathematical literature, and (2) the mathematical curricula of colleges and science schools. With reference to the former subject, it was resolved that the Council of the Society consider the desirability of offering their co-operation to the Mathematical Society of France in the work of classifying and indexing mathematical literature.

The American Chemical Society was presided over by Prof. E. F. Smith; and among the subjects of papers read before it were: an electrical process for the production of white lead; the heating effects of coal; speed of oxidation of chloric acid; reaction between copper and concentrated sulphuric acid; use of aluminium for condensers in the distillation of alcohol, ether, chloroform, benzine and similar liquids. Prof. Norton, who read the last-named paper, stated that the equipment of the chemical laboratory of the University of Cincinnati includes aluminium supports, rings, clamps, burners, water-baths, air-baths, hot water funnels, &c., in all of which connections the lightness, conductivity, and freedom from rust render the metal superior to iron or bronze.

The Botanical Society of America, which was organised in Brooklyn last year, held its first annual meeting on August 27 and 28. Mr. William Trelease presided. The officers elected for the ensuing year are: President, C. E. Bessey; vice-president, W. P. Wilson; secretary, Charles R. Bainer; treasurer, Arthur Hollick.

Prof. G. F. Swain opened the proceedings of the Society for the Promotion of Engineering Education with an address on the relation between mental training and practical work in engineering education. The papers read before the Society, and the discussions to which they gave rise, will do much to indicate what should be the scope of engineering and technical schools, and the places of different subjects in an engineering education. The units of force best adapted for use in the teaching of mathematics formed the subject of a discussion between the physicists and engineers. At the end of the meeting, Mr. Mansfield Merriman was elected President.

ON RECENTLY DISCOVERED REMAINS OF THE ABORIGINAL INHABITANTS OF JAMAICA.[1]

THE circumstances under which the human remains now exhibited to the meeting were discovered, are narrated in a communication by Mr. F. Cundall, Secretary to the Jamaica Institute, published in the *Journal* of the Institute for April 1895, and also in a letter by Mr J. E. Duerden, Curator of the Museum, in NATURE of June 20. From the former I extract the following description of the discovery:—"On the 10th April, a labourer, whilst cutting stakes on the Halberstadt Estate (a wild, rocky part of the Port Royal Mountains, about 2000 feet above the sea-level, and two miles from the shore) on the estate of Mr. B. S. Gossett, a quarter of a mile east of the Kalorama Mission Station, discovered on the hillside a human bone. This led the Rev. W. W. Rumsey to make a search on the following day, when he discovered a small aperture 25 inches wide, and less than 2 feet high, in the face of the limestone rock, and blocked by boulders; on removing these, and passing through which, he discovered a cavern with water-worn sides, partially covered with stalactite deposits, penetrating into the rock for a distance of about 20 feet, about 5 feet across at its widest part, and not more than 2 or 3 feet high. The floor was covered with a deposit about 12 inches thick, of a fine light yellowish dust, but the remains were superficial."

In addition to the human bones, to be presently described, were found a considerable portion of a cedar-wood canoe, about 7 feet long, fragments of pottery, including two, nearly perfect, earthenware vessels similar to those known to have been made by the Arawak Indians, an outer portion of the trunk of an *arbor-vitæ*, probably serving at one time as a "mortar," scarcely showing any sign of decay; the perfect skulls and other parts of the skeleton of a rodent (the so-called Jamaica coney, *Capromys brachyurus*); two large marine shells (*Fusus* and *Murex*), the soft parts of which are still eaten by the natives, numerous land shells (*Helix*, &c.). A flint implement is also mentioned in Mr. Duerden's account.

The only portion of the contents of the cavern submitted to me for examination consist of the human bones, and as they only arrived in London a few days before I was leaving town, at present I have only been able to make a general examination of them, without any detailed measurements.

Their principal interest consists in the circumstance, proved both by the conditions under which they were found, and by their own characteristics, that they are the remains of the race which inhabited the island previous to its discovery by the Spaniards, by whom they were in so short a time barbarously and utterly exterminated.

Whatever condition the bones were found in as they lay in the cave, they are now completely mixed up, and it is impossible to put together anything like complete skeletons, or even, except in very few cases, to associate the bones of individuals; and the number of odd bones and fragments show that large portions of the individuals who were buried or died in the cave are now missing. Their general condition of preservation, colour, &c., is nearly the same in all, so there is no reason to suppose that they were not contemporaneous. None of the bones show any wounds or marks of violence, but all appear to be those of persons who have died a natural or slow death. Both sexes and almost all ages are represented, from children of four or five years to very old persons, the proportion of the latter, as will be seen, being remarkable.

Of the crania, there are six complete, all those of fully adult or aged persons, and two calvariæ (without the facial portion), both of children. There are also fragments of six others, giving evidence of fourteen individuals.

Of the adult skulls three appear to be masculine and three feminine in type.

Five of these show evidence of artificial depression of the frontal region in various degrees. In two it is very marked; in the others less so. In the sixth, though the frontal region is low, no effects of artificial deformation are evident. Both the children's skulls are very broad and flat, but whether naturally so, or whether this character has been exaggerated artificially it is difficult to say. The mode of depression, when it occurs, is similar in all, evidently produced by the flat board upon the forehead the commonest custom throughout so large a portion of the ancient inhabitants of the American continent.

Although there is a considerable general resemblance between these skulls, they present strong individual characters; but their whole aspect, taken together, is characteristic of the American type. The retreating forehead, well marked supraciliary ridges, round broad arch of the palate, round high orbits, narrow nasal aperture, and especially the narrow prominent nasal bones, causing a high bridge to the nose during life, are very characteristic. There are, however, two rather remarkable exceptions to this form of nose, in which the breadth of the aperture and flatness of the

[1] Read before Section H of the British Association at Ipswich, September 12, by Sir William H. Flower, K.C.B., F.R.S.

[illegible] almost equal those of the negro; the nasal index being as high, respectively, as 542 and 563. These are both feminine-looking heads, and one of them is the most and the other the least deformed of the set. Whether this form of nose is met with in any other undoubtedly aboriginal American crania, is subject for investigation. Apart from these, the skulls are remarkably like the majority of those which I have seen of Peruvians, Mexicans, and the ancient mound-builders of the United States.

Of lower jaws there are in all twenty-two, a number which indicates that many of the crania must now be missing from the collection. They are interesting as showing age, and peculiarities of dentition; nineteen are adults, and three young. The youngest has the milk teeth only—the first permanent molar, and first incisors being just about to appear (about six years old). One is a little older, the first molar being fully in place with the two milk molars. Another has all the permanent teeth in place, except the last molars (wisdom teeth), which are still in their alveoli.

In all the others the permanent teeth appear to have been fully in place, but the number of losses sustained during life is remarkable. As so many of the teeth have dropped out since death, it is mainly by the condition of the alveoli that their presence or absence during life can be judged of, for in only two or three do all appear to have been retained. Two are absolutely edentulous. In eight, not one of the true molars remain, the whole available dentition being represented by the incisors, and in a few cases by an isolated canine or premolar. Seven had lost one or more of the true molars. All the teeth, except those of the very young individuals, are much worn, but scarcely any show signs of disease or decay, there being only three small carious cavities among them all. Yet the milk molars in both the child's jaws, which were soon to be shed, have their crowns deeply excavated.

The only dental anomaly is that in one of the skulls the right upper wisdom tooth is placed horizontally, its crown projecting outwards through the surface of the maxillary bone its lower edge two millimetres above the alveolar border.

The limb bones indicate an average height rather below the middle size, but, as just stated, I have not yet had time to make accurate measurements and calculations.

Clavicle, 7 right, 10 left, all adult. *Scapulæ*, all more or less broken; fragments of 15 right and 11 left adult, and 1 young. *Humeri*, right, 5 adult and 2 young; left, 10 adult, 1 young (not corresponding with either of those of the opposite side). *Radii*, right, 14 adult, 3 young; left, 17 adult, 1 young. *Ulnæ*, right, 14 adult, 2 young; left, 10 adult, 1 young. *Pelvic bones*, mostly very fragmentary, but showing evidence of at least 9 adult males, 5 adult females, and several children. *Femora*, as with the other long bones, there are very few pairs, thus showing that there were more individuals than the actual number of bones would indicate: right, 11 adult and 2 young, 1 nearly full grown, but without epiphyses, 1 younger; left, 17 adult and 6 young of various ages, from quite small children upwards. None of these six have corresponding bones of the opposite side, so there is evidence from the femora of at least 23 individuals. *Tibiæ*, 18 right and 19 left, all adult. *Fibulæ*, 12 right and 11 left adult, and 3 young.

One of the largest of the femora has the head greatly enlarged and deformed by chronic rheumatic arthritis. The lower articular surface was mostly broken away, but the portion that remained appeared healthy.

One of the left tibiæ shows throughout the shaft marked evidence of chronic periostitis, the surface being thickened and vascular. A bone of the opposite side, which might have been of the same individual, shows the same condition in a less marked degree.

These are the only pathological conditions observed in any of the bones.

The question that naturally occurs after the examination of these remains is, How did they get into the cave? The [illegible] of the bones, and of the objects which were found with them [illegible] to their belonging to the native Indian inhabitants, and [illegible] the races which have been introduced into the island during the last four hundred years. A cave of such small [illegible] could not stand upright, could scarcely [illegible] the regular habitation of such a large number of persons. [illegible] a place of sepulture, but from its inaccessible [illegible] more likely to have been a refuge to which [illegible] the tribe had fled for safety, and in a vain endeavour to escape the horrible massacres by which we know the great bulk of the native population perished, had met a scarcely less miserable fate. Other similar discoveries, which will doubtless be made in the future, may throw light upon this question, and it is satisfactory to know that the authorities of the Jamaica Institute are now alive to the importance of carefully examining and preserving all such evidence as may still remain of the ancient history of the island and its inhabitants. The communication was illustrated by sketches of the cave, made by Mrs. Frank Cundall.

ELECTRIFICATION AND DISELECTRIFICATION OF AIR AND OTHER GASES.[1]

§ 1. EXPERIMENTS were made for the purpose of finding an approximation to the amount of electrification communicated to air by one or more electrified needle points. The apparatus consisted of a metallic can 48 cms. high and 21 cms. in diameter, supported by paraffin blocks, and connected to one pair of quadrants of a quadrant electrometer. It had a hole at the top to admit the electrifying wire, which was 5·31 metres long, hanging vertically within a metallic guard tube. This guard tube was always metallically connected to the other pair of quadrants of the electrometer and to its case, and to a metallic screen surrounding it. This prevented any external influences from sensibly affecting the electrometer, such as the working of the electric machine which stood on a shelf 5 metres above it.

§ 2. The experiment is conducted as follows: One terminal of an electric machine is connected with the guard tube, and the

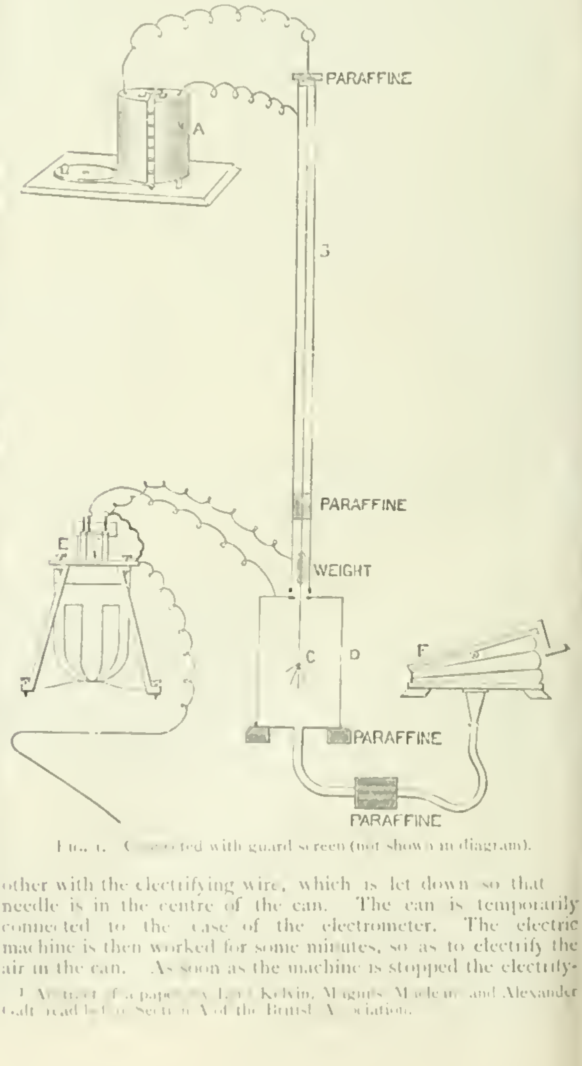


FIG. 1. Connected with guard screen (not shown in diagram).

other with the electrifying wire, which is let down so that needle is in the centre of the can. The can is temporarily connected to the case of the electrometer. The electric machine is then worked for some minutes, so as to electrify the air in the can. As soon as the machine is stopped the electrify-

[1] Abstract of a paper by Lord Kelvin, Magnus Maclean, and Alexander Galt, read before Section A of the British Association.

ing wire is lifted clear out of the can. The can and the quadrants in metallic connection with it are disconnected from the case of the electrometer, and the electrified air is very rapidly drawn away from the can by a blowpipe bellows arranged to suck. This releases the opposite kind of electricity from the inside of the can, and allows it to place itself in equilibrium on the outside of the can and on the insulated quadrants of the electrometer in metallic connection with it.

§ 3. We tried different lengths of time of electrification and different numbers of needles and tinsel, but we found that one needle and four minutes of electrification gave nearly maximum effect. The greatest deflection observed was 936 scale divisions. To find, from this reading, the electric density of the air in the can, we took a metallic disc, of 2 cms. radius, attached to a long varnished glass rod, and placed it at a distance of 1·45 cm. from another and larger metallic disc. This small air condenser was charged from the electric light conductors in the laboratory to a difference of potential amounting to 100 volts. The insulated disc thus charged was removed and laid upon the roof of the large insulated can. This addition to the metal in connection with it does not sensibly influence its electrostatic capacity. The deflection observed was 122 scale divisions. The capacity of the condenser is approximately $\frac{\pi \times 2^2}{4\pi \times 1·45} = \frac{1}{1·45}$. The quantity of electricity with which it was charged was $\frac{1}{1·45} \times \frac{100}{300} = \frac{1}{4·35}$ electrostatic unit. Hence the quantity to give 936 scale divisions was $\frac{1}{4·35} \times \frac{936}{122} = 1·7637$.

The bellows was worked vigorously for two and a half minutes, and in that time all the electrified air would be exhausted. The capacity of the can was 16,632 cubic centimetres, which gives, for the quantity of electricity per cubic centimetre, $\frac{1·7637}{16,632} = 1·06 \times 10^{-4}$. The electrification of the air in this case was positive: it was about as great as the greatest we got, whether positive or negative, in common air when we electrified it by discharge from needle points. This is about four times the electric density which we roughly estimated as about the greatest given to the air in the inside of a large metal vat, electrified by a needle point and then left to itself, and tested by the potential of a water-dropper with its nozzle in the centre of the vat, in experiments made two years ago and described in a communication to the Royal Society in May, 1894.[1]

§ 4. In subsequent experiments, electrifying common air in a large gas-holder over water by an insulated gas flame burning within it with a wire in the interior of the flame kept electrified by an electric machine to about 6000 volts, whether positively or negatively, we found as much as $1·5 \times 10^{-4}$ for the electric density of the air. Electrifying carbonic acid in the same gas-holder, *whether positively or negatively*, by needle points, we obtained an electric density of $2·2 \times 10^{-4}$.

§ 5. We found about the same electric density ($2·2 \times 10^{-4}$) of *negative* electricity in carbonic acid gas drawn from an iron cylinder lying horizontally, and allowed to pass by a **U**-tube into the gas-holder without bubbling through the water. This electrification was due probably not to carbonic acid gas rushing through the stopcock of the cylinder, but to bubbling from the liquid carbonic acid in its interior, or to the formation of carbonic acid snow in the passages and its subsequent evaporation. When carbonic acid gas was drawn slowly from the liquid carbonic acid in the iron cylinder placed upright, and allowed to pass, without bubbling, through the **U**-tube into the gas-holder over water, no electrification was found in the gas unless electricity was communicated to it from needle points.

§ 6. The electrifications of air and carbonic acid described in §§ 4 and 5 were tested, and their electric densities measured by drawing by an air pump a measured quantity of the gas[2] from the gas-holder through an india-rubber tube to a receiver of known efficiency and of known capacity in connection with the electrometer. We have not yet measured how much electricity was lost in the passage through the india-rubber tube. It was not probably nothing; and the electric density of the gas before leaving the gas-holder was no doubt greater, though perhaps not much greater, than what it had when it reached the electric receiver.

§ 7. The efficiency of the electric receivers used was approximately determined by putting two of them in series, with a paraffin tunnel between them, and measuring by means of two quadrant electrometers the quantity of electricity which each took from a measured quantity of air drawn through them. By performing this experiment several times, with the order of the two receivers alternately reversed, we had data for calculating the proportion of the electricity taken by each receiver from the air entering it, on the assumption that the proportion taken by each receiver was the same in each case. This assumption was approximately justified by the results.

§ 8. Thus we found for the efficiencies of two different receivers respectively 0·77 and 0·31 with air electrified positively or negatively by needle points; and 0·82 and 0·42 with carbonic acid gas electrified negatively by being drawn from an iron cylinder placed on its side. Each of these receivers consisted of block tin pipe, 4 cms. long and 1 cm. diameter, with five plugs of cotton wool kept in position by six discs of fine wire gauze. The great difference in their efficiency was no doubt due to the quantities of cotton wool being different, or differently compressed in the two.

§ 9. We have commenced, and we hope to continue, an investigation of the efficiency of electric receivers of various kinds, such as block tin, brass, and platinum tubes from 2 to 4 cms. long, and from 1 mm. to 1 cm. internal diameter, all of smooth bore and without any cotton wool or wire gauze filters in them; also a polished metal solid, insulated within a paraffin tunnel. This investigation, made with various quantities of air drawn through per second, has already given us some interesting and surprising results, which we hope to describe after we have learned more by farther experimenting.

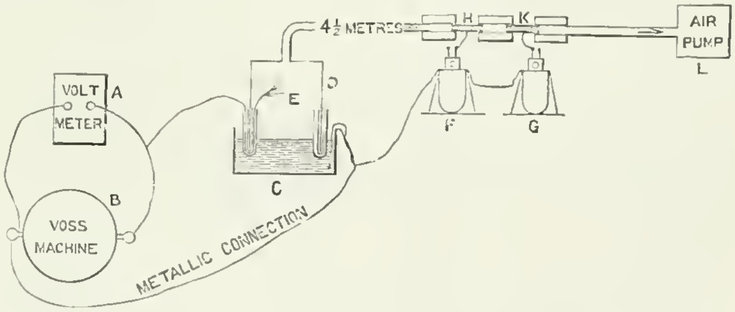


FIG. 2.

§ 10. In addition to our experiments on electric filters we have made many other experiments to find other means for the diselectrification of air. It might be supposed that drawing air in bubbles through water should be very effective for this purpose, but we find that this is far from being the case. We had previously found that non-electrified air drawn in bubbles through pure water becomes negatively electrified, and through salt water positively. We now find that positively electrified air drawn through pure water, and negatively electrified air through salt water, has its electrification diminished but not annulled, if the primitive electrification is sufficiently strong. Negatively electrified air drawn in bubbles through pure water, and positively electrified air drawn through salt water, has its electrification augmented.

§ 11. To test the effects of heat we drew air through combustion tubes of German glass about 180 cms. long, and 2½ or 1½ cms. bore, the heat being applied externally to about 120 cms. of the length. We found that, when the temperature was raised to nearly a dull red heat, air, whether positively or negatively electrified, lost little or nothing of its electrification by being drawn through the tube. When the temperature was raised to a dull red heat, and to a bright red, high enough to soften the

[1] "On the Electrification of Air," by Lord Kelvin and Magnus Maclean.

[2] The gas-holder was 58 cms. high and 81 cms. in circumference. Ten strokes of the pump raised the water inside to a height of 8·1 cms., so that the volume of air drawn through the receivers in the experiments was 42[illegible] cubic centimetres per stroke of the pump. This agrees with the measured effective volume of the two cylinders of the pump.

glass. Losses up to as much as four-fifths of the whole electrification were sometimes observed, but never complete diselectrification. The results, however, were very irregular. Non-electrified air never became sensibly electrified by being drawn through the hot glass tubes in our experiments, but it gained strong positive electrification when pieces of copper foil, and negative electrification when pieces of carbon, were placed in the tube, and when the temperature was sufficient to powerfully oxidise the copper or to burn away the charcoal.

§ 12. Through the kindness of Mr. E. Matthey, we have been able to experiment with a platinum tube 1 metre long and 1 milimetre bore. It was heated either by a gas flame or an electric current. When the tube was cold, and non-electrified air drawn through it, we found no signs of electrification by our receiver and electrometer. But when the tube was made red or white hot, either by gas burners applied externally or by an electric current through the metal of the tube, the previously non-electrified air drawn through it was found to be electrified strongly positive. To get complete command of the temperature we passed a measured electric current through 20 centimetres of the platinum tube. On increasing the current till the tube began to be at a scarcely visible dull red heat, we found but little electrification of the air. When the tube was a little warmer, so as to be quite visibly red hot, large electrification became manifest. Thus 60 strokes of the air-pump gave 45 scale divisions on the electrometer when the tube was dull red, and 395 scale divisions (7 volts) when it was a bright red (produced by a current of 30 amperes). With stronger currents, raising the tube to white-hot temperature, the electrification seemed to be considerably less.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Oxford.—There are few changes of importance in the lists of lectures issued by the Board of Faculty of Natural Science for Academical year.

Prof. Gotch has come into permanent residence, and has appointed Dr. Gustav Mann, of Edinburgh University, to be Demonstrator in Physiology, in place of Dr. Pembrey, who has been appointed Lecturer in Physiology at the Charing Cross Hospital.

The new pathological laboratory in the Department of Regius Professor of Medicine is approaching completion, and Dr. J. Ritchie will give a course of practical Pathological Bacteriology for the Regius Professor. The present pathological laboratory is on a modest scale, and it is hoped that before long the University will be in a position to afford a building and equipment more worthy of the growing needs of the medical school at Oxford.

The examination for the Burdett-Coutts' Scholarship is to begin on October 21. There are this year two scholarships to be awarded, as none was awarded last year.

Mr. Frederic Lucien Golla, of Tonbridge School, has been elected to a Demyship in Natural Science at Magdalen College.

Four scholarships are announced for election at Wadham College on December 1, 1895, and in addition the Warden and Fellows have power to give exhibitions of £30 to £40 a year. No papers in Natural Science will be set, but in the case of one of the exhibitions preference will be given to any candidate who shall undertake to read for honours in Natural Science, and to proceed to a degree in Medicine in the University of Oxford.

Cambridge.—The election to the vacant professorship of Botany will take place on Saturday, November 2, at 2.30 p.m. Candidates are to send their names and testimonials to the Vice-Chancellor, Sidney Sussex Lodge, by October 26. The electors are Dr. Vines, Mr. Sedgwick, Dr. Allbutt, Dr. D. Oliver, Dr. Phear, Mr. F. Darwin, Sir J. D. Hooker, and Prof. Foster.

The election of a head of a college to be an elector to the Sadleirian Professorship of Pure Mathematics will take place on Tuesday, October 22, at 1 p.m. The vacancy is caused by the resignation of Dr. Phear, late Master of Emmanuel. The electors are those persons whose names are on the electoral roll of the University. Dr. Ferrers, of Caius, and Dr. Taylor, of St John's, are the present "heads" on the board of electors to the professorship.

Mr. C. T. R. Wilson, of Sidney Sussex College, has been appointed assistant demonstrator of experimental physics in the place of Mr. Capstick, resigned.

The Clerk Maxwell Scholarship in Physics is vacant by the resignation of Mr. Whetham. Candidates are to apply to Prof. Thompson, at the Cavendish Laboratory, by November 1. The scholarship is worth about £180 a year, and is tenable for three years. Candidates must be members of the University who have worked for a term or more at the Cavendish Laboratory.

Among the Fellows of Trinity College elected on October 10, are Mr. C. P. Sanger, bracketed second wrangler 1893; the Hon. W. Russell, bracketed eighth wrangler 1893; and Mr. I. L. Tuckett, first class Parts I. and II. Natural Sciences Tripos, and Coutts Trotter student in physics and physiology. Mr. Sanger and Mr. Russell were also placed in the first class of Part II. of the Moral Sciences Tripos 1894.

The London University Guide for the year 1895-96 has just been published by the University Correspondence College Press.

Dr. Dunn, head master of the Plymouth Technical Schools, has been appointed principal of the Northern Polytechnic Institute, Holloway Road.

Mr. Henry Louis has been elected Professor of Mining at the Durham College of Science, Newcastle-upon-Tyne, by a Joint Committee nominated by the College and the Coal Trades Associations of Durham and Northumberland.

The October *Record* of Technical and Secondary Education contains an illustrated article on the Yorkshire College, Leeds; and also a comparative summary of recent progress in technical education in various counties. This latter article continues and concludes a review of the work done by the Technical Education Committees of the English counties, commenced in the April number of the *Record*.

The entrance scholarships at the London Hospital Medical School have been awarded as follows:—Price scholarship in science, £120, Mr. H. Balean; Science scholarships, £60 and £35, Mr. O. Eichholz and Mr. A. B. Soltau; Price scholarship in anatomy and physiology, for university students, £60, Mr. R. C. Wall and Mr. J. H. Evans.

The following awards have been made at St. Bartholomew's Hospital:—Scholarship of £75 in biology and physiology, to Mr. C. S. Myers; scholarship of £75 in chemistry and physics, to Mr. J. S. Williamson; scholarship of £150 in biology, chemistry, and physics, to Messrs. R. C. Bowden and R. H. Paramore; preliminary scientific exhibition of £50 in biology, chemistry, and physics, to Mr. J. C. M. Bailey.

At St. Mary's Hospital Medical School the two university scholarships, of the value of £52 10*s*. each, have been awarded to Mr. R. Wade and Mr. G. S. Keeling; the first natural science scholarship, value £105, has been awarded to Mr. W. H. Willcox, and the three value £52 10*s*. each to Mr. H. Lovell-Keays, Mr. E. W. Holyoak, and Mr. A. F. Hayden.

At St. George's Hospital Medical School, science entrance scholarships of £85 have been awarded to Mr. Herbert Stringfellow Pendlebury, to Mr. Henry Goodridge Deller, and to Mr. John Howell Evans.

The following recent appointments are announced:—Prof. W. A. Setchell to the chair of botany in the University of California; Prof. H. Talbot to be associate professor of chemistry in the Massachusetts Institute of Technology; Dr. O. Jaekel, Privat-docent in geology in Berlin University, to be Extraordinary Professor; Dr. P. Lenard to the chair of physics in the Technische Hochschule at Aacheen.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xvii. No. 4 (Baltimore, October).—On the deformation of thin elastic wires, by A. B. Basset. In the author's previous paper (vol. xvi.) on the deformation of thin elastic plates and shells, whilst commending the novelty, power and elegance of the *geometrical* investigations employed in Mr. Love's treatise on elasticity, he impugned the treatment of the *physical* portion of the subject. It is on the same ground of defective treatment that Mr. Basset considers that a further exposition on the theory of wires is needed, and this is what is furnished in the present paper. A useful table of contents precedes the text. Investigations in the lunar theory, by Prof. E. W. Brown, is a memoir to which reference has already been made in our columns (No. 1352, p. 533). The closing paper is by Otto Staude, "Ueber den Sinn der Windung in den Singularen Puncten einer Raumcurve."

Bulletin de l'Académie Royale de Belgique, No. 6.—The conditions under which hydrogen peroxide is decomposed, by W. Spring. The catalysis of hydrogen peroxide takes place without chemical action by contact with various substances when the formation of water is favoured. Any substance which is more easily impregnated with water than with H_2O_2 brings about the decomposition of the latter. A solution of H_2O_2 containing salts is the seat of a decomposition whose activity increases with the temperature.—Chemical study of eight earths of the Lower Congo, by E. Stuyvaert. The analysis of earths from Boma, Zenze, Banza-Kasi, Mayombe, and Vungu-Mumba proves that the soils of the Lower Congo, sandy as well as calcareous, are provided with reserves of phosphoric acid and potash which insure a high fertility. It is certain that in the territories where the disappearance of forests has not modified the rainfall, as in Mayombe, the cultivation of coffee, cocoa, and other economic plants can be carried on for a long time without the use of manure.—On the critical temperatures of solution and their application to general analysis, by L. Crismer. The critical temperatures of solution may be used for the identification of chemical bodies without the necessity of weighing them, and they form a valuable additional criterion for the purpose of qualitative analysis. The critical temperature of solution is independent of the amount of either body present. It varies very much from one substance to another, but is constant for the same substance. For a mixture of two bodies, it is sensibly equal to the arithmetical mean of those of the constituents taken singly. Just as the surface tension of a liquid is reduced to zero at the critical temperature of vaporisation, so the surface tension of the lower liquid tends towards zero at the critical temperature of solution, and the meniscus separating them becomes a plane. An optical method of determining these critical temperatures may be based upon this fact.

Wiedemann's Annalen der Physik und Chemie, No. 9.—Double refraction of electromagnetic rays, by Peter Lebedew. The author succeeded, by a modification of Hertz's apparatus, in dealing with waves not more than 0·6 cm. long, and in demonstrating the phenomena of polarisation, reflection, and refraction with apparatus of the size ordinarily used in optics. The resonator used was a small thermo couple of iron and "constantane." An ebonite prism 1·8 cm. long showed refraction to within 3° of arc. Rhombic sulphur showed measurable double refraction, and a "Nicoll prism" was successfully constructed of two sulphur prisms with a plate of ebonite in place of the Canada balsam.—Luminescence of organic substances in the three states, by E. Wiedemann and G. C. Schmidt. Many organic vapours show true fluorescence, and some, like naphthalene, give composition spectra under the electric discharge, without being dissociated. Kathode luminescence is shown by many organic liquids, and the colour corresponds to that of the vapour. But the luminescence of the solid bodies often differs from that in the liquid state. Solid anthracene shows green, gaseous anthracene blue luminescence.—A vibration galvanometer, by H. Rubens. This instrument somewhat resembles Wien's optical telephone, and is used for measuring the intensity of alternating currents. It consists of a soft iron armature attached to a stretched wire. This executes torsional vibrations which are timed to the period of the alternating current. The latter traverses four electromagnets ranged round the armature, and when the periods are identical the armature executes strong torsional vibrations whose amplitude is measured by the width of a slit as seen reflected in a mirror attached to the wire. This arrangement is much more sensitive than the electrodynamometer.—Theory of the broadening of spectrum lines, by B. Galitzin. The molecular theory is superior to those based upon Doppler's principle, upon Kirchhoff's law, or upon damping. It admits of a development based upon the electromagnetic theory, that of molecular resonators. The broadening is a consequence of the forced vibrations produced by the collision of molecules. The want of symmetry of the broadening, and the influence of temperature and pressure are immediate consequences of the molecular theory as developed by the author.

THE numbers of the *Journal of Botany* for August–October contain several articles of interest to descriptive botanists. Mr. E. G. Baker concludes his revision of the African species of *Eriosema*, and Mr. A. B. Rendle his description of Mr. Scott Elliot's tropical African orchids, including a large number of new species; Mr. D. Prain continues his account of the genus *Argemone*; Mr. E. A. L. Ballers contributes a list of Marine Algæ new to Britain; and Mr. Arthur Bennett some notes on British Characeæ. There are biographical notices of the late Profs. W. C. Williamson and C. C. Babington, with a portrait of the latter.

Boll. della Soc. Sismol. Ital., vol. i., 1895, No. 5.—Some observations made on Vesuvius on June 21, 1895, by M. Baratta.—Vesuvian notes (January–June 1895), by G. Mercalli.—Hydrothermal observations at Fiumecaldo from January to April 1895, by C. Guzzanti.—Notices of Italian earthquakes, April 1895. A valuable record of the observations of the first after-shocks of the Laibach earthquake of April 14 from a large number of Italian stations.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, October 2.—Prof. Raphael Meldola, F.R.S., President, in the chair. Mr. McLachlan exhibited, on behalf of Mr. Bradley, of Birmingham, the specimens of Diptera attacked by a fungus of the genus *Empusa*, of which an account had recently appeared in the *Entomologist's Monthly Magazine*. Mr. H. Tunaley exhibited specimens of *Lobophora viretata* from the neighbourhood of Birmingham. Specimens of the green dark form were shown in their natural positions on the bark, and specimens of the yellow form were shown on leaves on which they rested.—Mr. J. W. Tutt exhibited cases formed by a lepidopterous insect received from the Argentine Republic, which he said he recognised as being either identical with, or closely allied to, *Thyridopteryx ephemeræformis*, which did great damage to many orchard and forest trees in North America. Mr. Tutt also exhibited a series of *Lycæna ægon* captured by Mr. Massey, of Didsbury, on the mosses in Westmoreland. The males were remarkable in bearing two very distinct shades of colour. The females also differed considerably from the form occurring in the South of England. He also exhibited a long series of *Hydrœcia lucens*, captured in the mosses near Warrington, and for comparison a series of *Hydrœcia paludis*, and he read notes on the various specimens exhibited.—Dr. Fritz-Müller communicated a paper entitled "Contributions towards the history of a new form of larvæ of Psychodidæ (Diptera), from Brazil."—Baron Osten-Sacken communicated a paper, supplemental to the preceding one, entitled "Remarks on the homologies and differences between the first stages of Pericoma and those of the new Brazilian species."—The Rev. A. E. Eaton also contributed some supplementary notes to Dr. Fritz-Müller's paper.—Lord Walsingham, F.R.S., read a paper entitled "New Species of North American Tortricidæ." In this paper twenty-nine species were dealt with, of which twenty-six were described as new, from Florida, California, N. Carolina, Arizona, and Colorado. The paper also included certain corrections made by the author in the nomenclature of genera.

PARIS.

Academy of Sciences, October 7.—M. Janssen in the chair. On an ascension to the summit of Mont Blanc, and on the work carried out during the summer of 1895 on the "massif" of this mountain, by M. J. Janssen. The ascent is described, together with an account of the cloud phenomena observed during a day in the higher regions. Passing on to describe the 0·33 m. telescope about to be erected at the observatory, it is remarked that the parts, now all assembled at the summit, will be mounted as a polar siderostat. A 0·0 m. mirror is to be mounted with the telescope. The observer will control all movements from a chamber of observation, which will be heated as may be required. As the instrument could not be taken down and remounted, it was bodily moved on to a new base formed of strong plates frozen on to the ice, and its pendulum then beat as regularly as at Paris. Observations with a Duboscq two-prism spectroscope in this very dry atmosphere failed to show any rays of aqueous origin in the solar light. The observatory has suffered a slight downward settling towards Chamounix; this took place in 1893 and 1894, and the movement is now insignificant. (See Our Astronomical Column.)—Study of some meteorites, by M. Henri Moissan. Iron from Kendal county in Texas contained amorphous carbon, but neither graphite nor diamond. Iron from Newstead (Roxburghshire) yielded amorphous carbon and graphite, but not diamond. Déesite, found in 1866 in the Sierra Déesa in Chili, contained a form of graphite only. Caillite, iron from Toluca-Xiquipilso,

Meteorite of 1784 contained no variety of carbon. Iron from Novy-Urei, Krasnoslobodsk, Penza, Russia (fall of August 23, 1886), yielded black diamond only. A further sample of meteoric iron from Cañon Diablo gave transparent diamond. All three varieties of carbon have been found in this meteorite.—On hyperglycæmia and glycosuria following ablation of the pancreas, by M. R. Lepine.—On the integration of Hamilton's differential equations, by M. Paul Staeckel. Concerning the results shown in the paper, the author remarks: "There is the true generalisation of Liouville's theorem, which allows the utilisation of all progress in the integration of Hamilton's equations to find new types of integrable equations, that is, to form new linear elements of which the geodesic lines can be determined."—On parasitic electrodes, by M. G. Delvalez.—On the mechanical properties of alloys of copper and zinc, by M. Georges Charpy. The tensile strength increases with the percentage of zinc, attains a maximum at 43 per cent., and then decreases rapidly: the elongation before rupture also increases with the zinc, passes through a maximum at 30 per cent., and then rapidly diminishes.—On a carbide of glucinum, by M. P. Lebeau. Pure crystallised glucinum carbide has been prepared at the high temperature of the electric furnace. The properties of this carbide, more particularly its reaction with water resulting in its decomposition in the cold with the production of methane, resemble those of aluminium carbide C_3Al_4, hence support is given to the formula C_3Be_4. The atomic weight of glucinum must be near 14, and glucina becomes Be_2O_3.—Researches on the combinations of mercury cyanide with iodides, by M. Raoul Varet. A thermochemical paper dealing with iodocyanides. Iodocyanides in solution yield the isopurpurate reaction on addition of potassium picrate at 30° C. and turn red-litmus paper blue. These salts must then be of the type $HgCy_2.MCy.HgI_2$, and not like the chlorocyanides $MCl_2.2HgCy_2$. The transformation of the system $2HgCy_2 + MI_2$ into $HgCy_2 + MCy_2 + HgI_2$ absorbs on the average -0.3 Cal. in solution, a quantity surpassed by the heat of formation of $HgCy_2.MCy_2$, $+12.4$ Cal., with that of its union with yellow HgI_2 giving $+2.3$ Cal.

On the double decompositions of mercury cyanide and salts of alkaline and alkaline earthy metals, by M. Raoul Varet.—Action of air on grape must and on wine, by M. V. Martinand.—Deep dredgings made on the Caudan coast in the Gulf of Gascony during August 1895, by M. R. Koehler. Much material, which has not yet been thoroughly examined, was obtained from (*a*) depths of 300 to 600 metres, illustrating the change from littoral to profound faunas; (*b*) coralligenous depths on the abrupt cliff running parallel to the French coast; (*c*) the bottom of the deeper part of the Bay of Biscay.—On the effects of the winter of 1894–5 on the fauna of the coast, by M. Jourdain.—M. Resel communicated an extract from a memoir to the Minister of War on the storm at Besançon on July 1.

NEW SOUTH WALES.

Linnean Society, August 28.—Mr. Cecil W. Darley in the chair.—On the homology of the palatine process of the mammalian premaxillary, by R. Broom.—Botanical notes from the Technological Museum, Sydney, No. iv., by J. H. Maiden and R. T. Baker.—The Silurian Trilobites of New South Wales, with references to those of other parts of Australia. Part iii. *Phacopidæ*, by R. Etheridge and John Mitchell. This important family is represented in the Silurian rocks of Australia by five species of *Phacops*, and one of *Homalonotus*; of these four are [illegible]. The [illegible] forms are at present un[illegible].

DIARY OF SOCIETIES.

LONDON.

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NO. 1355, VOL. 52

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Guide Zoologique (Helder, De Boer, jun.).—Rural Water Supply: A. Greenwell and W. T. Curry (Lockwood).—Dog Stories: edited by J. St. Loe Strachey (Unwin).—Mesures Electriques: Prof. E. Gerard (Paris, Gauthier-Villars).—Index Kewensis, Part 4 (Oxford, Clarendon Press).—Metallurgy, an Elementary Text-Book: E. L. Rhead (Longmans).—Die Mechanische Bedeutung der Schienbeinform: Dr. H. H. Hirsch (Berlin, Springer).—Polarisation et Saccharimétrie: D. Sidersky (Paris, Gauthier-Villars).—The Beginnings of Writing: Dr. W. J. Hoffman (Macmillan).—London University Guide and University Correspondence College Calendar, 1895–96 (Clive).—Cours Élementaire de Manipulations de Physique: A. Witz, deux edition (Paris, Gauthier-Villars).

PAMPHLETS.—The Case against Butchers' Meat: C. W. Forward (Insurance Publication Depôt).—Neuere Forschungen über das Gebiss der Sänger: Dr. R. Dewoletsky (Czernowitz).—The Elephants: Prof. R. J. Anderson (Belfast, Mayne).—Die Oberflachen-oder Schiller-Farben: Dr. B. Walter (Braunschweig, Vieweg).

SERIALS.—Journal of the Chemical Society, October (Gurney).—Proceedings of the Physical Society, October (Taylor).—Record of Technical and Secondary Education, October (Macmillan).—Journal of the Franklin Institute, October (Philadelphia).—American Journal of Science, October (New Haven).—Journal of the Royal Statistical Society, September (Stanford).—Proceedings of the Royal Society of Edinburgh, Vol. xx. pp. 305–480 (Edinburgh).—Engineering Magazine, October (New York).—Zeitschrift für Physikalische Chemie, xviii. Bd. 1 Heft (Leipzig, Engelmann).—Himmel und Erde, October (Berlin, Paetel).—Strand Magazine, October (Newnes).—Strand Musical Magazine, October (Newnes).

CONTENTS.

THURSDAY, OCTOBER 24, 1895.

THE METALLURGY OF IRON.

The Metallurgy of Iron and Steel. By Thomas Turner, Associate of the Royal School of Mines. Vol. i. "The Metallurgy of Iron." (London: Charles Griffin and Co., Limited, 1895.)

THIS is the third volume of a valuable series of treatises on metallurgy, written by Associates of the Royal School of Mines, under the able editorship of Prof. Roberts-Austen. It occupies an intermediate position between a text-book and an exhaustive treatise, and is intended not only for the use of the student, but also of persons who are connected with the manufacture of iron and steel, and who, therefore, may be assumed to have already some knowledge of the subjects discussed.

The attempt made by the author to compress within the space of 367 pages a useful account of this vast subject—the metallurgy of iron—has been satisfactorily accomplished; and although in some of the chapters the condensation is perhaps unduly great, yet this fault is minimised by the numerous references, which abound in the text, to original papers where full details may be found. In preparing these references, the author appears to have made a painstaking research into the literature of the entire subject, and this, together with his practical knowledge of its chief branches, has resulted in the production of a valuable treatise, which covers the whole field of the metallurgy of iron more completely than any other book in our language. As a standard of reference for detailed information, the *Journal* of the Iron and Steel Institute has been wisely chosen, as in it all advances in the metallurgy of the metal are recorded, and the more important are dealt with by specialists of note: it is, besides, easily accessible.

The volume begins with a patiently compiled summary of the history of iron, in which the origin and development of the metallurgical processes for the production and purification of the metal, and of the furnaces and appliances used, are clearly traced from the earliest times up to the present day.

A condensed *résumé* of the nature, composition and characteristics of the chief iron ores, and of the modes of preparing them for smelting, follows in chapters iv. and v. In a future edition the latter chapter might be extended with advantage, for, although no important methods are omitted, the descriptions of some are very brief.

The next five chapters (vi., vii., viii., ix. and x.) deal respectively with the blast furnace, the blast, the reactions which occur in smelting, the fuels used, and slags and fluxes. The general arrangement of a blast furnace plant is illustrated by sketch plans of a modern Cleveland and American (Edgar Thomson) works, and under "Construction of the Blast Furnace" a typical furnace of each of these works is selected for detailed description. The marked differences which are found in the internal lines and dimensions of the furnaces of the two countries, and in their practical working, are compared, and the reasons which have been advanced in favour of each are clearly stated and discussed; all of which tend to demonstrate that there can be no universal standard form, size, or method of working for a blast furnace. There are, however, undoubtedly some points in American practice which might be adopted with advantage in this country.

The diagram given on p. 127, illustrating the application of the recording pyrometer, as devised by Prof. Roberts-Austen, for the measurement of the temperature of the hot blast, is instructive, and shows conclusively the value of this instrument to the blast furnace manager.

The reactions which take place in the blast furnace, and the conditions which regulate the consumption of fuel, are very fully considered. Here the editor has allowed the author to state his own view of the theory of reduction, probably because it is evidently a "theory." It differs from that which Prof. Roberts-Austen is known to teach in his lectures at the Royal School of Mines. In chapters xi. and xii. the "Properties of Cast Iron" and "Foundry Practice" are discussed with a thorough knowledge of the subjects, both chapters being full of important matter. The effects of the presence of other elements, especially of silicon, on the physical characters of cast iron, are ably and comprehensively set forth, and experimental data of much value to the practical founder are given in demonstration of the relations which exist between the chemical composition of the metal and its fitness for special purposes. The necessity for a knowledge also of the relations between its hardness and strength is wisely insisted on, as, when these are fully grasped, the iron-founder requires only the information how to harden or soften his metal at will by the use of silicon or other agents, to produce castings in which the crushing, transverse and tensile strength, or othe characters, shall predominate as desired. These chapters deserve the careful study not only of the student, but also of the practical man, if he wishes to work intelligently, and so avoid the uncertain results which follow the "rule-of-thumb" methods still too often practised in our foundries. In no other text-book are the subjects of these chapters so lucidly and completely treated.

A description of the methods for the "Direct Production" of wrought iron—the subject of numerous modern patents, and of probably more failures—follows: and the three next chapters (xiv., xv. and xvi.) deal with the "Indirect Production" of the metal. Of these, the chapter devoted to "Puddling" is one of the best in the book. The account of the process and its various modifications it contains is worthy of high commendation. The concise descriptions and explanations which are given, many of which are based on the author's personal experience and investigations, and the useful practical suggestions which abound regarding the relative economy and extent of purification resulting from modifications in the method of conducting the process, cannot fail to be of great value to all iron-workers.

The corrosion of iron, a subject of not a little importance when we consider the disastrous results which may arise from the oxidation of a boiler-plate, a girder, a rivet, or a wire rope, is reserved for the last chapter of the book. The conditions under which this change occurs, the methods which are adopted for preventing or retarding it, and the experimental data on which these are founded, are carefully summarised here.

The book, however, is too good to be dismissed with commendation alone, and it would be unfair to its author and readers if we omitted to indicate one or two points in which its value may be increased in a future edition, which will doubtless be soon required. The illustrations are a weak feature of the book; several are unsatisfactory, being either rough in execution, wanting in detail, or too small in size, and a few can serve no useful purpose. We are sure the student would be grateful for the improvement of some, the omission of others, and the substitution for them of working drawings, not diagrams. We trust the author will bear this in mind in the preparation of his companion volume on steel.

The other faults are few and of a minor character. They are chiefly those of excessive condensation in the sections dealing with the blast furnace. These sections might be usefully expanded by the insertion of additional details respecting the actual erection of a furnace; also of an example of actual working similar to the excellent *résumé* given of the process of puddling.

The book, however, is an excellent one, thoroughly up to date, and a welcome addition to modern metallurgical literature. We can confidently recommend it to metallurgical students and all concerned with the manufacture and use of iron.

W. Gowland.

THE LIFE OF RENNELL.

Major James Rennell and the Rise of Modern English Geography. By Clements R. Markham, C.B., F.R.S. (The Century Science Series.) (London: Cassell and Co., 1895.)

"JAMES RENNELL was the greatest geographer that Great Britain has yet produced." This, the first sentence of the preface, is the text of the biography. The authority of the President of the Royal Geographical Society, himself the leading geographer of the day in this country, may be accepted as sufficient evidence of Rennell's pre-eminence. The name would perhaps not suggest itself to one who had a less thorough knowledge of the rise of modern English geography; for until the publication of this little volume, Rennell was without any more pretentious memorial than an obituary notice or a paragraph in a biographical dictionary. Mr. Markham writes with an enthusiastic singleness of aim; intent on illustrating his theme, he has perhaps on one or two occasions failed to criticise his own conclusions very severely before accepting them. Possibly he may unconsciously have applied the method *post hoc ergo propter hoc* in connecting all British progress in geography during the last fifty years with a name which cannot be found to be familiar even amongst professed geographers. Indeed we believe that this happily-timed biography will make Rennell's example more fruitful in results in the next few years than it has been during the sixty-five which have elapsed since the death of the great geographer.

The time is appropriate, for the recent meeting of the International Geographical Congress in London has brought into public notice the superiority of other nations in the organised study of geography as a branch of science definite and distinct from others, capable of being cultivated by research and of being applied to numberless practical purposes.

Mr. Markham repudiates the suggestion that Major Rennell was an "arm-chair geographer"; but we are not sure that this somewhat hackneyed term is necessarily one of reproach. Rennell was greatest as a student and a critic, and by the practical experience of his earlier life he fitted himself to speak *ex cathedra* on questions, where insight and judgment were required to interpret, even to the travellers themselves, the full meaning and importance of their journeys. A professor's chair would have been his true place.

The greatness of Major Rennell may best be understood by a glance at the mileposts of his life. He was born in 1742, at Chudleigh, in Devon, and at the age of fourteen he joined the Navy, where he saw some service and learned to survey. In 1760 he went out to India as a midshipman; but after three years' hard work, largely occupied in surveying in the Indian Ocean, he left the Navy, joined the East India Company's service, and received the command of a ship. As if by a stroke of magic he was nominated Surveyor-General of Bengal and gazetted an ensign in the Bengal Engineers in 1764, when only twenty-one years of age. In this new and congenial sphere he worked devotedly for thirteen years, personally surveying the most unhealthy part of India with such success that in 1779 he published the "Bengal Atlas" containing the first authentic maps of the province. He left India in 1777, and, settling in London, devoted himself to critical geographical studies. His first purely geographical work was a "Memoir to the Map of Hindostan," and the map itself. In 1781 he became a Fellow of the Royal Society, and subsequently he communicated two papers to the *Philosophical Transactions.* Although ignorant of the classical languages, he studied the works of the Greek geographers in translations, and so produced his famous "Geography of Herodotus" and "Comparative Geography of Western Asia." Then turning to the burning question of his time in geography, the penetration of Africa, he pieced together the information brought home by Ledyard, Hornemann, Mungo Park, and other explorers sent out by the African Association. Here the results of subsequent discovery did not always confirm the provisional conclusions he arrived at from a critical study of the data at his disposal, but his controversies as to the course of the Niger interest the world no more.

Mr. Markham considers that Rennell was "the founder of another branch of the science of geography, which has since been called oceanography"; yet we find in Dr. Murray's compendious history of oceanography in the summary of the scientific results of the *Challenger* Expedition, a much more ancient lineage for that branch of science, and in the record of its development Rennell's name is not even mentioned. He certainly succeeded in calling attention to the importance of ocean currents, and made many shrewd observations as to their origin, preparing the way for the wider generalisations of Maury. He strongly held the theory that ocean currents are primarily due to the prevailing winds; and it is interesting to notice that the particular current issuing from the Bay of Biscay, to which his own name is attached, should only last year have been shown by Hautreux to have no permanent place, but to vary in force and direction with the changes of the wind.

It would be impossible to notice the numerous memoirs by which Major Rennell impressed the learned world of his time. With Sir Joseph Banks and other friends, he formed a sort of social circle for travellers and scientific men, which led to the formation of the Raleigh Club in 1827, and may be said to have formed the nucleus of the Royal Geographical Society established three years later.

Rennell's training was purely a practical one in the hard work which gave him a mastery of the technicalities of surveying and map-construction. Knowing the actual forms of sea and land at first hand, able himself to delineate them with exceptional skill, he could not make the mistakes which beset the merely theoretical student. This is still the one way to become a practical geographer, only in the present day a working knowledge of geology must be added to proficiency in the arts of observation and measurement. On such a foundation, so gained, theoretical instruction may profitably be superimposed. Mere lectures on theoretical geography, isolated lessons in the use of instruments, do not suffice to make a man a geographer, any more than lectures on theoretical chemistry and a few repetitions of the routine of simple analysis will make a man a chemist. If British geographers are to catch up and keep pace with those of the continent, they must receive systematic training in their student days, and take up geography as a serious study, as one takes up any other science. For, alas, the good old days are gone, and there is no Warren Hastings on the threshold of the twentieth century to confer pensions of £600 at the age of thirty-five on the would be Rennells of to-day! As geological students have to follow other methods than those of Murchison, so present-day geographers cannot take Rennell too literally as their model; and Mr. Markham plainly states that he looks to the labours of the University lecturers in geography to maintain the succession of British geographers. If this is to take place, there must be fresh organisation and encouragement of pure geographical research on the part of the Universities. Much progress is improbable as long as the antithesis between "geography" and "science" is a possible figure of speech. It is not so in Germany.

HUGH ROBERT MILL.

COUNTER-IRRITATION.

The Theory and Practice of Counter-Irritation. By H. Cameron Gillies, M.D. (London: Macmillan and Co., 1895.)

DR. GILLIES has selected a subject rich in literature but poor in experiment, and has treated it entirely from the literary as opposed to the experimental side. The first part of the book is devoted to a *résumé* of the literature of counter-irritation, and inflammation, which Dr. Gillies rightly considers he must not only quote, but criticise. Some of his criticisms we do not understand, some are entirely superfluous, Dr. Gillies taking up much space in demolishing theories which in the present day nobody could possibly believe in, some—and two of these we shall consider—show a want of scientific understanding.

On page 73, our attention is drawn to a paper by Dr. Hollis, published in the St. Bartholomew's Hospital Reports for 1874. Dr. Hollis showed that vesication could be produced in the Actiniæ by the local application of liquor ammoniæ. The importance of these researches consisted in the fact that they demonstrated that the living cell itself, using this term in its general sense, was capable of reacting to an irritant. It is to work done exactly on these lines by Metschnikoff[1] that we owe the modern theory of Phagocytosis. The physiology, the pharmacology, and the chemistry of the cell are presumably to Dr. Gillies, as "provoking" as he admits Dr. Hollis' monograph to be. The second class of experiments performed by Dr Hollis demonstrated that local reaction to irritants took place in the excised tail of a newt, thus showing that this local reaction was independent of the general circulation. Dr. Gillies objects to "all such experiments, not only upon moral and humane grounds, but on the ground also that we have not been able to make sure that any good has come by them." "The tail is either dead or living, if living the result only shows that it is a living result; if dead we are not as physicians concerned with the chemistry of the action."

On page 78, our author considers an article by Dr. Lauder Brunton in the St. Bartholomew's (not the St. George's) Hospital Reports for 1875. Dr. Gillies differs from the author upon two points. First, he (Dr. Gillies) denies that inflammation can occur independently of congestion. One would have thought that this had been settled by Hollis. The discrepancy is explained when one finds, after a page's reading, what Dr. Gillies means by congestion—"an acceleration of the processes of nutrition." When arguing with a physiologist it is as well to adopt the usual physiological terminology. The second point of difference is Brunton's dictum that "pain in an inflamed part is probably due to distension of the vessels and pressure on the nerves." "The characteristic pains of neuralgia so called," says Dr. Gillies "are not easily if at all referable to the pressure from active congestion." Is a nerve which is the seat of neuralgia an inflamed part?

Dr. Gillies evidently believes that "he alone destroys who rebuilds," so we are not left merely amongst the ruins of other theories, but are provided with a "new" one. "Whatever good comes by the use of counter-irritants is because, by their irritant effects, they stimulate the activity of the tissues of the part to which they are applied and accelerate the blood supply thereto, so increasing nutrition or repair, as the need may be." This is the only new theory which we have been able to extract from chapter vii. What about the remote effects of counter-irritants? If Dr. Gillies is convinced that whether directly or remotely counter-irritants act beneficially only when they directly, or reflexly, increase the blood supply, that is at least a coherent theory; we think it quite probable that irritation of a given skin area by a blister or otherwise can give rise to reflex dilatation of the corresponding vascular area. Bradford[2] actually observed dilatation of the vessels of the kidney upon stimulating the central ends of the posterior roots of the so-called renal area, whereas stimulation of the central end of an intercostal nerve always caused contraction. Dilatation of the vessels of the splanchnic

1 "Leçons sur le Pathologie comparée de l'inflammation."
2 *Journal of Physiology*, vol. x. 4 4.

area has been observed upon stimulation of the central end of the sciatic nerve during chloral[1] and pyridin[2] poisoning, showing the influence exerted by the condition of the centre at the time of peripheral stimulation.

Of the second, the so-called "practical" part of the book, we have little to say. From what we have read, we regard Dr. Gillies' practice as no sounder than his theories. The reprint with which he provides us of Dr. Davies' original communication on blistering in acute rheumatism, and the controversy thereon, is the most interesting part of the book. We should like to know who it is who believes that the "serum" is "*abundantly* charged with lactic acid" in acute rheumatism; and, supposing it was, how much one is likely to get from the serum, say, of half a dozen blisters? (p. 88.) To sum up our remarks, we do not consider the book of value either to physicians or physiologists. The facts it contains are not new, and the theories do not justify their existence, since they fail to fulfil the conditions which should be demanded of all hypotheses, viz. to indicate lines of research which shall offer a reasonable hope of increasing our knowledge. One merit which it possesses, is that it may draw attention to some valuable pieces of work which might perhaps otherwise have been disregarded.

F. W. T.

A NEW DEPARTURE IN GEOMETRY.

Die Grundgebilde der ebenen Geometrie. By Dr. V. Eberhard, Professor at the University of Konigsberg i. P. Bd. I. 8vo. xlviii. + 302 pp. Five plates. (Leipzig: Teubner, 1895.)

THE history of Analytical Geometry affords a curious subject of study to the thoughtful mathematician. It would seem that equations between coordinates were first used to express spatial relations discovered by intuitional processes, and the equations were combined algebraically to discover other implied spatial relations. For this purpose it was necessary to interpret in geometrical terms equations arrived at by algebraic processes from geometrical data, and the facility thus acquired led men to seek for similar interpretations of equations set down without reference to geometrical conditions. Hence it happens that modern developments of Analytical Geometry appear rather to present algebraic facts in geometrical language than to deduce results that can be apprehended by intuition from data of intuition. Such a notion as that of a cubic surface, for instance, would seem to be essentially analytical, and although it has been proved possible to arrange a geometrical construction for an algebraic curve whose equation is given, yet the construction arrived at is so artificial that intuition fails to grasp by its aid the necessary form of the curve. Looking at the subject in this way, it seems hardly too much to say that the algebra which was designed to be the servant of the geometer has become his master.

Some such reflections as these form the starting-point of Dr. Eberhard's work. The volume under notice is to be the first of a series, and in his long preface[3] he sets forth his aim and method. Here, after tracing the origin in experience of simple geometrical notions such as those of the straight line and the plane, he divides curves and surfaces into two classes, the regular (gesetzmässig) and the fortuitous (zufällig), and proceeds to inquire after intuitional criteria available for distinguishing between them. He defines a regular locus as one in which a relation that can be apprehended by intuition connects a variable point of the locus with a finite number of points fixed in it. The kind of relation which he admits as capable of being apprehended by intuition is essentially topographical. This will be elucidated by considering the example he gives. Let a system of points be taken, and let planes be drawn through them three by three. These planes will in general intersect in other points besides those of the original system. Let planes be now drawn through the points of the extended system three by three. These planes will again intersect in some new points, and the process can be continued. Let the process be arrested at any stage, and suppose a set of four points of the extended system lie in one plane. If one of the points of the original system were slightly displaced these four points would generally cease to lie in one plane, but if the particular point of the original system were displaced on a certain surface, the four points would remain in a plane. This property constitutes a definition of the surface available for intuitional geometry. It will be seen from the example that the method rests upon the topographical relations of systems of points.

The description of these relations for a given system can be carried out systematically, and the process consists in the use of two related notions. The first is the notion of "characteristics," and the second is the notion of the "index" of a point in a plane system. If three points out of four are taken in a definite order, the triangle formed by them is described in the positive or negative sense by an observer on the same side of their plane as the fourth point. The sense of description of the triangle formed by three points in a definite order for an observer on a definite side of their plane is the characteristic of the three. The index of a point in a plane system is the order in which a line turning about that point meets the other points of the system. A statement of the indices simplifies the problem of stating the characteristics.

The bulk of the present volume is taken up with theorems concerning the characteristics and index-systems of groups of points in a plane, and they are fully exemplified in the cases of groups of four, five, and six points. In an investigation of so novel a character we find, as we might expect, original methods of working and difficult arguments. The want of figures in illustration of the earlier chapters, and some of the notations employed, combine with the nature of the subject to render the book difficult to read.

The endeavour to make the geometry of curves and surfaces of high degrees more intuitive is laudable, a new classification of loci founded on geometric rather than algebraic principles is also a worthy object of research, and the idea of grounding such a classification in topographical circumstances is ingenious; but a final judgment as to Dr. Eberhard's success in these directions can only be pronounced after his complete work has been given to the world.

A. E. H. L.

[1] [illegible] 4th edition, p. [illegible]
[2] [illegible] and [illegible], *Journal of Physiology*, xvii. p. 272.
[3] The [illegible] pages of the preface are separately published as tract, with the title "Über die Grundlagen und Ziele der Raumlehre."

OUR BOOK SHELF.

Handbook of Grasses; treating of their Structure, Classification, Geographical Distribution, and Uses, also describing the British Species and their Habitats. By William Hutchinson. 8vo. Pp. 92, 40 woodcuts. (London: Swan Sonnenschein and Co. New York: Macmillan and Co., 1895.)

THIS is a cheap popular work, adapted for the use of elementary students. There is nothing that covers the same field in existence already, and it fulfils its purpose excellently well. It would have been better to have called it "An Introduction to the Study of the British Grasses," as it only deals in detail with the British species, which are not more than one-thirtieth of the total number of grasses that are known in the whole world. The short introduction explains how easily a collection of dried grasses can be made. The first chapter, called "Structure," gives all the different organs in detail, showing what is the general plan on which grasses are organised, and explaining the general and special terms which are used in describing the genera and species. In the second chapter, which is the longest in the book, the hundred and odd British species are classified according to their localities, and described in detail, most of the common kinds being illustrated by small woodcuts, with dissections. The third chapter is devoted to classification, in which Bentham and Hooker's "Genera Plantarum" is followed. The British genera are described in detail, and the characters of the thirteen tribes there adopted, several of which are not represented in Britain, are given. The rest of the book is occupied by a readable account of the geographical distribution of the grasses, especially of the cereals, and an account of their various uses for food, and in other ways. *Gramineæ* is one of the most universally distributed of all the natural orders of plants, and, in point of the number of species, is only exceeded by five other natural orders: *Compositæ*, *Leguminosæ*, *Orchideæ*, *Melastomaceæ*, and *Rubiaceæ*. Between three and four thousand species of grasses are known, and they are classified under three hundred genera. The little book is well written and trustworthy, and no doubt will secure a good circulation.

Rural Water Supply. By Allan Greenwell, A.M.I.C.E., and W. T. Curry, A.M.I.C.E. Pp. 210. (London: Crosby Lockwood and Son, 1895.)

IN this volume we have an elementary work on water engineering, containing a sufficient account of the principles and construction of waterworks to be of real use to engineers, and forming at the same time a good introduction to more elaborate treatises. The volume is based upon a series of articles which appeared in the *Builder* last year, and it contains valuable information upon all matters connected with water supply. It is, indeed, what its secondary title represents it to be, namely, "a practical handbook on the supply of water and construction of waterworks for small country districts." The book is full of details on points which are continually before waterworks engineers; and though these details are mostly rules and formulæ which have to be accepted without being understood, they will be of great assistance in planning schemes of water supply and in carrying out the works.

Climbing in the British Isles. II. Wales and Ireland. Wales. By W. P. Haskett Smith. *Ireland.* By H. C. Hart. Pp. 197. (London: Longmans, Green, and Co., 1895.)

CLIMBERS will find this little pocket-book an invaluable guide to instructive scrambles in Wales and Ireland; but the large number of fatal accidents recorded in its pages is hardly calculated to give other readers the mountaineering fever. On the first two pages of the book, three fatal falls and one severe accident are noted, and the tale of deaths is sustained throughout the book. To those who are filled with the desire to climb, this spice of danger only gives zest to the recreation; and the fact that several lives have been lost in attempts to scale a certain rock, is a sufficient reason for many Englishmen to tackle that rock and endeavour to scale it. In the book under notice, all the essential information about climbs in Wales and Ireland is given, with thirty-one illustrations (by Mr. Ellis Carr) and nine plans. By means of it, the would-be climber will be able to select his hills and peaks without difficulty, and with its assistance he may do in these islands hill-climbing which will form no mean part of a real mountaineering education. The book is primarily intended for those who climb for climbing's sake, hence little attention is paid to the geological interest of the rocks and hills described.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Feeding-Ground of the Herring.

IN his presidential address to Section D of the British Association at Ipswich, Prof. Herdman says:—

"Probably no group of animals in the sea is of so much importance from the point of view of food as the Copepoda. They form a great part of the food of whales, and of herrings and many other useful fish, both in the adult and in the larval state, as well as of innumerable other animals, large and small. Consequently, I have inquired somewhat carefully into their distribution in the sea, with the assistance of Prof. Brady, Mr. Scott, and Mr. Thompson. These experienced collectors all agree that Copepoda are most abundant, both as to species and individuals, close round the shore, amongst seaweeds, or in shallow water in the Laminarian zone over a weedy bottom. Individuals are sometimes extremely abundant on the surface of the sea amongst the plankton, or in shore pools near high water, where, amongst *Enteromorpha*, the Harpacticidæ swarm in immense profusion; but, for a gathering rich in individuals, species, and genera, the experienced collector goes to the shallow waters of the Laminarian zone. . . . In order to come to as correct a conclusion as possible on the matter, I have consulted several other naturalists in regard to the smaller groups of more or less free-swimming Crustacea, such as Copepoda and Ostracoda, which I thought might possibly be in considerable numbers over the mud. I have asked three well-known specialists on such Crustaceans—viz., Prof. G. S. Brady, F.R.S., Mr. Thomas Scott, F.L.S., and Mr. I. C. Thompson, F.L.S.—and they all agree in stating that, although interesting and peculiar, the Copepoda and Ostracoda from the deep mud are not abundant either in species or in individuals. In answer to the question which of the three regions, (1) the littoral zone, (2) from low water to 20 fathoms, and (3) from 20 fathoms onwards, is richest in small free-swimming, but bottom-haunting, Crustacea, they all replied the middle region from 0 to 20 fathoms, which is the Laminarian zone and the upper edge of the Coralline. . . . [Mr. T. Scott] tells me that as the result of his experience in Loch Fyne, where a great part of the loch is deep, the richest fauna is always where banks occur, coming up to about 20 fathoms, and having the bottom formed of sand, gravel, and shells. The fauna on and over such banks, which are in the Coralline zone, is much richer than on the deeper mud around them. On an ordinary shelving shore on the west coast of Scotland, Mr. Scott, who has had great experience in collecting, considers that the richest fauna is usually at about 20 fathoms."

It seems to me that these three specialists, or experienced collectors, have not given Prof. Herdman any information as to whether free-swimming Crustacea, such as Copepoda, are found in considerable numbers over the mud or not, as maintained by Dr. Murray in his concluding remarks in the Summary volumes of the *Challenger* Report, and I propose to answer the

question here. For ten years I have been engaged in dredging and trawling about the coasts of Scotland, chiefly as captain of Dr. Murray's yacht *Medusa*, and my experience does not coincide with that of Messrs. Brady, Scott, Thompson, and Herdman. For instance, in Loch Fyne I have always been able at any time of the year to collect in half an hour enormous numbers of *Euchæta*, *Calanus*, and *Nyctiphanes* over the mud in depths of about 70 fathoms or greater. Any person can see at the Millport Biological Station large bottles filled with these Crustaceans taken in a single haul. The stomachs of the herrings are frequently crammed with these Crustaceans, and the herrings certainly never got this food about the Laminarian zone, as suggested in Prof. Herdman's address.

Dr. David Robertson, who is one of the best-known collectors in the country, pointed out years ago that the Loch Fyne herrings got their food in the deep water, and attributed their fine quality to this fact. Dr. Robertson authorises me to say that, though there may be more species of Copepods in the Laminarian zone than in the deep water, still the number of individuals is very much greater in the deep water over the mud, as is conclusively proved by the *Medusa's* work.

Proper methods must, of course, be used, for I know of at least one instance in which a gentleman of considerable scientific repute was prepared to say that the free-swimming Crustaceans over the mud had completely left Loch Fyne; he communicated his opinion to Dr. Murray, with the result that the *Medusa* was ordered to Loch Fyne to investigate the matter. As was expected, the result was that these Crustaceans were found in as great profusion as on any previous occasion.

The result of my experience in Loch Fyne is that the nearer the nets are dragged to the mud in the deep water the greater will be the number of *Euchæta*, *Calanus*, and *Nyctiphanes* captured. I have taken, hundreds of times, in 70 fathoms, in a single haul lasting from twenty minutes to half an hour, more Copepods than can be collected in the Laminarian zone in eight or ten days. I have also captured herrings by means of drift nets sunk to the bottom in depths of 70 and 80 fathoms, and their stomachs were filled with Crustaceans of the same species as we captured by the nets just over the mud at these depths.

As to the deep mud in Loch Fyne not producing a rich fauna, I may state that in the deepest water the trawl could not be kept down for a longer time than about half an hour; otherwise the deck engine and all other appliances on board would have failed to bring the net to the surface through sheer weight, chiefly of organisms. There was generally a certain percentage of mud present, but the bag of the net was generally crammed with thousands of *Actinia*, which live there, along with *Pecten septemradiatus*, *Hippolyte*, *Pandalus*, *Crangon*, *Ascidians*, and many other invertebrates and fishes.

I have dredged Loch Fyne systematically for months, and examined its fauna from the littoral zone to the greatest depth; the specimens collected are now beside me, and all the journals with the notes are in Dr. Murray's possession. But I think enough has been said to show that the greatest abundance of Copepods is not to be found in the Laminarian or other shallow zones, but in the deep water over the mud; also that the deep mud does possess a very rich fauna. I speak only of the abundance of the above-mentioned organisms, with which I am well acquainted. I am not a specialist nor a scientific man, but I have had a great deal to do with the practical part of the investigations which have assisted Dr. Murray in drawing his conclusions.

ALEXANDER TURBYNE.

Millport, Cumbrae, N.B., October 5.

The Toronto Meeting of the British Association.

AN effort will be made to have the meeting of the American Association for the Advancement of Science held at San Francisco in 1897, so that the members of the British Association may cross the continent, and join us there, either before or after their own meeting at Toronto, which many of us hope to attend.

A suggestion of great importance, and deserving immediate consideration, seems to me that the Australasian Association should try to arrange a meeting for the same year on the Pacific coast of America, so that we may all join in the meeting of the American Association at San Francisco. This will be the first meeting of any of these Associations on that coast, and hence a momentous occasion.

I do not know how to reach the officers of the Australasian Association; but think that an insertion of this letter in NATURE will find them. I have already sent a letter at a venture to the President by his official title, as I do not know his name, in care of the Post-master of Melbourne, to be forwarded; but perhaps the Post-master may not know where to send it.

I have also written to Mayor Sutro of San Francisco, calling his attention to it.

WM. H. HALE.

Brooklyn, October 9.

The Theory of Magnetic Action upon Light.

IN the British Association Reports for 1893, Mr. Larmor has attempted to show that a satisfactory theory of magnetic action upon light can be constructed by means of a modification of Maxwell's theory which was proposed by Prof. Fitzgerald in 1879; and he alleges, with special emphasis (see p. 340), that his theory furnishes "a consistent scheme of equations of reflection and refraction, without the necessity of condoning any dynamical difficulties in the process." And on p. 350, after raising objections against a theory originally suggested by Prof. Rowland, and afterwards fully developed by myself, he says: "But against this procedure," that is my own, "there stands the pure assumption as regards discontinuity of electric force at an interface."

To fully discuss the defects of Larmor's resuscitation of Fitzgerald's theory would occupy too much space, and would necessitate the introduction of a considerable amount of mathematical analysis. I shall, therefore, confine myself to pointing out that his theory is open to exactly the same objections as my own, viz. *discontinuity of the tangential component of electromotive force at an interface.*

One of Larmor's boundary conditions (see p. 349) is equivalent to the condition that the expression

$$\frac{4\pi g}{K} + 4\pi\frac{Cd\beta}{d\theta} - 16\pi^2\pi C\gamma_0\frac{df}{dt}$$

should be continuous. Now $4\pi g/K = Q$, where Q is one of the tangential components of the E.M.F. at an interface; also in unmagnetised media $C = 0$. Consequently, if accented letters refer to the latter medium, the condition becomes

$$Q + 4\pi C d\beta/d\theta - 16\pi^2 C\gamma_0 df/dt = Q';$$

in other words, *the tangential component of the E.M.F. is discontinuous.*

A. B. BASSET.

Holyport, Berks, October 9.

The Society of Chemical Industry and Abstracts.

AT the recent annual meeting of the Society of Chemical Industry, the retiring President and the new President each made some remarks concerning the cost of the journal of the Society, and the necessity of curtailing expenses by dealing more strictly with the abstracts. I suppose hardly any two of us would quite agree as to what is the rubbish, Teutonic or otherwise, which ought to be left out, and what is good matter, which ought to be abstracted at greater or less length. No matter who is editor, all of us would abide as firmly as ever in the belief that we could have made a better selection of articles for abstraction. Before, however, we set about any further movement in the direction of cutting down abstracts to a mere useless list of titles, I would like to point out one direction in which expense might safely be curtailed without fear of objection from any quarter. All will agree, I am sure, that it is a waste of money to abstract the same article twice. I am sure other members besides myself must have noticed that this blemish is not entirely absent from the Society's journal. It should be known to every chemical babe and suckling, that even very unimportant papers are sometimes published more than once. Yet this seems to have escaped the notice of whoever is responsible for the editing of the abstracts. Witness the following from this year's journal:—P. 389, "Sulphides of Cobalt and Nickel, A. Villiers (*Bull. Soc. Chim.*, 1895, 13 [4])," and "*Qualitative* Separation of Nickel from Cobalt, A. Villiers, *Bull. Soc. Chim.*, 1895, 13 [4]." Now let us turn to p. 524, where we find, "Sulphides of Nickel and Cobalt, A. Villiers, *Comptes rend.*, 1894, 119," and on p. 509, "Qualitative Separation of Nickel and Cobalt, A. Villiers, *Comptes rend.*, 1895, 120." We have cobalt and nickel in one case, and nickel and cobalt in the other; but the articles from the *Bull. Soc. Chim.* are the same as those from the *Comptes rend.*, and by the same author. A still more incomprehensible example will

be found on comparing pp. 191 and 313. On p. 191 we have a short abstract of an article on petroleum, by A. Riche and G. Halphen. On p. 313 we have a long abstract of the same article. In one case it is given under *qualitative* organic chemistry, in the other under *quantitative* organic chemistry. Yet the reference in each case is the same—"*J. Pharm. Chim.*, 1894. 30, 289." In this case, therefore, the abstracts are not even prepared from different journals.

I would suggest, then, that the first reform which the Editing Committee might institute in carrying out their scheme of retrenchment, should be one placing a limit on the number of abstractors who are to deal with one and the same article, even when it occurs in different publications. JAMES HENDRICK.

Glasgow, October 2.

Note on the Dendrocolaptine Species, "Dendrexetastes capitoides" of Eyton.

IT recently became necessary for me to examine some of the Dendrocolaptine birds in this museum, and among them the species named above. Our specimen, the type of the genus *Dendrexetastes* founded by Eyton in 1851, in Jardine's "Contributions to Ornithology," on a skin from an unknown locality, formerly in Lord Derby's museum, has evidently been examined by Dr. Sclater, for its label bears, in the well-known calligraphics of that distinguished authority on this group, the name *Dendrexetastes temmincki*. The difficulty I have in ascribing our specimen to that species is the cause of this note. According to the fifteenth volume of the "British Museum Catalogue of Birds," by Dr. P. L. Sclater, the genus contains but two species, *D. temmincki* and *D. devillii*, which, by his key on p. 140, are distinguished from each other, the former by having "blackish cross-bands" on the belly, and the latter having that region "uniform brown." On consulting Eyton's original description in the "Contributions to Ornithology," I can find no mention of any cross-bands on the belly; for there are none on the skin, which is apparently that of a mature bird. In looking up next the description by Lafresnaye, in the "Revue de Zoologie" for 1851, of his *D. temminckii*, to which Dr. Sclater has relegated as a synonym Eyton's *D. capitoides*, I read:—" . . . . pectoris ventrisque plumis totis umbrinis, in medio maculâ triangulari-elongata nivea nigro marginata notatis; ventris maculis strictis; fere linearibus; subcaudalibus pallide rufescentibus, albo late, fuscoque anguste vittatis. . . ." These words, as I interpret them, make no mention of the presence of cross-bands on the belly of *D. temmincki*, while the latter half of the quotation, in regard to the under-tail-coverts being pale rufous, with broad white and narrow fuscous spots, does not apply to *D. capitoides*, for the type-skin before me presents no such characters. The plate illustrating Lafresnaye's description of the first-mentioned bird (*loc. sup. cit.*) shows its breast-spots to be much narrower, though not linear, and shorter than those in *D. capitoides*, while the spots on the feathers on the upper part of the belly can hardly be termed "fere linearibus," which they are, however, in *D. capitoides*. The lower belly in the plate, "plumis totis umbrinis," shows, just as in the last-mentioned species, not a single cross-band. It would appear to me, therefore, that *D. capitoides*, Eyton, can scarcely be = *D. temmincki*, Lafr., while the latter differs from *D. devillii* (of which I regret our museum does not possess a specimen), and, I take it, from *D. capitoides*, by its smaller and narrower throat-spots. The subcaudal characters separate *D. capitoides* from *D. temmincki*, and apparently the typical *D. devillii* is separated from it also by the "striis strictissimis" of the breast, and the very linear shaft-stripes of the upper neck feathers. Is *D. capitoides* = *D. devillii?* Or are there three species? I incline to the opinion that there are. HENRY O. FORBES.

The Museums, Liverpool, October 8.

The Pressure of a Saturated Vapour as an Explicit Function of the Temperature.

IT may be of some interest to note that Cailletet and Mathias' "Law of Diameters," in combination with any equation of state, such as Van der Waals', which applies to the region of coexistence of liquid and vapour, supplies an (empirical) expression for the maximum pressure of a vapour at any temperature T in the form of an explicit function of this temperature and known constants.

Let p, v and T denote the pressure volume and absolute temperature of unit mass of the substance. According to Van der Waals' original equation of state, we have then:—

$$\left(p + \frac{a}{v^2}\right)(v - b) = \mathrm{RT}.$$

If v_1, v_2, v_3 denote the roots of this cubic in v, we have:

$$v_1 + v_2 + v_3 = b + \frac{\mathrm{RT}}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{i.})$$

$$v_1v_2 + v_2v_3 + v_3v_1 = \frac{a}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{ii.})$$

$$v_1v_2v_3 = \frac{ab}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{iii.})$$

Now, for any definite value of T less than the critical temperature, these equations give, when we put p equal to the maximum vapour-pressure corresponding to this temperature, three values, v_1, v_2, v_3, two of which (say v_1 and v_2) denote the volumes of unit mass of the substance in the states of saturated vapour and "saturated" liquid at this temperature. Accordingly, $\frac{1}{v_1}$ and $\frac{1}{v_2}$ denote the densities of the substance in these states, and the law of Cailletet and Mathias, above referred to, enunciates that the arithmetic mean of these densities can be very fairly represented as a linear function of the temperature. Therefore we can write:—

$$\frac{v_1 + v_2}{v_1v_2} = \phi\mathrm{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{iv.})$$

where ϕ denotes a linear function, whose two constants are known.

Eliminating v_1, v_2, and v_3 from the four equations (i.), (ii.), (iii.) and (iv.), we readily obtain:—

$$p = \frac{\mathrm{RT}(1 - b\phi\mathrm{T}) - a\phi\mathrm{T}(1 - b\phi\mathrm{T})^2}{b^2\phi\mathrm{T}} \quad . \quad . \quad . \quad (\text{v.})$$

This result simply amounts to the following:—

If we fix the temperature T of a vapour, then the maximum vapour-pressure at this temperature is completely determined, *i.e.*

$$p = \mathrm{F(T)}.$$

Similarly the sum of the densities of saturated vapour and liquid in contact with it is determinate if T is fixed, and thus

$$\frac{1}{v_1} + \frac{1}{v_2} = \phi(\mathrm{T}).$$

Equation (v.) shows that the former function is known if the latter be known, and as Cailletet and Mathias have shown that the latter is very approximately linear, we can give the form of F(T).

This result, however, is not of any practical use unless the equation of state does really apply with good approximation to the region of liquid and vapour. F. G. DONNAN.

Holywood, Co. Down.

Colours of Mother-of-Pearl.

IN numerous text-books the colours of mother-of-pearl are included amongst phenomena of colour produced by striated surfaces, and though it is conceded that only a part of the colour is due to this cause, that part is generally assumed to be, at any rate, an appreciable quantity. Experiment will show, however, that such is not the case. When the colour produced by the striations is viewed in an impression of the pearl on sealing wax or gelatine it is visible, though it is totally different in character from the iridescence of the pearl itself, in which the tiny contribution of colour from the striations is completely overpowered by that due to another cause. In white mother-of-pearl the striations are often as close together as in coloured varieties, and at certain angles, when viewed by light from a defined source, there is a little colour visible in the white specimens; just so much, and no more, is contributed by the striations of the coloured specimens, as may be shown by viewing a piece under the surface of water, when the effect of the striations is necessarily abolished, though the iridescence is not at all appreciably diminished. The whiteness of some varieties must be attributed to a different thickness or greater opacity of the laminæ. It is

these laminæ which, acting as "films," give rise to all the colour of nacre, practically; and the phenomenon should be included amongst those of colours from "films," and not from "striated surfaces," the latter being merely incidental, and for all practical purposes contributing nothing to the effect.

C. E. Benham.

A RATIONAL CURE FOR SNAKE-BITE.

When it was established beyond dispute or cavil that the serum obtained from animals, immunised against bacterial infections and intoxications, possesses in a marked degree antitoxic powers (as distinguished from antibiotic powers) and that such serum when mixed in a test-tube with the bacterial poison in question will, so to speak, neutralise the toxic effects of such poison, however deadly, it was merely a question of time, opportunity, and patience that attempts would be made to extend the principle of serum-immunisation to other, *i.e.* non-bacterial, poisons. Ehrlich was the first to show us the way. He gradually accustomed animals to withstand comparatively large doses of abrine, ricine, and robine, three vegetable toxines, all belonging to the group of proteines, reacting as albumoses or globulines. In that manner he produced in the animals a relative immunity, or perhaps, more correctly, a tolerance. He found that though subcutaneous inoculations lead to better results, that this immunity can be brought about also by feeding. In whatever way the animal is prepared, its serum eventually acquires specific antitoxic, immunising, and curative properties. It was thus demonstrated that the wonderful discovery of Behring and Kitasato—for which Behring, however, claims the sole credit—has a scope much wider than at first was dreamt of. Behring himself, to begin with, explained the action of the serum as antibiotic or germicidal; but it soon became evident that, though when injected into the animal body it causes the destruction and death of the infective pathogenic organisms, nevertheless its chief action is "vitally" antitoxic. For working with the tetanus toxine alone, separated from the bacilli which produced it, its deadly effects can be readily neutralised by a few cubic centimetres of a powerful serum. And if we remember that ·23 milligram of tetano-toxine would represent the fatal dose for a human being weighing 70 kilogrammes, then we can get an idea as to what extraordinary changes must have been produced in the serum, or rather in the blood and tissues, of the immunised animal, to enable its serum instantaneously to remove the lethal effect of the toxine. The only poison comparable to tetano-toxine in virulence and rapidity of action is cobra poison, and it also resembles chemically the bacterial toxines, reacting as an albumose, though for the sake of accuracy it must be mentioned, that the poison of tetanus has been clearly shown by Brieger, Cohn, and Sidney Martin not to be an albuminous body, and that possibly most of the bacterial toxines may turn out not to be albuminous substances. Still, so far as our present knowledge reaches, cobra poison and other snake venoms are chemically closely allied and analogous to the "toxalbumins" of bacteria.

It had also been demonstrated by several observers,[1] that by means of oft-repeated injections of small sub-lethal doses of snake poison (rattlesnake, cobra, or viper venom) the resistance of an animal against the poison may gradually be increased considerably, it may be rendered "giftfest," to borrow a German expression. In fact, all the methods used for inducing a tolerance against tetanus poison can be shown to work in the case of cobra poison (this is the poison generally employed). That Calmette, whose work in this line follows directly that of Sewall's and of the writer of this article, has shown that a so-called immunity can also be produced by gradually increasing injections of poison attenuated by heat, iodine, trichloride of iodine, hypochloride of calcium, &c.; in fact, the analogy is complete. From this stage, at which others had already arrived, Calmette went ahead with Phisalix and Bertrand. Having previously attempted both to prevent and to cure the effects of inoculation with cobra poison by means of chloride of gold—wherein, however, as shown by the writer,[1] he failed—he directed his attention at once to the serum of immunised animals, and in February 1894 he showed, before the Société de Biologie, that on mixing cobra or viper venom with small quantities of serum obtained from an immunised rabbit the deadly effect of the venom disappears, a fact at once confirmed by independent observations of Phisalix and Bertrand. In May 1894 and in April 1895, Calmette published two concise papers in Pasteur's *Annales*, containing a full account of his results. These, briefly summarised, are as follows: (1) The serum of an animal immunised against snake poison (he used poisons of the following snakes: *Naja tripudians and haje, Crotalus durissus, Bothrops lanceolatus, Cerastes, Pseudechis porphyriacus, Hoplocephalus curtis and variegatus, Acanthopis antarctica, Trimeresurus viridis*) possesses properties similar to those which the serum of animals immunised against tetanus and diphtheria possesses. (2) The serum of a rabbit immunised against cobra or viper venom acts equally well against any of the other poisons, *i.e.* there is no specificity of action, as judged by the species of snake. (3) The serum possesses not only neutralising properties when mixed with the venom in a test-tube, but possesses also marked immunising and curative properties, *i.e.* poison injected after previous serum administration becomes powerless, and serum injected after previous poison administration neutralises the effects of the poison in the animal body, even after the symptoms of intoxication have already set in. Naturally the effect depends on the degree of immunity of the serum giver and on the proportionate amount of serum used. (4) The immunising effect produced by serum injections is not so lasting as that produced by direct injections of the poison, *i.e.* serum injections are incapable of rendering animals "giftfest." Calmette alludes to other matters, but since these are of secondary importance and still debatable, and not directly related to the subject of this article, we must pass them over. There is, however, one point which must be mentioned, since it is one affecting the whole principle of serum immunisation. He states that he has succeeded in producing a "Giftfestigkeit" by means of repeated intravenous injections of hypochloride of calcium, and that the serum of such "chlorinated" animals will neutralise, in the test-tube at least, the effects of cobra poison. Roux elsewhere mentions[2] that the serum of animals immunised against tetanus or rabies is capable of neutralising snake venom and of protecting other animals against subsequent intoxication with cobra poison, and that rabbits vaccinated against rabies can withstand four to five times the lethal dose of cobra venom; and also that abrine serum will counteract the effects of cobra poison, and cobra serum those of abrine. Calmette goes so far as to say that an animal vaccinated against abrine may acquire a relative immunity against diphtheria, ricine, and anthrax. If this be so, we shall have to modify our views as to the specific action of antitoxic serum, *i.e.* the first principle of serum therapeutics. We require a number of control observations before we can accept these remarkable statements; partial contradiction they have already received from Germany,[3] and the

[1] [illegible] *Journal of Physiology*, Kaufmann, *ibid.*, [illegible], vol. xvi. Nos. [illegible]; Phisalix et Bertrand, *Compt. rend. de la Soc. de Biol.* [illegible] xviii. 1894, [illegible] *Arch. de physiol.* [illegible] 1894, pp. [illegible]; *Compt. rend.* [illegible] 1894, pp. [illegible]; Kaufmann, *ibid.*, p. [illegible]; and Calmette, *Annales de l'Inst. Past.* 1894, vol. [illegible] No. [illegible] p. [illegible].

[1] *Lancet*, June 11, 1892. The uselessness of strychnine was previously demonstrated by the writer in his paper in the *Journal of Physiology*.

[2] *Annales de l'Inst. Past.* 1894, No. 11, p. 722.

[3] Ehrlich emphatically denies any such vicarious counteraction with regard to abrine and ricine (cf. *Deutsche Med. Wochenschrift*, vol. xvii. No. 44, p. 1218).

writer's own experiments, so far at least, do not lend much support to them. So long, however, as the whole question of this new treatment, striking though it is in its results, is still a mystery to us, we cannot afford to push aside observations because they seem improbable, or because they are contradictory.

Calmette asserts also that the fresh serum of *Naja tripudians* (a species of cobra) possesses to some degree at least immunising properties, and, as we shall see, Fraser[1] bears him out in this, by stating that fresh serum of poisonous snakes possesses strong antitoxic and protective properties, not only against their own venom, but also against that of other species. D. D. Cunningham[2] and the writer,[3] however, in India, invariably failed to obtain antitoxic or immunising effects with cobra blood or serum, although the writer succeeded in keeping the effects of cobra poison in abeyance by means of the blood (or serum) of the *Varanus Bengalensis*, a large lizard which is naturally strongly resistant against cobra poison.

These are the chief results obtained by Calmette, and knowing the difficulties of working with such deadly poison as cobra poison venom is, and the innumerable failures which accompany it, the writer is able to appreciate the success of the French author, all the more since he himself failed while working on the same lines where to succeed seemed simply a matter of course. Recently these French observations have received entire confirmation in their leading points by Prof. Fraser of Edinburgh, and the writer may be forgiven for stating here that though he took up the control of Calmette's work with strong bias against the latter, he felt himself forced, already before Fraser's communications appeared, to acknowledge the correctness of the work done at Pasteur's Institute, so far as the antitoxic and immunising properties against cobra poison of serum obtained from animals treated with that poison are concerned. He has not, however, convinced himself that hypochloride of calcium can immunise animals, or lead to the formation of an antitoxic serum. Fraser's contributions, though merely confirmatory, are of great importance, since they contain unquestionable proof of the truth of what must have appeared to all, except a few shrieking "zoophilists," to be striking and surprising revelations. The credit, however, of the discovery of a cure for snake-bite in the laboratory at least belongs solely to France. Having discussed Calmette's work more fully, we can speak of Fraser's experiments in a few words; but thereby we do not wish to detract in any way from the merit which characterises his researches.

Fraser[4] worked with venom obtained from the Indian cobra, three species of rattlesnakes (*Crotalus horridus*, *C. adamanteus*, and *C. durissus*), the copper-head (*Trigonocephalus contortrix*), the Australian black and brown snakes, and an unidentified *Diemenia* (*Pseudechis porphyriacus* and *Diemenia superciliosa*), the African puff-adder, night adder, yellow cobra, and "rinkas" (*Vipera arietans*, *Aspidelaps lubricus*, *Naja haje*, *Sepedon hæmachates*). He immunised his animals by the usual method of minimal subcutaneous inoculations, or by feeding, against the venoms of some of the snakes mentioned, and then established (*a*) the strong specific antidotal properties of the serum of these vaccinated animals against the poison with which they had been vaccinated, and (*b*) the vicarious antidotal properties against the other poisons. This serum he obtained in a dry, pulverisable condition without any appreciable loss of antidotal power; but we can hardly forgive him the hybrid and barbaric name "antivenene" which he applies to it. He confirms Calmette's results in almost every point, so that there is no longer any doubt left as to possibility of a successful cure against snakebite, especially as, by both observers, the curative injection was shown to be efficacious when the symptoms of intoxication had already set in, and as the experimental animals used were highly susceptible to the poisonous action of serpents' venoms, while man is weight for weight much less sensitive than a guinea-pig or a rabbit. True, Fraser has generally worked with comparatively small lethal doses; this possible objection is, however, met by Calmette's results, which were obtained with much larger doses, and which therefore allow us to judge favourably of the practical application of the serum treatment. The final verdict must, of course, depend on the success or failure following the use of the serum in cases of snake-bite, and it must be remembered that, striking though our laboratory results are with tetanus antitoxine, so far the success obtained with acute cases of tetanus in man is disappointingly small, as the writer has shown elsewhere.[1] Yet here we have a rational method of treatment, and the promise of almost certain success; we must now look for facilities and opportunities of trying the cure. In France they have already begun to manufacture this antitoxic serum in larger quantity, and Calmette writes that he has immunised a horse, and is ready to supply the remedy; and Fraser also has larger animals under treatment. No doubt India will not delay in carrying out the necessary arrangements for procuring what, after all, will be an imperial benefit.

The vicarious action of the immunising venom-serum is surprising, and may find an explanation in the similarity of the physiological action of the various poisons used. They are all poisons which cause death by acting on the central nervous system, especially the medulla, the animal dying from respiratory failure with salivation, retching, &c. And it is quite possible that chemically similar poisons which, according to their action on the animal body, belong to one physiological group, have the same antidote. It would therefore be interesting to test the antitoxic cobra-serum on the poison of the Daboia, which, according to Wall, Cunningham, and others, differs essentially in its physiological action; for whereas cobra, crotalus, and viper venoms are paralysing, medullary poisons, the poison of Russell's viper produces very varying symptoms, in some cases convulsions, in others paralysis and asphyxia, in yet others violent convulsions followed by paralysis. Daboia venom undoubtedly contains a substance capable of producing the most violent convulsions, especially in birds, their occurrence depending on the size of the animal and on the amount of poison injected. It would indeed be more than a surprising revelation, if a serum which is capable of acting as an antidote to a paralysing toxine were also capable of neutralising the effects of a toxine of opposite physiological action.

The vicarious antidotal action of venom-serum must appear all the stranger and more contradictory if we remember that not all poisonous snakes are "giftfest" against the poisons of other different species. Waddell[2] has shown that the venom is neither a poison to the snake itself nor to members of its own species, but that cobra poison is fatal to some, if not perhaps to all, poisonous snakes. It will certainly kill the *Trimeresurus erythrurus*, and in the writer's experience also the crotalus, while according to Fayrer the *Bungarus* readily falls a victim to the bite of a cobra. This being so, why should the antitoxic serum of an animal immunised against cobra-poison be active against rattlesnake venom, when in an experiment recently performed by the writer, a strong and healthy crotalus succumbed to five milligrammes of cobra venom? Lastly some writers, Fraser included, assume that the immunity of poisonous snakes against their own

[1] *Lancet*, August 10, 1895, p. 376, and *Brit. Med. Journal*, Aug. 17, 1895.
[2] Private communication.
[3] *Journal of Physiology*, 1892, vol. xiii. Nos. 3 and 4, p. 28?.
[4] *British Medical Journal*, 1895, June 15, p. 1309-1312.

[1] *Medical Chronicle*, May 1895.
[2] "Scientific Memoirs by Medical Officers of the Army of India," 1889, iv. p. 59.

poison depends on self-immunisation, called forth by swallowing their own venom, or by repeatedly inoculating themselves. This is highly improbable, if we remember that some of the innocent snakes are very resistant against cobra poison, as, *e.g.*, the *Ptyas mucosus* and the *Tropidonotus natrix*, and also that, as the writer has shown, the *Varanus Bengalensis* is possessed of a marked tolerance, and that, according to Fayrer, other species of *Varanus* survive the bite of a cobra 24 to 48 hours. Jourdain further gives a list of four innocent snakes which are immune against viper venom. In what manner are we to account for this immunity? Interesting observations on the poisonous nature of serum of innocent and poisonous snakes are also found in Calmette's paper of April 1895, which, while rendering Fraser's theory still more improbable, do not assist us in clearing up the mystery. The explanation must be left to future researches; for the present we must be thankful for the promise which the researches of Calmette and Fraser have given us, of allaying an almost national calamity.

A. A. K.

SCIENTIFIC KNOWLEDGE OF THE ANCIENT CHINESE.

THE question of China has been so much to the front lately, that an article which appeared in one of the August numbers of the *Revue Scientifique*, on the knowledge of science possessed by the Chinese, seems very *à propos*. It cannot be denied that the Chinese of the present day have very elementary ideas on any branch of science. This however, was not so formerly.

In early times, as far back even as 2000 B.C., we find that science in China had reached a fairly advanced stage. The Chinese possessed undoubtedly a great knowledge of astronomy; inscriptions have been found which prove this. In the "Chou-King," a book of records, we read that Emperor Yao, who reigned 2357 B.C., did much to advance the study of this science. He ordered his astronomers to observe the movements of the sun, moon and stars, and showed them how to find out the commencement of the four seasons by means of certain stars. We read also that he told them that a year consisted of a little less than 366 days, and as he divided the year into lunar months, he taught them the years in which the additional lunar month ought to be included. It is also known that the Chinese had the annual calendar, that they observed the planets Mercury, Venus, Mars, Jupiter, Saturn, and were able to calculate eclipses, and knew the difference between the equator and the ecliptic. It is quite probable that the ecliptic was not known of before the Mussulmans occupied the Mathematical Tribunal, which they held for three centuries.

We see, therefore, that the knowledge of astronomy was very extensive. With regard to the meridian, it was apparently unknown to them. M. Chavannes, who is at present Professor of Chinese at the College of France, says that it is not mentioned in any astronomical book. As substitute a certain star was observed at the same hour, according to the times of the year, note being taken of its positions with regard to the horizon.

Astronomy has always been closely connected with astrology. By means of astronomy the time was ascertained for the numerous public ceremonies recorded in the Imperial calendar; it likewise regulated the affairs of the Government. But the calendar has long since ceased to be used for this latter purpose, and the majority of the Chinese population merely look upon it as a means of continuing the mysterious ceremonies and oracles connected with the different positions of the planets. It is ordered in the "Collection of the Laws," that at each eclipse, ceremonies should be gone through to succour the eclipsed sun or moon. At this time therefore, an alarm is sounded on the drums, the mandarins arrive armed, utter many objurgations, and thus deliver the endangered bodies.

In the seventeenth century, certain Jesuit missionaries arrived in China. On seeing the low state into which the Mathematical Tribunal had fallen, they offered to help it. They found an observatory containing many instruments, which shows plainly that this branch of science had at one time reached an advanced stage. This decay of science is not to be wondered at when we remember that twenty-two dynasties were brought on the throne by actual revolutions. Nor is this decay confined to astronomy. According to the ancient books and traditions, we find that various branches of science had reached a high degree of culture.

The Emperor Kang-hi, who reigned in the seventeenth century, had a great love of study himself, and endeavoured to advance the general education in China. The Jesuit missionaries instructed him in geometry and physics. He translated some text-books into Chinese.

The Chinese have generally been credited with the invention of gunpowder. A certain document has been found, however, by Archimandrite Palladius, a Russian sinologue, stating that in the ninth century a Persian regiment, under the Chinese sovereign, made known a material similar to wild fire, which was afterwards used for fireworks.

Apparently, chemistry has never been studied, unless by a certain sect, the Tao-tse, who spent all their time endeavouring to discover the philosopher's stone and the elixir of life.

The Chinese have not a great knowledge of geology. The mines have been worked without any machinery, and are not very deep, therefore fire-damp has rarely been the cause of destruction. Coal was extracted at as early time as 200 B.C. in the dynasty of Han. Although the mode of extraction was very primitive, enough was obtained to satisfy all wants.

About 1861 the Government handed the exploration of the mines over to American prospectors. The work, lasting from 1862-64, was directed by Prof. Pumpelli, who at its termination sent the Emperor a report and a map of the coal-fields. The Smithsonian Institute of Washington have had these documents published; they have also appeared in the diplomatic correspondence of the United States (1864). Later on, Baron de Richtofen did similar work, and found that the coal-fields in China are even more extensive than those in North America.

Research work has not been carried far in natural science. In zoology their classifications are quite wrong. The drawings in zoological and botanical books can often scarcely be recognised. Their most ancient work on botany dates from 2700 B.C., and is a treatise written by the Emperor Shen-nung; it is merely enumerative. Another work, the "Rh-ya," dates from 1200 B.C., and shows signs of progress. The "Pen-tsao," an encyclopedia, is, according to M. Bretschneider, of little value.

This Russian investigator speaks of the Chinese as follows: "It is an undeniable fact that the Chinese do not know how to observe, and have no regard for truth; their style is negligent, full of ambiguities and contradictions teeming with marvellous and childish digressions."

However, in a more recent communication, M. Bretschneider retracts his words, and says that it is more that the Chinese will not observe, than that they cannot, for Lichi-Tchen, author of several interesting pamphlets, brings forward many facts concerning cultivated plants.

With regard to medical science, it is very elementary. Occasionally here and there a successful doctor is to be found. This lack of knowledge is not to be wondered at, for Buddhism forbids dissection of bodies. In the temple of Confucius a bronze figure is to be found, on which all the different parts are marked where the surgical needle

may be applied. This needle is practically the only instrument used in the profession.

The height of civilisation in China was reached at the end of the reign of Kang-hi. The gradual decline is supposed to have commenced with the Tartar domination.

THE FLORA OF THE GALAPAGOS ISLANDS.

DR. G. BAUR'S theory of the origin of the Galapagos Islands is too well known to need explanation here: yet it may be briefly designated the theory of subsidence. He argues that the islands were formerly connected with each other, and at an earlier period with the American continent. It is also almost needless to say that this theory has met with an exceedingly hostile reception; few indeed accepting it, even as restricted to a former union of the islands themselves. The publication of an account of the botanical collections[1] affords an opportunity of examining this theory from a botanical standpoint. For the purposes of the "Botany" of the *Challenger* Expedition, and ever since the publication of that work, I have collected all the data coming under my notice bearing on the dispersal of plants to considerable distances by wind, water, birds or other creatures excepting human. The evidence thus collected sufficiently accounts for the vegetation of low coral islands, and the littoral vegetation of widely separated countries; but it in no way helps to explain the vegetation of the enormously distant islands of the Antarctic seas, for example, or that of the islands of the Galapagos group, to give another instance.

But these are not parallel cases; they are the two extremes in the amount of differentiation in connection with isolation.

The biological phenomena of the Galapagos Islands left a deeper impression, probably, on the mind of Darwin than those of any other part of the world he visited, and doubtless had much to do with his later conception of the origin of species. The fact on which he laid special stress was that the genera, to a very great extent, were the same in all the islands, and the species different in each island. Dr. Baur's much more extensive zoological and botanical collections and observations confirm and emphasise the correctness of the view of his illustrious predecessor of fifty years ago. Darwin specially refers to the existence of different species or races of tortoises and mocking-thrushes in many of the islands; and Baur's examination of the lizards of the genus *Tropidurus*, from twelve of the islands, reveals the same condition of things. The botanists bring forward *Euphorbia viminea* in illustration of this phenomenon. This species was described by Sir Joseph Hooker from a single specimen collected by Macrae in Albemarle Island, and the author remarks that he "knew of no species with which to compare this highly curious one." Dr. Baur collected it extensively in eight of the islands, and the specimens from almost every one of them exhibit distinct racial characteristics. *Acalypha*, a genus of the same natural order, presents somewhat more pronounced variation in the different islands, which some botanists regard as of specific value; other botanists as of varietal value only. Whatever status we give these forms, the flora as a whole is a most instructive and convincing illustration of evolution.

A remarkable peculiarity of the Galapagos flora, as an insular flora, is the almost total absence of endemic genera, for the two or three genera of the Compositæ restricted to the islands are so closely allied to American genera as hardly to count as distinct. Indeed the whole flora is so thoroughly American that, apart from geological difficulties, it might be regarded as a differentiated remnant thereof, rather than derived therefrom, after the supposed elevation of the islands. Analogous conditions and phenomena are repeated in the deep valleys of the great mountain chains of northern India and western China, where, in neighbouring valleys, the genera are to a great extent the same and the species different.

Returning to Dr. Baur's extensive botanical collections from the Galapagos, it may be mentioned that they yielded about a dozen new species belonging to the predominating genera.

Looking at the composition of the Galapagos flora, especially with an eye to the probabilities of the transport of the seeds of its constituents, combined with present conditions, Dr. Baur's theory seems deserving of more serious consideration than it has hitherto received. My very slender knowledge of geology alone prevents me from taking up a more decided position.

W. BOTTING HEMSLEY.

[1] B. L. Robinson and J. M. Greenman, in *American Journal of Science*, vol. l. pp. 135-149.
N.B.—Dr. G. Baur was attached to the United States Fish Commission steamer *Albatross*, and spent nearly three months in the islands, from June 10 to September 6, 1891.

THE LATE PROFESSOR HOPPE-SEYLER.[1]

II.

Hoppe-Seyler's Work in Berlin, 1850 54 *and* 1856–61.

IT has already been stated that Hoppe selected as the subject of his inaugural dissertation some observations on the structure of cartilage and on chondrin.[2] Chondrin had been first separated and examined by Johannes Müller,[3] and afterwards by Mulder and Donders. Pursuing his study of the chemical reactions of the so-called chondrin, Hoppe in 1852[4] described its lævo-rotatory property, and showed that when decomposed by long boiling with dilute mineral acids it yields leucine, but neither glycocine nor tyrosine. Still directing his attention to the connective tissues, Hoppe in the following year published a valuable and interesting paper[5] on the structural elements of cartilage, bone, and tooth. Virchow had shown[6] the possibility of isolating the so-called bone corpuscles. Hoppe now alleged facts which seemed to prove that the lacunæ and canaliculi of bone are lined by a tissue resembling elastic tissue, and are left surrounding the bone cells when decalcified bone is boiled in a Papin's digester. Extending his investigation to tooth, Hoppe studied the chemistry of the organic basis of dentine, and isolated the "dentinal sheaths," which he showed to correspond structurally and chemically to the more internal portion of the ground substance of bone, which may be separated as a distinct investment bordering the lacunæ, canaliculi, and Haversian canals. There can be no question of the important bearing which these early histologic-chemical researches had upon the development of our knowledge of the relations and affinities of the connective tissues: attention has been drawn to them for this reason, as well as because they differed somewhat in their scope and method from the work with which Hoppe afterwards mainly busied himself.

Passing over three interesting papers on auscultation[7] and communications of minor importance on chemical

[1] In the fragmentary notes which follow, I do not pretend to give a complete or entirely consecutive account of Hoppe-Seyler's labours; my object is to draw attention to some of the principal results of his life-work, and to indicate in this way his position among those who, during the last half-century, have contributed to the advancement of biological science.—A. G.
[2] F. Hoppe, "De Cartilaginum Structura et Chondrino nonnulla," Diss. Inaug. Berol. 1850.
[3] Joh. Müller, *Poggendorff's Annalen*, vol. xxxviii. (1836) pp. 295-356.
[4] Hoppe "Ueber das Chondrin und einige seiner Zersitzungsproducte," *Journ. f. Prakt. Chemie*, vol. lvi (1852) p. 129.
[5] Hoppe, "Ueber die Gewebselemente der Knorpel Knochen und Zähne," Virchow's *Archiv*, vol. v. (1853) p. 17[illegible].
[6] Virchow, "Verhandl. d. Phys. Me[illegible]. Gesellschaft zu Wurzburg," vol. ii. p. 152.
[7] Virchow's *Archiv*, vol. vi. (1854) pp. [illegible]3-173, vol. vi. (1854) pp. 331-[illegible]49, vol. viii. (1855) pp. 250-259.

questions relating to physiology and pathology, we come to the first in the long series of valuable contributions which Hoppe made to the physiological chemistry of the blood. This short paper of only two pages was published in 1857, after his return to Berlin, and consisted of a preliminary communication on the action of carbonic oxide on the blood.[1] In this paper he announced that carbonic oxide so affects the colouring-matter (at that time designated Hämatoglobulin by Hoppe) as to render it incapable of fulfilling the function, so important for the blood as well as for the whole organism, of acting as the carrier of oxygen. Simultaneously and independently, Claude Bernard[2] had observed the same facts as Hoppe, and had shown, in addition, that when carbonic oxide acts upon blood it is absorbed and displaces oxygen. Although his analytical data did not bear out the assertion, Claude Bernard stated that for each volume of oxygen displaced one volume of carbonic oxide is absorbed, a relation which was afterwards shown to be actually correct by the fine investigation of Lothar Meyer.[3] As will be afterwards stated, it was, however, Hoppe-Seyler who, in 1865, after Stokes' beautiful researches on the reduction of oxy-hæmoglobin, furnished the complete explanation of the way in which carbonic oxide exerts its action on the blood and its colouring-matter, and placed in the hands of the medical jurist a method of distinguishing between blood which has been rendered florid by carbonic oxide and blood which owes its red arterial colour to oxygen.

The year 1857 witnessed also the publication of the first[4] of a series of researches on the property which many of the proximate principles of the body possess of rotating the plane of polarisation. Biot had discovered that albumin rotates the plane of polarisation to the left, and Bouchardat and A. Becquerel had endeavoured, but without success, to base upon this discovery a method for the quantitative estimation of albumin. In his first paper Hoppe showed (1) that, as was to be predicted, the rotation produced by a solution of albumin was strictly proportional to the amount of albumin in solution, and to the thickness of the stratum traversed by the light; (2) that albumin existing in a state of solution in a liquid rotates the plane of polarisation of light almost exactly as much to the left as an equal percentage of grape sugar rotates it to the right. In the same year (1857) and the year following, Hoppe published other papers on the rotatory properties of other organic proximate principles of the animal body.[5]

With his hands full of original work, with the chemical laboratory of the Pathological Institute to direct, busily helping the students who were attracted to work under a teacher full of enthusiasm and ability, Hoppe yet found time to publish, in 1858, the first edition of his "Handbook of Physiologico-Chemical and Pathologico-Chemical Analysis."[6] The only work at that time in existence which fulfilled the same object was the very useful work of Gorup-Besanez, of which the first edition appeared in 1850, the second in 1854,[7] and the third and last in 1871. Hoppe-Seyler's book was written on lines essentially the same, but was distinguished by containing many new methods, the results of the original researches of its author; as, for example, on the rotatory properties of various origanic bodies, on the polarimetric estimation of albumin and milk-sugar, on the colorimetric estimation of the blood-colouring matter, on new methods of blood analysis, &c. Personally, the writer is greatly indebted to the first and the subsequent editions of Hoppe-Seyler's work, and in saying that it has exerted a powerful and useful influence in diffusing a knowledge of the best methods of practical work throughout the laboratories where researches in physiological chemistry are pursued, he is only expressing an opinion which he believes to be shared by all who are best qualified to judge. In spite of a decided narrowness, amounting at times to unfairness, which asserts itself in nearly all Hoppe-Seyler's writings, and which caused him to attach undue importance to his own work and that of his own pupils, and which explains some unfortunate omissions and deficiencies, the "Handbook" remains the recognised practical work consulted by the student of physiological chemistry. The sixth, and last, edition of the book,[1] edited jointly by Hoppe-Seyler and his pupil Dr. Thierfelder, appeared early in 1893.

1 Hoppe, "Ueber die Einwirkung des Kohlenoxydgases auf das Hämatoglobulin. [illegible] Mittheilung." Virchow's *Archiv*, vol. xi. (1857) p. [illegible].

2 Claude Bernard, "Leçons sur les effets des substances toxiques et [illegible]." Paris, 1857, see p. [illegible].

3 Lothar Meyer, "De sanguine oxydo carbonis infecto." Dissert. Inaug. [illegible] Vratislaviae, [illegible].

4 Hoppe, "Ueber die Bestimmung des Eiweissgehaltes im Urin, Blut- [illegible] Ventzke-Soleil'schen Polarisations-Apparates." Virchow's *Archiv*, vol. xi. (1857) pp. 547-[illegible].

5 (a) Hoppe, "[illegible] Circumpolarisationsverhältnisse der Leim und Gallen [illegible]." Virchow's *Archiv*, vol. xii. (1857) p. [illegible]; (b) "Bestimmung des M[illegible] der Milch mittelst des Soleil-Ventzke'schen Polarisations[illegible]." Virchow's *Archiv*, vol. xiii. (1858) p. [illegible]; (c) "Ueber die C[illegible] Eigenschaften der Gallensubstanzen und ihre Zersetzungs[illegible]." Virchow's *Archiv*, vol. xv. (1858) pp. 126-141.

6 I [illegible] by me this first edition, which I long treasured [illegible]. If I remember rightly, the title of the first edition [illegible] Path[illegible]-Chemischen Analyse."

7 [illegible] Anleitung zur qualitativen und quantitativen Z[illegible] (Nürnberg, Verlag v. J. L. Schrag, 1854.)

Hoppe-Seyler's Work in Tübingen, 1861-72.

With his appointment as ordinary Professor of Applied Chemistry in the University of Tübingen commenced the most prolific period of Hoppe-Seyler's scientific life, during which he contributed to science his researches on hæmoglobin and its derivatives—researches which, with the work of Stokes, Claude Bernard, Pflüger, Ludwig and his school, have furnished us with the greater part of the knowledge which we at present possess concerning the chemistry of the blood-colouring matter and the part which it plays in respiration. At Tübingen, Hoppe, then in the very prime of life, surrounded by pupils, amongst whom were Diakonow, Dybkowsky, Miescher, Parke, and Salkowski, showed much more clearly than was possible in the position which he occupied in Berlin, his capacity to be the head of a school—that is, his power of inducing men to work out his own ideas, and of animating them with the desire to advance science by their own researches.

It was in 1862 that appeared Hoppe's short but epoch-marking paper "On the behaviour of the blood-colouring matter in the spectrum of sunlight."[2] Through the researches of Brewster and Herschel, the fact that absorption bands occurred in the spectrum of light which had been passed through certain coloured gases, vapours, and diluted coloured solutions had become known, and the absorption spectra of indigo and chlorophyll had been described. The discovery of the wonderfully characteristic absorption spectrum of blood at once enabled Hoppe to assert that hæmatin, which had up to that time been by many considered the true blood-colouring matter, did not exist preformed in the blood corpuscles, but that it is a product of decomposition of the true blood-colouring matter which is the cause of the absorption bands which he had discovered, and which, under the influence of heat, acids, &c., splits up into hæmatin and an albuminous substance. Without doubt, added Hoppe, the true blood-colouring matter is the body which forms the blood crystals of Funcke, and these crystals are not, as Lehmann had erroneously supposed, composed of a colourless albuminous *hæmatocrystalline* stained with hæmatin.

There can be no question that Hoppe at once appreciated the immense value of the information which

1 "Handbuch der Physiologisch- und Pathologisch-Chemischen Analyse für Aerzte und Studirende, von Felix Hoppe-Seyler. Sechste Auflage neu bearbeitet von F. Hoppe-Seyler, Professor in Strassburg, und H. Thierfelder, Privat-docent in Berlin. (Berlin, Verlag von Aug. Hirschwald, 1893.)

2 Prof. Felix Hoppe in Tübingen, "Ueber das Verhalten des Blutfarbstoffes im Spectrum des Sonnenlichtes," Virchow's *Archiv*, vol. xxiii. (1862), pp. 446-449.

he had acquired by his study of the spectrum of blood, though the full light which it was destined to throw on the function of the blood-colouring matter was only recognised when Stokes published his paper "On the Reduction and Oxidation of the Colouring-matter of the Blood." Having described the beautiful experiments which he had performed after becoming acquainted with Hoppe's paper on the blood spectrum, Stokes stated the conclusions, which might legitimately be drawn from them in the following words: "We may infer from the facts above mentioned that the colouring-matter of blood, like indigo, is capable of existing in two states of oxidation, distinguishable by a difference of colour and a fundamental difference in the action of the spectrum. It may be made to pass from the more to the less oxidised state by the action of suitable reducing agents, and recovers its oxygen by absorption from the air."[1]

The new facts acquired by the combined use of chemical and optical methods at once explained a large number of facts. Hoppe-Seyler showed that carbonic oxide blood was distinguished from normal blood in being unacted upon by reducing agents, and thus placed a valuable test in the hands of the medical jurist called upon to investigate cases of death by charcoal fumes.[2] The explanation of the facts discovered by Claude Bernard and by Lothar Meyer was obvious—to wit, that carbonic oxide forms a compound with the blood-colouring matter, more stable than the oxygen compound, and in which apparently one molecule of CO has replaced O_2.

With the resources of spectrum analysis to aid him, Hoppe now devoted himself with energy to the investigation of the blood-colouring matter (which he named Haemoglobin[3]), showing how to separate and purify it by repeated crystallisation, determining its composition, studying personally, and, with the aid of his pupil Dybkowsky, its combinations with oxygen and with carbonic oxide, examining its products of decomposition, and showing its probable connections with certain other animal colouring matters.[4]

It would be impossible in this place to comment in detail on all Hoppe-Seyler's contributions to the chemistry of the blood-colouring matter; these constitute his highest claim to distinction, and will ever cause him to be remembered as having contributed most largely to our knowledge of the manner in which the respiratory exchanges of animals are effected.

Until he removed from Berlin to Tübingen, and for some time after, Hoppe-Seyler published his researches for the most part in Virchow's *Archiv*, some of his papers appearing, however, in Fresenius' *Zeitschrift*, in the *Annalen d. Chemie und Pharmacie*, and in the *Berichte* of the Chemical Society of Berlin. In 1866, however, he commenced the publication of the collected papers issuing from his laboratory, under the title of "Med.-Chemische Untersuchungen."[5] Four parts of this publication appeared, the last in 1871.

Hoppe-Seyler's Work in Strasburg, 1872–1895.

A proper estimate of Hoppe-Seyler's work would necessitate a careful review of the fine researches published by his pupils, for there can be no doubt that in his case, as in that of many of the most distinguished scientific men of Germany, the work of the master has often been credited to the pupil under whose name it has appeared. It is obvious, however, that it would be impossible, within the limits of such an article as the present one, to give an account, however brief, of the succession of valuable papers which issued from the new Physiologico-Chemical Institute of Strasburg. Two events in Hoppe-Seyler's scientific life in Strasburg cannot, however, be passed over, viz. the publication of his "Text-Book of Physiological Chemistry," and the foundation of the *Zeitschrift für Physiologische Chemie*. The first part of the "Text-Book of Physiological Chemistry" appeared in 1877, the second in 1878, the third in 1879, and the fourth in 1881. This work is of interest as giving Hoppe-Seyler's views of the chemical processes of the body; yet it neither achieved nor merited great success. Devoted though he was to work by which he unquestionably did much to advance both physiology and pathology, Hoppe-Seyler was essentially a chemist rather than a biologist; and when, as in his systematic treatise, he left chemical, to speculate on biological, questions, his weak points became very obvious.

This account of Hoppe-Seyler's work must close with a reference to the great service which he rendered to our branch of science by founding, in 1877–78, the *Zeitschrift für Physiologische Chemie*. From the first number to the last this periodical has maintained a high standard, and, besides containing the results of all the work done in the Strasburg Laboratory, it has received contributions from nearly all the prominent workers in physiological chemistry. In succession to Hoppe-Seyler, Professors Baumann and Kossel are, it is understood, to be the future editors of this journal.

Arthur Gamgee.

NOTES.

We are informed that a biography of Prof. Huxley is being prepared by his son, Mr. Leonard Huxley, who will be greatly obliged if those who possess letters or other documents of interest will forward them to him at Charterhouse, Godalming. They will be carefully returned after being copied.

The Committee of the Pasteur Institute have appointed Dr. Duclaux, formerly sub-director, to succeed M. Pasteur as director, and Dr. Roux to be sub-director of the Institute.

We understand that the final interment of M. Pasteur in the Pasteur Institute will not take place on Friday, as had been intended, because the vault and part of the sculpture cannot be ready in time.

The centenary celebrations of the Institute of France commenced as we went to press yesterday, and will terminate on Saturday by a visit to the fine chateau of Chantilly, where the associates and members will be received by the Duc d'Aumale. An account of the foundation and membership of the Institute appeared in these columns a few weeks ago, and we hope to give in our next issue a full description of the ceremonies now taking place.

A bronze portrait bust of Dr. Robert Brown was unveiled on Friday in his native town, Montrose, Forfarshire at a reception held by the Provost, magistrates, and town council of Montrose. Beneath the bust is a tablet, with the following inscription:—"Robert Brown, D.C.L. Oxon., LL.D. Edinburgh, F.R.S. London, President of the Linnean Society, Member of the Institute of France. Born in this house 21st December, 1773; died in London 10th June, 1858. 'Botanicorum facile princeps,' Alex. Von Humboldt.'" A large number of distinguished botanists from all parts of the kingdom were present.

[1] Prof. Stokes, F.R.S., "On the Reduction and Oxidation of the Colouring-matter of the Blood." *Proceedings* of the Royal Society, vol. xiii. (1864) p. 357, paragraph 8.

[2] Hoppe-Seyler, "Erkennung der Vergiftung mit Kohlenoxyd." Fresenius' *Zeitschrift*, vol. iii. (1864) p. 439. *Philosophical Magazine*, vol. xxx. (1865) p. 456.

[3] "Um Verwechselungen zu vermeiden nenne ich den Blutfarbstoff Hämatoglobulin oder Hämoglobin," Virchow's *Archiv*, vol. xxix. (1864) p. 233.

[4] Hoppe-Seyler's "Beiträge zur Kenntniss des Blutes des Menschen und der Wirbelthiere"; "Med-Chem. Untersuchungen," pp. 169-214, 366-385, 523-550; "Zur Chemie des Blutes und seiner Bestandtheile," *ibid.*, pp. 29-300; Dybkowsky, "Einige Bestimmungen über die Quantität des mit dem Hämoglobinlose gebundenen Sauerstoffs," *ibid.*, p. 117-132.

[5] "Medicinisch-Chemische Untersuchungen aus dem Laboratorium für angewandte Chemie zu Tubingen herausgegeben, von Dr. Felix Hoppe-Seyler." Berlin, 1866.

Mr. P. H. Lawrence, whose name will be remembered by some students of mineralogy, but more widely in legal circles, died a few days ago. We have also to record the death of Prof. E. W. Blake, until lately professor of physics in Brown University; of Dr. E. E. Rogers, instructor in chemistry at Harvard University; of Prof. A. Rydberg, the Swedish archæologist; of Mr. H. W. V. Stuart, who devoted much attention to the study of Egypt and its monuments; of Father Hirst, the author of numerous contributions to archæology; and of Dr. F. M. Stapff, the geologist, while prospecting for gold in East Africa.

The sixth Congress of Medicine was opened at Rome on Tuesday by Dr. Baccelli, Minister of Public Instruction.

In addition to the papers already notified in the usual way, to be read at the next meeting of the London Physical Society to-morrow, there will be read, if time permits, a paper "On the Magnetic Field of any Cylindrical Coil or Plane Circuit," by Mr. W. H. Everett.

The steamship *Windward*, which conveyed the members of the Jackson-Harmsworth Polar expedition to Franz Josef Land, arrived at Gravesend on Tuesday. It will be remembered that the *Windward* left the Thames in July 1894; she has brought back the records of the expedition from that date up to the beginning of July of this year. Mr. Jackson and his party remain in Franz Josef Land, and the vessel will return there, with stores, next June.

A Fine Art, Industrial and Maritime Exhibition will will be held in Cardiff in the spring and summer of 1896, under the patronage of Her Majesty the Queen. The general object of the exhibition is to illustrate the most recent progress in the sciences, arts, and manufactures, and not merely to be a popular show. The following is a list of the chief sections, and the number of square feet allotted to each:—Mining and mining appliances, 13,280; machinery, electricity, and local and general industries, 20,480; maritime, 8400; agriculture and horticulture, 7280; health, 5400; fine arts, 6600.

The annual exhibition of the South London Entomological and Natural History Society was held on Thursday last, and was much appreciated by the company who went to see the numerous interesting specimens arranged by the Committee. The Society has for its object the popularising of the study of natural history, and to promote this it holds bi-monthly meetings, at which papers are read, discussions take place, observations are communicated, and specimens shown and commented on. In the summer time field meetings are held for the purpose of collecting and observing, and periodical exhibitions are promoted. The Society's rooms are at Hibernia Chambers, London Bridge, where a large library and typical collections are kept for members' reference, as well as a lantern for demonstration purposes. At present the number of members is about two hundred. The Secretary is Mr. Stanley Edwards, Kidbrooke Lodge, Blackheath, S.E.

Mr. D. Pidgeon, Letherhead, sends us an account of a curious effect apparently produced by lightning in the early morning of September 7. In a cottage on Cherkley Court estate, three or four tumblers were left standing over-night, mouth upright, on a shelf affixed to the wall of a small pantry, and about twelve inches from the window, which was open. In the morning one of these tumblers was found to have a crack completely round it, so that a ring of glass, having an uniform width of half an inch, could be cleanly and easily detached. This hoop of glass has been preserved to be a witness to the vagaries of electrical discharge. There seems little doubt that electricity had to do with the formation of the crack, for large shrubs, just outside the open window near which the glasses stood, were found to have been damaged by the lightning. It would be interesting to know whether the glass was empty or not, or whether it was wet up to the level of the crack.

The Harveian Oration was delivered on Friday last, at the Royal College of Physicians, by Dr. W. S. Church, who took for his subject "Harvey and the Rise of Physiology in England." For 239 years, with but few intermissions, the College has met in obedience to Harvey's direction to commemorate its benefactors. After referring to the long list of these, Dr. Church remarked that during the present year the College had received the magnificent endowment of £3000 to establish a triennial prize for the furtherance of original research on the prevention and cure of tuberculosis, the donor being Dr. Hermann Weber, who, in instituting the prize, joined the name of the late Dr. E. A. Parkes with his own. After the delivery of the oration, the Baly medal was presented by the President, Sir Russell Reynolds, to Dr. W. H. Gaskell, F.R.S., of Cambridge. The medal is awarded biennially to some person who has distinguished himself in the science of physiology; it was founded in 1866 by Dr. F. D. Dyster, "In Memoriam Gulielmi Baly, M.D.," and amongst the names of those who have since received it are those of Claude Bernard, Carl Ludwig, Darwin, Owen, Kitchen-Parker, and Brown-Séquard.

In connection with the proposal to change the name of the Boulevard de Vaugirard to Boulevard Pasteur, the Paris correspondent of the *Chemist and Druggist* points out that a Rue Pasteur already exists, while twenty-one other streets of Paris have been named after chemists. Of these fourteen were of French nationality, and include Chevreul, Gay-Lussac, Lavoisier, Raspail, &c. Davy figures as the sole English chemist, and the only other foreigner is the Swede Berzélius. The names of seven botanists appear on street corners, amongst which are Dupetit-Thouars, Jussieu and Linné. Nicholas Flamel, writer and alchemist, who flourished in the second half of the fourteenth century, has the distinction of being the most remote name connected with sciences after which the Parisians have called a street. Thirty-nine thoroughfares take their names from doctors and surgeons; amongst these figure Jenner and Vesale, the Belgian anatomist, the only two foreign names. We commend the French custom to English and municipal authorities at a loss for suitable street names. It may be thought a doubtful honour to have one's name handed down to posterity in this manner, but the custom serves to show that men of science are remembered in France in little as well as in great things.

The following statistics, from the *Zoologist*, with reference to the progeny of a female Manx Cat and an ordinary Tom Cat, are interesting. The successive litters consisted of three on each occasion, and the distribution of tails is shown in the table:—

	No tails.	Half tails.	Full tails.
1st litter	3	0	0
2nd ,,	2	1	0
3rd ,,	1	2	0
4th ,,	0	2	1
5th ,,	0	1	2
6th ,,	0	0	3

The gradual elimination of the tailless condition characteristic of Manx cats is singular, and well worth putting on record.

Very little detailed information exists as to the effect of wind and atmospheric pressure on the tides around the British Isles, but it is to be hoped that the Committee appointed at the recent meeting of the British Association will succeed in eliciting sufficient trustworthy data to enable some general law to be deduced for the guidance of navigators. The Committee consists of Prof. Vernon Harcourt, Prof. Unwin, Mr. G. F.

Deacon, and Mr. W. H. Wheeler (Secretary); and as it is desirous of obtaining information from as many ports as possible, we are asked to make its existence known. A printed form, showing the manner in which it is proposed to collect the tidal statistics, will be sent to any one who will render assistance to the Committee, by Mr. W. H. Wheeler, Boston, Lincolnshire, who will also be glad to receive records of tides affected by gales.

In connection with the growth of orchids, writes Mr. J. H. Hart, in the October *Bulletin* of the Royal Botanic Gardens, Trinidad, it has been noticed that the presence of ants is apparently necessary to their maintaining a healthy condition; but whether this is in reality due to some action of the ant itself, or to some indirect cause, has not yet been proved, and investigations are needed to show what is the real influence the ant has upon the health of the plant. It has been suggested that the presence of stinging ants acts as a protection to the plants; but Mr. Hart is inclined to think, from recent investigations, that the benefit the ants confer on the plant are those of providing it with the mycelium of a fungus to cover its roots, which organism enables it to take up food which would be otherwise unattainable. It may be shown that the ants act as protectors to the plants, as well as providing them with a means of obtaining nutriment; but Mr. Hart believes it to be almost certain that the fungus which grows in the material they accumulate around the root plays a much more important part, by providing the plant with food material.

The first number of what promises to be a useful serial publication has just reached us from the U.S. Weather Bureau. The periodical has for its name *Climate and Health*: it is edited, under the direction of Prof. W. L. Moore, the new chief of the Weather Bureau, by Dr. W. F. R. Phillips, and it is devoted to climatology in relation to health and disease. Tables are given showing, for one hundred selected stations, statistical information relative to atmospheric pressure, temperature, humidity, precipitation, wind, and sunshine; the relative prevalence of certain diseases; and the mortality from different causes, in each State. In addition to these statistics, all of which refer to the conditions during July of this year, the new publication contains charts showing the average pressure departures from the normal, ranges of pressure, prevailing winds, and normal wind directions for each week in the month, and similar charts to exhibit graphically the absolute and relative data referring to temperature, humidity, and precipitation. There is also a chart for each week showing the total mortality by States, and representing diagrammatically the average climatological conditions so far as determined by the mean temperature and humidities and the total amount of precipitation. The general aim of the Weather Bureau in this new field of work is to collect the meteorological and hygienic statistics considered by medical climatologists of the greatest correlative importance, and to publish them in a useful and instructive form. By showing the statistics of mortality and morbidity side by side with those of climate, new information as to connections between sickness and weather changes will probably be discovered.

The *Psychological Review* for last month contains an interesting paper by Mr. R. Meade Bache, on "Reaction Time according to Race." He suggests that the higher intellectuality of civilised white races may have been gained at the sacrifice of quickness of response to sensory stimuli, and states that it is a matter of familiar observation that Negro children are quicker in their movements than the children of white folk. At his request Prof. Lightner Witmer made careful and exact observations on persons of the Caucasian, American Indian, and African (Negro) races. These are given in three tables. Taking response to auditory stimuli, for example, the order of quickness is (1) Indian, (2) African, (3) Caucasian, in the relation of 116·27 : 130 : 146·92; these being the reaction times in thousandths of a second. Although the numbers of individuals dealt with (not more than a dozen in each case) are small, the results are suggestive, and will no doubt lead to further investigation.

The attention of those who are interested in the question of the inheritance of acquired characters may be drawn to a paper which Prof. Mark Baldwin contributed to *Science* (August 23, 1895), under the title "Consciousness and Evolution." Prof. Baldwin fails to see any great amount of truth in the claims of Mr. Spencer that intellectual progress in the race requires the hereditary transmission of acquired increments in mental faculty, and adopts the view advanced by Weismann in 1889, and taken up more or less independently by Mr. Ritchie and Mr. Kidd, that social advance is rather by tradition than by hereditary transmission. "Man," said Prof. Weismann, "availing himself of tradition, is able, in every part of the intellectual domain, to seize upon the acquirements of his ancestors at the point where they left them, and to pursue them further, finally himself leaving the results of his own experience and the knowledge acquired during his lifetime to his descendants, that they may carry on the same process." Prof. Baldwin seems to have reached this view independently, and his paper is well worth reading.

Under the extraordinary heading of "The Chemical Theory of Freedom of Will," Dr. W. Ostwald makes, in the *Leipziger Berichte*, some suggestive speculations upon the mechanical theory of the universe. That all the phenomena of nature, organic as well as inorganic, should be ultimately of a purely mechanical character, is contradicted by the science of energy. The theorems of energetics give the conditions under which any event takes place: they indicate which out of all the possible courses it will follow, and to what state of equilibrium it tends. All this does not involve the element of time, except in the case of kinetic energy. In the equations representing mechanical processes, time may be put as positive or negative without rendering them invalid. In other words, all purely mechanical processes are reversible, while natural processes are not. They have a forward and a backward aspect. Now there are processes in nature in which an agent influences the time during which a certain event takes place, without being itself affected in any way. This happens in all cases of catalysis, and the laws of catalytic action are as yet only very imperfectly understood. It is known, however, that the acceleration of the process is proportioned to the concentration of the catalyser. May not the human mind, the author argues, act upon matter somewhat in the manner of a catalyser, accelerating the chemical and mechanical processes associated with psychical activity without any expenditure of energy? This may be worth considering. But it must be remembered that the course of natural phenomena can be influenced in many ways without the expenditure of energy. An elastic missile rebounding from a rigid plane is a case in point, or a river flowing between its banks.

The production of antiseptics appears to be more and more engaging the attention of the great German colour manufacturers, and yet another compound, rejoicing in the name of potassiumorthodinitrocresolate, has been introduced, which promises to prove of considerable service both to the brewer and to the horticulturist. Messrs. C. O. Harz and W. von Miller have published an account of their investigations with this substance—or *antinonnin*, as it is more generally called—in the *Muenchen Allgemeine Zeitung*, and it appears that a

solution containing but one part in 1500 to 2000 parts of soap-water proves destructive to all common injurious parasites without any deleterious action on the plants. Prof. Aubry, the well-known director of the experimental brewing station in Munich, has examined its disinfectant action on yeast, and finds that the latter, when treated with antinonnin, remained for a long time in a fresh condition in the heated workrooms, whilst untreated yeast rapidly underwent decomposition. A closer examination showed that all the specimens exhibited destruction of bacteria, while the yeast itself proved resistant to even stronger solutions, up to 5 per cent. Numerous other experiments have been made with this substance, and so far it promises well, being also odourless and very inexpensive. Whether this new antiseptic will succeed in carrying out all that is hoped of it, remains to be seen; meanwhile it may be regarded as an interesting, and possibly important, contribution to our list of disinfectants.

MESSRS. MACMILLAN AND CO. will issue in the course of November a further instalment of their "Cambridge Natural History." The volume is mainly devoted to insects, being the first part of a complete treatise on the subject by Mr. David Sharp, F.R.S. Introductory sections on Peripatus and on Myriapods are contributed respectively by Mr. Adam Sedgwick, F.R.S., and by Mr. F. G. Sinclair. The volume is the fifth in the series, and will be followed at no long interval by the second volume, in which various contributors deal with worms and Polyzoa. The ninth volume, in which Mr. A. H. Evans treats of birds, may be expected before the end of next year. Among Messrs. Macmillan's announcements for next week, one of the most important is that of an exhaustive work on "The Structure and Development of the Mosses and Ferns" (Archegoniatæ), by Dr. D. H. Campbell.

WITHIN the past few days, a bulky bundle of new publications of the U.S. Geological Survey has been added to the many reports and memoirs of the Survey already lying on our table. The amount of work represented by these volumes is so exceedingly great, that limits of space prevent us from attempting to describe and discuss the ground covered in them. We propose, however, to give in an early issue a general account of the recent publications of the Survey, and content ourselves at present with the bare statement of the volumes received during this month. First of all, we have to acknowledge the receipt of the fourteenth annual Report of the Survey, in two parts. Part 1 contains the report of Mr. J. W. Powell, the Director, on the operations of the Survey for the year ending June 30, 1893, and part 2 (a volume of six hundred pages) contains papers on geological subjects, among which we notice—the potable waters of Eastern United States; the natural mineral waters of the United States; measurements of river discharges; the laccolitic mountain groups of Colorado, Utah, and Arizona; the gold-silver veins of Ophir, California; geology of the Catoctin Belt; tertiary revolution in the topography of the Pacific Coast; the rocks of the Sierra Nevada; pre-Cambrian igneous rocks of the Unkar Terrane, Grand Cañon of the Colorado. Two monographs of the U.S. Geological Survey have been received, viz. vols. xxiii. and xxiv. The former deals with the "Geology of the Green Mountains in Massachusetts," by Messrs. R. Pumpelly, J. E. Wolff, and T. Nelson Dale; and the latter contains Prof. R. P. Whitfield's text and drawings of the Mollusca and Crustacea of the Miocene formations of New Jersey. Both these valuable monographs are profusely illustrated. Finally, *Bulletin* Nos. 118-122 of the Survey have come to hand. No. 118 is a geographical dictionary of New Jersey. The next *Bulletin* contains the report of a geological reconnaissance in North-west Wyoming, with special reference to economic resources; No. 120 is on the Devonian system of Eastern Pennsylvania and New York. No. 121 is a bibliography of North American palæontology for the years 1888-92, inclusive; and No. 122 contains the results of the primary triangulation executed by the Survey during the past twelve years—that is, since the commencement of work upon the topographic atlas of the United States. In conclusion, we wish only to remark that the gratitude of geologists is due to the United States Government for providing the funds to publish so many works, not only of national but also of international importance.

THE current number of the *Journal de Physique* contains a paper by MM. Abraham and Lemoine on the measurement of very high potentials by means of a modified attracted disc electrometer. Two forms of instrument are described, the one for standard measurements, and the other, which is of simple design, intended for measuring potentials up to 100,000 volts to within about one per cent. In the standard instrument, which resembles a modified Kelvin electrometer as designed by M. Baille, the movable disc is suspended from the beam of a short-beam balance, the extent of the movement being limited by stops. In order, when desired, to make the movement of the balance beam stable, an auxiliary knife-edge is placed below the chief knife-edge of the beam, and weights are placed in a pan suspended from this auxiliary knife-edge. The attracted disc is maintained centrally within the guard-ring by means of three fine fibres. The simplified form of electrometer is, however, the one which exhibits most novelty. In this instrument the attracted disc is carried by a rod attached to one arm of a Roberval's balance. The movements of the balance, which is limited by stops, is noted by means of a long pointer attached to one of the horizontal bars of the moving parts. Finally, the adjustments of the guard-ring and attracted disc are not made by means of a complicated system of adjusting screws, but by the simple bending of their supports. These supports are made of soft copper wire, and, in the case of the guard-ring, have an S shape. This manner of allowing for the adjustment of the parts of a piece of apparatus is one which will very often be found of use, and we may mention that lead wire is particularly well suited for the purpose. The authors have made a series of experiments to test what is the maximum distance between the attracted and attracting discs it is allowable to use, and find that the greatest distance to be equal to half the width of the guard-ring. In making their measurements, the authors have used a novel method of obtaining a high potential which should remain steady for some minutes. Their arrangement consists of an electrostatic electric machine driven at a uniform speed by a small motor. The poles of the machine are joined to two points, between which a continuous stream of sparks passes. One of these points is connected to earth, and the other by means of a poor conductor, such as cotton soaked in paraffin oil, to the inner coating of a Leyden jar. Under these circumstances it is found that the potential of the interior coating of the jar is very constant. Thus in a series of measurements recorded by the authors, the maximum change in six minutes amounted to only 1 part in 1000, the potential being about 20,000 volts.

THE additions to the Zoological Society's Gardens during the past week include a Mozambique Monkey (*Cercopithecus pygerythrus*, ♀) from East Africa, a Smith's Dwarf Lemur (*Microcebus smithi*) from Madagascar, presented by Mr. E. Dyer; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mrs. Vernon Biden; a Polar Bear (*Ursus maritimus*, ♂) from Spitzbergen, presented by Mr. Arnold Pike; two Masked Parrakeets (*Pyrrhulopsis personata*) from the Fiji Islands, a Blue and Yellow Macaw (*Ara ararauna*) from South America, a Peregrine Falcon (*Falco peregrinus*, var. *Anatum*) from North America, a Night Heron (*Nycticorax*

griseus), European, an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, presented by the Hon. Walter Rothschild; two Senegal Touracous (*Corythaix persa*) from West Africa, presented by Mr. I. J. Roberts; three Blackcaps (*Sylvia atricappilla*), a Nightingale (*Daulias luscinia*), British, presented by Mr. Poynter; a Wall Lizard (*Lacerta muralis*) from Sicily, presented by Mr. A. M. Amster; a Dwarf Chameleon (*Chamæleon pumilus*) from South Africa, presented by Mrs. S. Jackson; two Squirrel Monkeys (*Chrysothrix sciurea*) from Guiana, a Spotted Eagle (*Aquila nævia*) from India, three Weka Rails (*Ocydromus australis*), four Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, deposited; two Grisons (*Galictis vettata*), a Coypu (*Myopotamus coypus*) from South America, two Western Boas (*Boa occidentalis*) from Paraguay, purchased.

OUR ASTRONOMICAL COLUMN.

SUN-SPOT OBSERVATIONS IN 1894.—In a *Separatabdruck aus der Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich*, Jahrgang 4, 1895, Dr. A. Wolfer brings together some results relating to the sun-spot statistics made in Zürich and elsewhere for the year 1894. The pamphlet opens with a determination of the constants for reducing the observations of each observer to one scale.

The mean observed relative number of spots for 1894 came out as 78·0 as against 84·9 in 1893, showing a distinct decrease. The secondary variations were also very prominent during this year; further, between two very low minima there occurred a prominent maximum lasting from May to July. Nevertheless there was on the whole a general decrease, making it possible to determine the epoch of the last important maximum. Having plotted the relative number of observed sun-spots for the three years 1892–94, and connected them together, the smoothed curve indicated a maximum at 1894·0. The length of the elapsed period, that is, from maximum to maximum, became

$$1894{\cdot}0 - 1883{\cdot}9 = 10{\cdot}0,$$

and the interval between the last minimum and the present maximum

$$1894{\cdot}0 - 1889{\cdot}6 = 4{\cdot}4.$$

Dr. Wolfer makes a comparison of the sun-spot numbers with the variations of the magnetic declination. Here there seems to be a very good agreement, and the curves for both are very similar. The epoch of the maximum magnetic variation, independently determined, occurs in August 1893 or 1893·6, which coincides exactly with the secondary rise of the curve of relative spot numbers. This secondary rise in the curve occurs just before the time of maximum deduced from the smoothed curve, and suggests rather that the former date should represent the chief sun-spot maximum. Dr. Wolfer, however, is not of this opinion, and prefers to hold to the date gathered from the mean curve. The pamphlet concludes with a tabular statement of each of the observers' individual observations for the year 1894, together with reference to the literature.

PLANETARY PERTURBATIONS.—In No. 3312 of the *Astronomische Nachrichten*, Prof. A. Weiler gives another paper on the subject of long-period and secular perturbations. The particular case considered is that of the disturbance of a planet, having a mean motion approximately twice that of the disturbing planet, and is really a special case of the more general problem of perturbations already treated in earlier numbers of the same journal. We cannot indicate here the mathematical formulæ which are given, and much of which would be unintelligible without the earlier papers, but attention may be called to one of his results.

When the commensurability in the periods of the disturbed and disturbing planets becomes very close, that is if $\delta = 1 - 2\mu$ be very small, where μ is the ratio of the two mean motions, the series by which the perturbations are expressed is not convergent, and the problem is apparently insoluble. Such a result is inconsistent with the regularly observed motions of the planets, and therefore points to some error in the assumptions on which the solution of the problem is founded. This error Prof. Weiler traces to the treatment as constant of the semi-axis major of the disturbed planet's orbit. The justice of this remark is illustrated by a reference to the arrangement of the asteroids in space, whose distribution offers peculiarities explicable on the hypothesis that the mean daily motion is variable if the approximation to commensurability oversteps a definite limit. Taking a list of twenty-five asteroids, wherein the value of $\delta = 1 - 2\mu$ is less than one-fifteenth, he shows that none have a period giving a mean daily motion very approximately twice that of Jupiter (598″·3). The mean daily motion of these twenty-five lies between 562″·2 and 640″·2, but none come between 572″·6 and 614″·4; that is, the mean motions separate on both sides of twice that of Jupiter. The force of this illustration is somewhat impaired if the list be made to comprise those more recently discovered. The asteroids Nos. 332 and 381 have mean motions of 605″·5 and 613″·5, respectively, and it should further be remembered that in the whole list of asteroids, there are only five whose means approach the lower limit of 562″. This remark simply refers to the value of the illustration, not to the accuracy of the fact it is called in to support.

THE SYSTEM OF α CENTAURI.—The meridian measures of the positions of α_1 and α_2 Centauri, made at the Cape in 1879–1881 have been utilised by Mr. A. W. Roberts for a determination of the relative masses of the two stars, and other data connected with the system (*Ast. Nach.* No. 3313). The place of the centre of gravity for 1880 is given as R.A. 14h. 31m. 27·537s., declination − 60° 20′ 20″·63 ± 0″·13; proper motion in declination (1880) = + 0″·750 ± 0″·005; proper motion in R.A. (1880) = − 7″·291 ± 0″·032. For the relative masses of the two stars, the values derived are 51 to 49 ± 1/50 of the amount.

According to the results obtained by Mr. Roberts, α_2 Centauri is very slightly heavier than the sun, while α_1 is about two-hundredths lighter. Since α_2 is now between five and six times brighter than α_1, it must have by far the brighter surface. Taking a mean of the different values which have been obtained for the sun's brightness in relation to the stars, "it would appear that α_2 Centauri is as bright as our sun, while α_1 is about five times fainter. α_1 Centauri is accordingly some distance on the downward track from the dignity of a sun to that of an ordinary planet; while α_2 Centauri is, as regards light, size, and mass, a twin-brother of our sun." Spectroscopic observations will furnish another method for determining the relative masses, but, in order to improve on our present knowledge, the observations of velocities must be accurate to within one or two tenths of a mile per second.

HOLMES' COMET.—This comet, which has presented such peculiarities both in its physical structure and the form of its orbit as to make it one of the most remarkable comets of short period, has been made the subject of an elaborate investigation by Dr. H. J. Zwiers. Taking into account the action of Jupiter and Saturn, but neglecting that of the Earth, to which, owing to the great perihelion distance of the comet, it cannot make any close approach, Dr. Zwiers is led to fix the date of the next perihelion passage on April 27, 1899, and gives an ephemeris commencing on February 16, 1898, the earliest date at which a search is likely to be successful. The theoretical brilliancy is then 0·0063, and when last seen in 1893, the brilliancy was expressed by 0·0118. In April and May, when the comet will be well situated for observation in the southern hemisphere, this latter quantity will be exceeded, and will approach that, that the comet possessed in January 1893, when it underwent such a remarkable change in its appearance. If the comet retains its stellar-like character, the difficulty in detection will no doubt be increased, but an early discovery is eminently desirable.

ON THE HABITS OF THE KEA, THE SHEEP-EATING PARROT OF NEW ZEALAND.

THE kea, the mountain parrot of New Zealand (*Nestor notabilis*), has earned considerable notoriety from its remarkable habit of attacking living sheep. It is commonly stated that the natural food of this bird consists of insects, fruit, and berries; and that it has developed a taste for a carnivorous diet only during the last thirty years. Mr. Taylor White, however, has recently pointed out (*Zoologist*, August 1895) that the various statements on the habits of this bird have all been derived from second-hand information; and, as the habitat of the parrot is on the tops of Alpine ranges, owners of sheep and shepherds who

[illegible] in summer search the mountain tops for their stock, are the men best fitted to tell us about the habits of the bird. On observations made during such experiences Mr. White bases his own account. In the district with which this writer was acquainted, the kea always lived high up on the mountains, among rocks and boulders, a long distance above the forest-line: in such a situation, of course, berries and fruits were out of the question, and the bird appeared to live on lichen and any insects it could find. Even when the ground was covered with several feet of snow, and when roots and other food were out of reach, lichen growing on steep rocks would still be obtainable by the bird. The view that the diet of the kea generally consists of fruit and berries would thus appear to be erroneous.

It will be remembered that Wallace and others state that the kea regards the kidneys of sheep as a "special delicacy," and that it attempts to burrow into its victim in such a way as to reach this part. Mr. White, however, opposes this prevalent view, and regards it as probable that the bird desires to obtain the blood of the sheep rather than the kidneys; and in support of this view states that he has never seen a dead sheep attacked by keas. The fact that the kea so frequently pierces the body of a sheep in the region of the kidneys is due to the position it takes on the back of its victim to maintain a firm hold—a position from which it cannot be easily dislodged, as it could from the head or rump of the sheep. In corroboration of this Mr. White mentions that sheep with long wool are more frequently attacked than animals with short wool; as apparently the long wool gives the bird better facilities for holding on with his feet when drilling a hole into the back of the sheep. It is not very easy to conjecture how this habit of attacking sheep was first acquired by the kea. In winter time the sheep are covered with snow, and often have icicles hanging to their wool; and it is suggested by Mr. White that keas may have mistaken sheep so disguised for snow-covered patches of rock. It may further have happened that when searching the supposed rocks for insects the birds in some cases would taste the blood of the sheep. "When some of the birds had once found out that the blood of the sheep was good for food, others were soon initiated into the performance." It is possible that in some such manner the kea may have gradually acquired this curious and unattractive habit which renders the bird such a pest to the New Zealand farmer.

W. Garstang.

THE PENETRATION OF ROOTS INTO LIVING TISSUES.

THE capacity possessed by the roots of certain parasites, such as *Cuscuta*, to penetrate into the tissues of their host, is apparently an unique, not to say a remarkable phenomenon. A little reflection, however, upon the powers of roots in general, leads us to doubt whether this property is really as restricted as the first glance would lead us to imagine; and when we peruse Prof. Pfeffer's work upon the pressure of the root, and find that, for instance, the root of the common bean exerts during its growth a pressure of some 400 gms., we realise that this mechanical action alone might suffice to drive the growing root of most plants into living tissue, if circumstances necessitated such an expediency. This is evidently an important point, and touches upon the evolution of the higher parasites; it is only remarkable that it has so long remained untouched. We must now thank George Peirce for taking up this neglected [illegible], and placing it upon a sure basis (see *Bot. Zeit.* September 1894). The question first to be decided was whether the pressure which Pfeffer had found in the growing roots was in itself sufficient to force the roots through living tissue. For the determination of this, iron models of roots weighted up to 27[illegible] gms. were employed. The apices of these were placed upon a cube cut from a potato, and the whole surrounded with damp sawdust to keep the living substance fresh. After an interval of twenty three hours, it was found that the iron point had penetrated 1½ mm. into the potato. Again, a similar model weighted to 320 gms. was driven in twenty-four hours through the cork layer and 2 mm. of parenchyma of an uncut potato. Also a root model placed on the stem of *Impatiens sultani*, one and a half centimetres thick, pierced this in less than twenty hours when 300 gms. weight were employed.

Thus a pressure inferior to that found by Pfeffer in the root of *Vicia faba* was sufficient to drive an iron model an appreciable distance through the living tissues of the potato.

It was far from certain, however, whether a pressure which was ample to impel a rigid iron rodlet against a considerable resistance would have equal efficiency in the case of a root, the pressure in which arose from so uncertain and inextricable a source as its life.

There were many facts both *pro* and *contra*.

The acid substance or substances, which it would seem that most roots excrete during their growth, might possibly facilitate the root's power of penetration. Just as many fungi eat their way, as it were, into the solid wood of their host by means of ferment-like substances which they secrete and pour out upon their substratum, so might the roots perhaps be expected to soften and prepare their way by means of their acid excretions. Against the supposition could be raised the fact, already broached, that the forces, impelling the root-apex forward, are derived from the vital activities of that structure, and than these nothing can be more sensible to change of surroundings, or less to be reckoned upon by us, whose conceptions of anything dealing with life are yet shrouded over with the darkest obscurity.

But to pass from speculation to facts, we find that Peirce tested this point by experiments on the seedlings of *Brassica napus* and *Sinapis alba*. He took a potato, and split it in half; on one of the halves he cut a number of small slits, into each of which he inserted a seed of the plant under observation. He then placed the potato-halves together, binding them tightly with string. The whole contrivance was placed in a vessel containing damp sawdust, care being taken that the cut surfaces of the tuber lay horizontally. After an interval of twelve days the specimens were examined, and although some were found to have grown between the cut surfaces (for nearly all had germinated), yet others had pushed their rootlets vertically downwards so as to penetrate the substance of the potato. In some instances so vigorous had been the growth that the rootlet had traversed the whole thickness of parenchyma, pierced the hard corky layer of the surface, and then reached the sawdust without.

Anatomical examination of the root and surrounding potato tissue showed several peculiarities. In the first place, the young root was almost devoid of the customary clothing of hairs: secondly, the cells of the potato had undergone alteration, inasmuch as those which were in immediate contact with the advancing root were much contorted and torn, whilst two or three layers neighbouring on the injured elements had undergone division by walls parallel to the long axis of the root, and had subsequently become corky in nature. By this means the intrusive rootlet was enclosed within a corky cylinder or sheath, cutting it off more or less perfectly from the living, unharmed tissue of the tuber. The starch grains were in every case unaltered, but Prunet, in his research on *Cynodon*, and Peirce, in his examination of one of his specimens of *Pisum*, noticed certain grains in the neighbourhood of the root apex which were partially disintegrated. This, however, is not a necessary consequence of ferment action; indeed, a check experiment of Peirce's leaves little doubt that the disintegration results in these cases from the activities of bacteria which had gained an entrance with the root. Glass tubes closed and pointed at one end were sunk, like the iron models already mentioned, into potato tissue. In one instance the apex of the glass was surrounded by "corroded" starch-grains. Here there could be no question of ferment formation, and evidently bacteria were adherent to the apex.

"So far the experiments had proved that the thin, delicate, and pointed roots of rape and white mustard are able to penetrate living tissues. Peirce carried the matter further by testing the powers of the blunt rootlets of *Pisum* and *Vicia faba* to do likewise. The rootlets of germinating seeds of these were placed in glass tubes into which they accurately fitted, and their apices placed in contact with the surface of a cube of potato. The seed and glass tube were rigidly held by layers of gypsum, in which a gap was left for the extension of the plumule. The whole was kept moist by damp sawdust. After three days the roots were found to have pierced the living tissue to the extent of 7.5 mm.

Other experiments were made on the same plants in which other tissues, such as stem of *Impatiens sultani*, leaves of Echevaria and Aloe, petioles of Rheum, &c., were substituted for the potato. These also were penetrated by the rootlets.

In some instances, however, such as leaves of Aloe and petioles of *Rheum officinale*, the pabulum was evidently unsuited to the healthy existence of the root, for after a short

period of growth the apex of this organ became more or less spherical, and finally withered away.

Similar results had been obtained with the haustoria (modified roots) of Cuscuta, in a former research of George Peirce's.

Another interesting achievement of the same worker was to grow specimens of Pisum as parasites upon other plants, from the seedling stage until flowering. The host which gave the most favourable results appears to have been *Impatien[s] [S]ultani*.

The young Pisum grown under these unwonted conditions produced an almost normal root system, with numerous side branches; but the stem was stunted in its growth, although it bore leaves and a few flowers. The roots, it may be mentioned, were here also devoid of hairs. This experiment is extremely interesting in a great many ways. It shows, in the first place, how fine is the line of demarcation between an ordinary earth-grown plant, such as Pisum, and a phanerogamous parasite, especially a partial parasite like mistletoe.

Again, it has a physiological interest: it is suggestive of a new path of research. A strict and careful comparison of the details of outward form and internal anatomy in a normally grown Pisum, or other plant, with those found in one which is, so to speak, an induced parasite, must, beyond all doubt, shed much light upon the relationship between the shape and structure of an organism and its surroundings.

We know but too little of this branch of biology at present. Why an organ should be shaped this way in one individual and that way in another, may indeed be partially answered in some cases; but these instances are few, and the answers are incomplete, to say the least of them.

RUDOLF BEER.

DR. A. SCHMIDT'S THEORY OF EARTHQUAKE-MOTION.

[NOTE. The following pages contain a summary of an interesting but little known paper by Dr. August Schmidt, of Stuttgart. An English translation was prepared by the late Dr. E. von Rebeur-Paschwitz for the *Seismological Journal of Japan*, but arrived too late for publication in the concluding volume of the series. The original being too long for insertion in NATURE, I have condensed it at the translator's request, at the same time adhering as closely as possible to the author's words. The title of the paper is "Wellenbewegung und Erdbeben ein Beitrag zur Dynamik der Erdbeben" (*Jahreshefte de[s] Vereins fur vaterl. Naturkunde in Wurttemburg*, 1888, pp. 248-270). In a later paper (same journal, 1890, pp. 200-232), Dr. Schmidt applies his theory to the Swiss earthquake of January 7, 1889, and the Charleston earthquake of August 31, 1886. C. DAVISON.]

SEISMOLOGISTS assume the propagation of earthquake-waves to take place uniformly in all directions; they regard the coseismal or wave-surfaces as concentric spheres, the rays as straight lines normal to the spheres. This, however, is an entirely unjustified assumption, which certainly facilitates the calculations, but leads to very doubtful results in determinations of the velocity of propagation and of the depth of the earthquake-centre. It is impossible that seismic rays should be straight lines, because the conditions on which the velocity depends undergo change with increasing depth below the surface. Though experimental determinations of the velocity do not agree with the theoretical value $\sqrt{e/d}$, yet it is clear that the velocity must depend on the density and elasticity of the rocks through which the wave is propagated. Now, the modulus of elasticity, owing to increased pressure, must increase with the depth below the surface; and therefore the velocity of the earthquake-wave must also increase with the depth.

As the velocity of propagation increases, the energy of a vibrating particle diminishes; and thus, as is well known to be the case, earthquakes should be less noticeable in mines than on the surface of the earth.

Amendment of Hopkins' Law.—Let us imagine a wave emanating from a deep centre and propagated in all directions. A vertical plane through the centre cuts all the successive coseismal surfaces, as well as the earth's surface. Let us suppose the section of the latter to be a horizontal straight line. The lower parts of Figs. 1 and 2 show the successive positions of the coseismal surfaces from minute to minute. Fig. 1, with its equidistant concentric coseismals and its straight rays, corresponds to the ordinary earthquake theory. Fig. 2, with its excentric coseismals approaching each other as they rise and with its curved rays convex downwards, represents our new theory. The horizontal straight line, dividing the upper part of the figures from the lower, represents the surface of the earth. In both figures, the rays at first appear equally distributed in all directions from the centre; in Fig. 1 they remain so, but in Fig. 2, in order to continue normal to the wave-surfaces, they must diverge at a much more rapid rate below than above, thus becoming convex downwards. Of course, Fig. 2 only represents a special law of increase of velocity with the depth—the velocity is supposed to vary as the depth—but the general character of the figure with its rays convex below remains the same if the law is a different one.

A comparison of the figures shows that in Fig. 2 there is a greater condensation of the seismic rays, and therefore a greater intensity of the shock, in the neighbourhood of the epicentre, and this corresponds better with the effects observed within the area of greatest disturbance.

But more important for our purpose are the sections of the earth's surface contained between two successive coseismals. Each of these sections is a measure of the distance through which the wave appears to progress from minute to minute at the surface. In reality it progresses obliquely from below in the direction of the rays, and the real distance through which it moves is smaller than the apparent one. We can only observe the apparent velocity at the surface. If we have at our disposal a sufficient number of good time-observations, we can draw the horizontal coseismal lines on a map and determine the apparent velocity from their relative distances. In both figures, the apparent velocity has its greatest value at the epicentre and decreases outwards. In Fig. 1, it gradually approaches asymptotically the true value in the direction of the rays. This is the law which Hopkins propounded in 1847. In Fig. 2, the apparent velocity at first diminishes rather rapidly, until it reaches the value of the true velocity at the depth of the centre, but afterwards it again increases gradually with the distance. We thus arrive at the following amendment of Hopkins' law, which will be expanded afterwards: the apparent velocity at the surface is never less than the true velocity at the centre, and varies with it.

Differences in Earthquake Velocities.—According to the old theory, every substance ought to possess its own velocity, dependent on its internal structure. The limit, which is defined by Hopkins' law as the lowest possible value of the apparent velocity, ought always to be the same in any given region. Experiments by Pfaff, Mallet, and Abbot lead to different values for different substances, as was to be expected. But they also show considerable variations in the same material, the velocity increasing with the strength of the initial impulse. Real earthquakes show even larger differences in velocity than artificial ones, and often earthquakes of less intensity are propagated with a greater velocity in the same region than very strong ones.

These differences are inconsistent with Hopkins' law. To be explained by the old theory, they require for the centres of earthquakes with great velocities an enormous depth below the surface, a near approach to the centre of the earth, for an earthquake emanating from the centre itself would arrive simultaneously at all points of the surface. With our new hypothesis, such differences are necessary, and even with the largest velocities the earthquake-centre may be at a considerable distance from the centre of the earth.

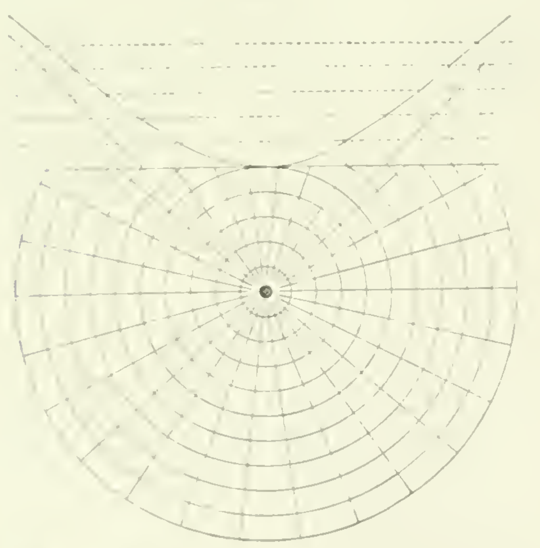
Proof of the Law.—The law that the velocity at the surface is never less than that at the earthquake-centre includes Hopkins' law. This indicates that the law is a general one. Its mathematical demonstration is contained in the law of refraction. We may distinguish the following three velocities: (1) the velocity at the centre, u_1; (2) the true velocity at the surface, *i.e.* that part of an earthquake-ray through which the wave progresses in one minute, u; (3) the apparent velocity at the surface, *i.e.* the horizontal distance between two successive coseismals corresponding to an interval of one minute, v. As an example, let us take in Fig. 2 the horizontal distance between the fourth and fifth coseismals from the epicentre as a representative of v, and let the section of the ray between the same coseismals near the surface represent u, and the distance between the centre and the first coseismal u_1. Then, if α be the angle between the ray and the vertical at the point where it meets the surface, we have $v = u \sin \alpha$; and, if α_1 be the angle which the same ray makes with the vertical through the earthquake-centre, we have by the law of refraction $v = u/\sin \alpha = u_1/\sin \alpha_1$.

Now, let us consider the different rays emanating from the

earthquake centre. When a_1 is equal to zero, v is infinitely great. As a increases, v decreases, until $a_1 = 90°$. This corresponds to the ray which starts horizontally from the centre, and at the point where this ray reaches the surface we have $v = u_1$. When a_1 becomes obtuse, the value of sin a_1 decreases again, and v increases, though more slowly because the rays diverge more and more: but at an infinite distance v would again be infinitely great.

The only condition by which our law is bound is that the true velocity of the wave is always the same at the same depth; but the variation of velocity may follow any law. The law would even remain true if the velocity were to decrease with the depth; but in this case the rays would be concave downwards, and only a few would reach the surface. But, as we have every reason to believe that u increases with the depth, it follows that the rays must be convex downwards; and not only the ray which is horizontal at first bends upwards, but all rays do so in time. The whole disturbed area of an earthquake is thus divided into two zones: an inner circle in which the apparent velocity v decreases as the distance from the epicentre increases, and an outer ring in which v increases with the distance up to infinity. The inner circle is the region of the direct rays, the outer ring that of the earthquake energy which by refraction is brought up from below. The smallest value of v, corresponding to the boundary between the two zones, measures the velocity of propagation at the depth of the centre.

Fig. 1



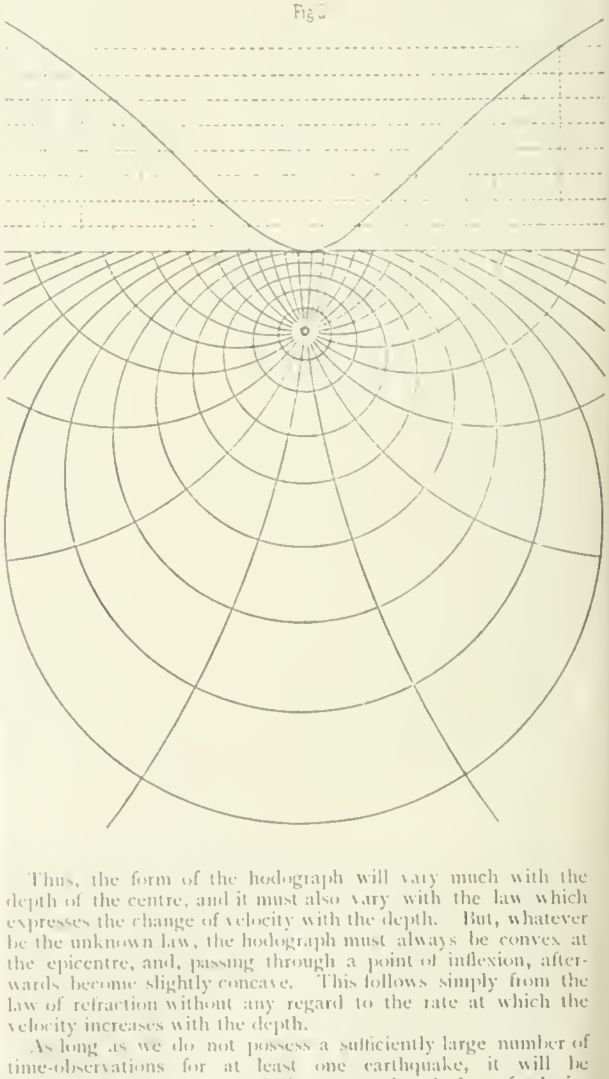
* The [illegible] curvature of the earth's surface, which we have [illegible], will consist in a diminution of the rate at which the velocity increases in the outer zone.

The Earthquake Hodograph.[1] The law connecting the variations in the apparent velocity at the surface is best expressed by the upper parts of Figs. 1 and 2. At the points where the surface line is cut by the coseismals, normals are erected to the surface of 1, 2, 3, &c., units in length, representing the difference in time from that at the epicentre. A curve passing through the ends of these normals represents what we call the hodograph. The greater the inclination of the curve to the vertical line, the less is the apparent velocity, v, at the corresponding point of the curve. Where the curve is horizontal the velocity is infinitely great, where it is convex downwards the velocity decreases outwards, where it is concave the velocity [illegible]. The hodograph in Fig. 1 is the hyperbola of von [illegible] Minnigerode. If we use the same scale for the units of time and velocity, the hyperbola is rectangular and the asymptotes are directed towards the centre. In Fig. 2, the hodograph is no longer hyperbolic; at the epicentre it is horizontal and convex downwards, gradually approaching a [illegible] inclination at a point of inflexion, after which it becomes concave downwards, and gradually becomes horizontal again at infinity. If, in the lower part of the figure, we follow the ray which leaves the centre horizontally until it reaches the surface, a normal erected at this point passes through the point of inflexion.

[1] [illegible] Hamilton to a curve which re[illegible] the [illegible] velocity of a moving point. We do not [illegible] of [illegible] the same for our purpose.

It is important to study the changes in the form of the hodograph as the depth of the centre gradually diminishes. The result is that the two points of inflexion move towards the epicentre, the convex portion becomes smaller, and so also does the "inner zone" of the disturbed area. When the centre and epicentre coincide, the convex portion of the curve and the inner zone of the disturbed area disappear entirely: the hodograph consists of two symmetrical concave branches which meet at an angle at the centre. This suggests to us how we should explain the results of measurements of velocities in artificial earthquakes. In a shock produced at the surface of the earth, the velocity must increase from the centre outwards. The stronger the charges of gunpowder are, the longer are the distances that can be employed in the experiments, and the greater the mean values of the velocity obtained.

Fig. 2



Thus, the form of the hodograph will vary much with the depth of the centre, and it must also vary with the law which expresses the change of velocity with the depth. But, whatever be the unknown law, the hodograph must always be convex at the epicentre, and, passing through a point of inflexion, afterwards become slightly concave. This follows simply from the law of refraction without any regard to the rate at which the velocity increases with the depth.

As long as we do not possess a sufficiently large number of time-observations for at least one earthquake, it will be impossible to draw any conclusion concerning the law of velocity from the form of the hodograph. Even with the best observations, we can never, in drawing the hodograph, expect that all points will fall on a regular and continuous curve. But what we may expect is that, with a sufficiently large number of observations, the points will be distributed equally on both sides of such a curve. The hodograph contains the observations from places

in all possible directions from the epicentre combined in a single plane. If the velocity is different in different directions, in the general figure these differences will be eliminated when the number of observations is large enough, and the result will be a curve free from local disturbances.

Although the time has not yet come for us to determine the definite form of an earthquake-hodograph, yet we know enough from the best examined earthquakes to decide whether the hodograph is an hyperbola or a curve with points of inflexion, whether Hopkins' law is confirmed by the observations, or an increase of velocity is noticeable in the outer zone of the disturbed area.

The best example for such an investigation is contained in von Seebach and Minnigerode's discussion of the earthquake of March 6, 1872, in Central Germany. An inspection of the map of coseismals published by them is sufficient to show that the horizontal coseismals are crowded together in a striking manner near Gottingen and Leipzig, at a distance of sixteen (German) miles from the epicentre. Accordingly, in drawing the hodograph we see how badly the hyperbola suits the observations. Several points which are most valuable for the determination of the epicentre, because they are nearest to it, and which agree most perfectly with one another, must be rejected in constructing the hyperbolic hodograph, in order that the earthquake may not begin at the surface of the earth until 1½ minutes after it was actually observed at five different places at five to six miles distance from the epicentre. For sixteen miles the hyperbola leaves all the best observations below it, after which nearly all points remain above it until it ends at Breslau, at a distance of fifty-seven miles from the epicentre. At this place a magnetic needle was found swinging by Prof. Galle at 4h. 5m. 25s., Berlin time, but the shock itself may have occurred several minutes earlier. The hyperbola is made to pass exactly through the point corresponding to this time, for otherwise its vertex would have to be placed still higher than it is now, and this would increase the already existing disagreement between the calculated time of the beginning of the earthquake and the actual observations.

How well, on the contrary, are the observations represented by a curve the vertex of which is a little below 3h. 55m., and, being convex downwards, passes at a distance of seven to eight miles between 3h. 55m. and 3h. 56m., reaches its points of inflexion at about eleven miles distance with a slope corresponding to 2·5 miles per minute, and then leaving some points on one side and some on the other, passes through Tübingen (36·7 miles), the last trustworthy point, until it reaches Breslau one minute before the observed time, with a velocity of at least fifteen miles a minute.

The Herzogenrath earthquake of October 22, 1873, leads to somewhat similar results. In drawing the hyperbolic hodograph, some of the best observations, those used for determining the position of the epicentre, have to be rejected altogether, while others must be supposed to err by as much as two or three minutes. But a curved line, passing through the mean positions of the points, is concave throughout on its lower side, with a large curvature at the epicentre. The figure certainly differs little from the form of the hodograph corresponding to a centre at the surface, and the inner zone is a circle of not more than four kilometres radius.

Thus the best investigated earthquakes at our disposal show that the observations agree much less closely with the older theory of concentric earthquake-waves, straight rays and hyperbolic hodograph, than they do with the new theory of a velocity of propagation increasing with the depth, rays convex downwards, and a hodograph with points of inflexion.

The Determination of the Depth of the Centre.—If the law connecting the velocity with the depth were known, we should be able to calculate the forms of the corresponding rays and hodograph, and to find a relation between the depth of the centre and the form of the hodograph. In Fig. 2 we have started with the simplest assumption, and supposed the velocity to vary as the depth. As this law is an entirely arbitrary one, the figure can only give a nearer approach to the truth than the theory represented in Fig. 1. If, for instance, the modulus of elasticity were to vary as the depth, the velocity would change much more rapidly near the earth's surface than far below it; and the fact that the perceptibility of earthquakes decreases so rapidly as the depth increases, certainly indicates that a rapid change in the velocity takes place immediately below the surface. Consequently, in calculating the depth of the centre according to our law, we should find too large a value. Other difficulties in determining the depth of the centre are our ignorance of the true superficial velocity, and the uncertainty as to the form of the hodograph, especially the doubtful position of its points of inflexion. But, in spite of all these difficulties, we may consider it as a rule that the depth will increase with the radius of the inner zone of the disturbed area, and that it will certainly always be smaller than this radius.

On the other hand, a minimum value of the depth may be found by means of the tangent at the point of inflexion. This tangent in Fig. 2, like the asymptote in Fig. 1, makes an angle of 45° with the horizon, because in both figures the central velocity (u_1) was taken as the time scale. While in Fig. 1 the asymptote passes through the centre, in Fig. 2 the tangent at the point of inflexion passes above it. Now, let us imagine the depth of the centre in Fig. 2 to remain the same, as well as the velocities u_1 at the centre, and u at the surface; but let the increase of velocity be no longer uniform as before, but be principally restricted to the neighbourhood of the surface. The consequence will be that the rays will differ little from straight lines at first when they leave the centre, and that the principal increase of curvature will be near the surface. The point of emergence of that ray which leaves the centre horizontally, will move to a greater and greater distance, and, as the same is the case with the point of inflexion of the hodograph, its tangent at that point will be displaced parallel to itself downwards; and when the whole change is imagined to take place in the surface itself, the hodograph will coincide with Seebach's hyperbola, and the tangent at the point of inflexion becomes an asymptote and passes through the centre.

Thus, with a hodograph adapted as well as possible to the existing observations, we find a depth of more than five, and less than ten, geographical miles for the earthquake in Central Germany, and a depth of less than three kilometres for the earthquake of Herzogenrath. Each of these earthquakes represents a special type. Type I., with a very small depth of centre and an approximate disappearance of the inner zone, is represented by the earthquake of Herzogenrath: Type II., in which both zones are pretty equally distinct, and the depth is rather considerable, by the earthquake of Central Germany. There may exist a Type III. with a very deep centre, or with small intensity and moderate depth, for which the point of inflexion of the hodograph falls outside the region when the earthquake is perceptible, and where, consequently, the hodograph is convex throughout. Amongst the earthquakes so far studied, for which the mean velocity has been calculated, those with small velocities, which generally have a merely local character, may safely be regarded as belonging to the first type.

THE TOTAL SOLAR ECLIPSE OF AUGUST 8, 1896.[1]

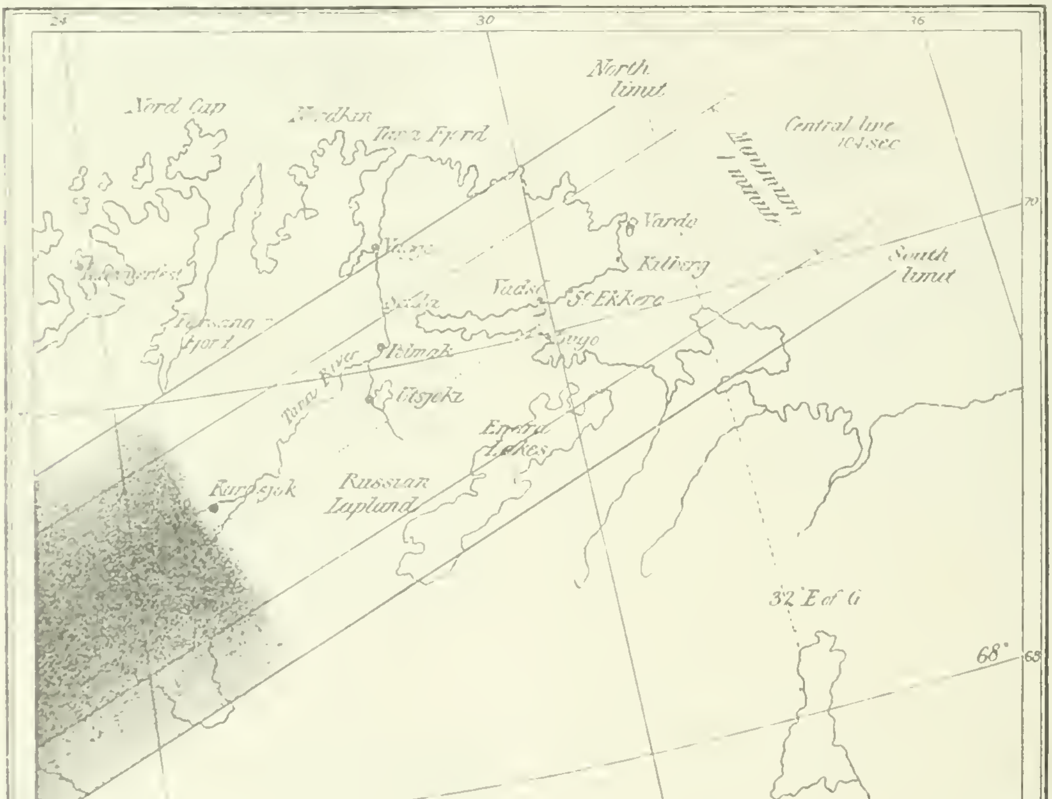
IT having come to my knowledge that some doubts had arisen as to the suitability of Norway as a post of observation for the total eclipse of the sun in 1896, and having had both experience in total eclipse expeditions and of travelling in Norway, I determined to make a special tour of observation both to the west coast, and also to Finmarken, Lapland, and the Russian frontier on the east coast.

In selecting stations in such an exceptional country as Norway, many points must be considered that do not apply to most places; thus it is not enough to know that A is twenty miles from B without also knowing how many fjords lie between, how many peaks three or four thousand feet in height, how many glaciers, and how far they are crevassed, if the mountains are passable, and if so what weight besides himself a man can carry up. Those people who have visited Norway, and the more so those who have travelled in the interior and north of the country, are surprised at the almost impossibility of moving at all except by the fjords and certain made roads. These generally may be said to extend as far north as Trondhjem, latitude 63° 26′; longitude 10° 30′ about. After that, on the north and north-east coast and Russian frontier, roads are the feeblest tracks generally, and the fjord communication only of a special character; the population, except at such places as Tromso, Hammerfest, Vardo, and Vadso, is very scanty, and chiefly confined to the sea coast, and in the latter case subject to consider-

[1] Abridged from a paper read before the Royal Astronomical Society, by Col. A. Burton-Brown (*Monthly Notices*, R.A.S., vol. lv. No. 3).

[illegible] ruing of the season of the year and the nature [illegible] fishing. How far these points would affect an astronomical [illegible] will be seen later on.

From Trondhjem we take a north north east course, passing [illegible]batten, an island of about 800 feet in height, and shortly [illegible] pass the island of Donnæss, at the bottom of the map on the west side, which will be on the border of the shadow during [illegible]; steaming north to Tommen Oe, which will have about [illegible] minute totality, a careful search for stations is kept. Luroe [illegible] low, Oxtinden promontory too much inland. Hest-[illegible], well within the 85° line, and the island of Tranen, near the 12th parallel, are too far west, though the central line passes [illegible]ly; but having a longitude of only 12° 5', the sun is [illegible] low. The height of ground is 3710 feet. Rod Loven [illegible], somewhat further eastward, and Bolgen, a peaked island, close to the central line, are rather too far westward; passing [illegible] Oe and Meloe, crossing the central line, we next miles direct north of the central line, where there is a hill to the north-east of the town. Excellent accommodation can be had there, and a telegraph station exists. Further north is the island of Lande Gode, within the 85° limit. The islands of Lundo and Engelo are on the northerly limit of the shadow of totality, and therefore useless. If Bodo were occupied as headquarters, with a steam launch at the disposal of the party, and Sandhornet, Arnoe, Fleina, Kunnen, or the lighthouse rock of Slot, Bolgen, Rod Loven, or Hestmando as detached stations, a valuable area would be covered, although these positions have the astronomical disadvantage of the sun's altitude being as low as $6\frac{3}{4}°$ to $7\frac{3}{4}°$, and the duration of totality but little over $1\frac{1}{4}$ to $1\frac{1}{2}$ minutes. Yet, owing to the stillness of the atmosphere on the west coast of Norway at that time, and the general freedom from clouds on the horizon, some good results should be obtained. The further fact of the corona being seen through a considerable thickness of watery vapour would have both a spectroscopic and photographic



[illegible] promontory of Kunnen, two or three miles above [illegible] line, a rather inaccessible position of some 2000 [illegible]. The lighthouse island of Slot is, however, in [illegible] desirable position, and Kunnen has a telegraph [illegible] adjacent. Proceeding east-north-east [illegible] island of Fugloe is passed, 2300 feet high, steep and rugged; and [illegible] miles further north, Fleina, about [illegible] feet [illegible] position. On the right is the island of [illegible], with the Sandhornet Mountain of 3300 feet—a rare [illegible], only about five or six miles above the [illegible] minute and a half totality, and the sun an [illegible] 7½°, the longitude being six or seven minutes [illegible]. This position gives an uninterrupted view all round. [illegible] the Arnoe Islands on the west, where there are [illegible] an almost equally good position. [illegible] communication with Bodo, the latitude [illegible] 67° 17' and longitude 14° 24' about, and which is ten interest in comparison with similar observations taken at about double the altitude on the east coast. Although several islands could be used as sites for stations, experience has shown some to be better adapted for many reasons than others. Thus Fugloe is steep and rugged, and has no advantages over Fleina except height; also Omnæs Oe is a troublesome place; Kunnen is an almost inaccessible promontory of chert and granite rocks; the island of Slot has a good anchorage for boats, and the sun would not be masked by Kunnen during totality. Here also is a telegraph station.

Leaving the west coast positions, we reach Tromso, and steaming up to Hammerfest, signs of civilisation die fitfully away. After rounding the North Cape, 71° 10' 40" N., and about 26° E., and steering east, the Nordkin is passed, and from there in a south-east direction we travel along a bleak, inhospitable shore of quartzite rocks to Vardo, on an island; this is about the north-east corner of Norway, a good-sized whaling station, and a fort,

with some second-class hotel accommodation. This place was used as an observing station in 1769 by the Austrian Hell for the transit of Venus; and, being less than twelve miles directly north of the central line of shadow, might be advantageously occupied. It forms the most easterly station; being in longitude 31° 8′, and latitude about 70 22′, it would have a duration of totality of over 1m. 31s., and the sun's altitude will be about $14\frac{1}{2}$°. It is easily accessible, no high ground obstructs the view, and provisions and labour are to be had. Passing south down a dreary coast of quartzite rocks and Silurian slates, we come to Kilberg, about ten miles south, and two miles inland. There is a hill about 500 feet high, but although this would be only five or six miles north of the central line, it is not in other respects a desirable station. Steaming south-south-west we pass Store Ekkero, a promontory some twenty-five or thirty miles west of Vadso, which appears to have all the attributes of a good station, provided accommodation can be arranged for: the central line of totality passes over the southern point, and there is a free view to the south-south-east and east-north-east, the sun's azimuth at the local time of 18h. being 97° south towards east, and the duration of totality a maximum—viz. over 1m. 41s., the sun's altitude about $14\frac{1}{4}$°. Passing on to Vadsö, the town of the Finmarken district, there are several hills, two or three hundred feet, easily accessible, and in all respects suitable for observing stations within three or four miles; indeed, Vadsö should be looked upon as the headquarters of an East Norway expedition. The local time of totality here would be 17h. 57m. 46s., and duration about 1m. 35s. All the aforementioned places are in telegraphic communication with most parts of Norway during the fishing season, and no doubt arrangements could be made for keeping the offices open as late as August 8. The temperature at Vadso is remarkably high, probably between 50° and 60° F. in August, and there is every chance of fine weather at that time.

Crossing the Varanger Fjord we come to Bugö, a Lapp fishing station, and within a mile and a half of the central line; the longitude is about 29° 50′, and latitude 69° 58′. There the duration would be about 1m. 40s., with nothing to obstruct the view; frequent communication could be had with Vadsö at certain times of the day; heliograph signals might be transmitted; there are several sites hereabouts, but one in particular desirable. The Bugönæsfjeld I will leave to those who like to be in the clouds! So little is known of it that every map shows it in a different position; but if intrepid mountaineers are fond of carrying half-hundred-weights up mountains, there is no reason why they should not have the satisfaction they desire, but they will find no one to do it for them.

In order to distribute the parties and multiply the chances of success, one party might proceed from Vadsö to Seida, on the Tana River. This station is a good one for all points except the length of totality, which is only about 1m. 12s., and has the sun at an altitude of about $13\frac{1}{2}$°. Polmák, some twelve miles due south (reached by poling up the river), is not so easily got at, but astronomically better situated, and south-east of it, about five miles, is a mountain over 1000 feet high. About forty or fifty miles further up this river, in a south-west direction, is Utsjoki, a place also that might be advantageously occupied in the Russian Lapland. The duration of totality there would be about 1m. 26s., and the sun's altitude about 13°; both at Polmák and Utsjoki camp equipage would have to be taken. Both are in telegraphic communication with Vadso and Vagge, the latter place being at the mouth of the Tana Fjord. Karasjok is astronomically a good place, within four miles of central line, the sun's altitude being about $12\frac{1}{4}$° and duration of totality over $1\frac{1}{2}$m. Much, of course, will depend on the number of observers it is proposed to send out, their powers of endurance, and knowledge of Lappish, Russian, and Norwegian for the east coast expeditions (except at Vardo).

To the information which Colonel Burton-Brown has brought together, we may add that the Orient Steam Navigation Company propose to send one of their large steamships to Vadsö, for the purpose of enabling observations to be made of the eclipse. The steamer will leave London on July 21, and, after calling at Odde, Bergen, Naes, Molde, Trondhjem, Hammerfest, and North Cape, will arrive at Vadsö on August 3. It will leave a week later, and will arrive in London on August 17. (Full particulars of this journey will be found in our advertisement columns.)

We are informed by Messrs. Cook and Son that the Bergenske Steamship Company have consented, subject to certain conditions, to send one of their best steamers from Bergen and Trondhjem to Vardo and back, for the purpose of enabling persons interested in astronomy to view the eclipse. It is proposed that the steamer shall leave Bergen on July 31, calling at Trondhjem two days later, reaching Vardo on August 8, and remaining until 4 p.m. on August 9, returning to Trondhjem August 13, and Bergen August 15. The steamer will call at all the usual places visited by the tourist steamers between Bergen and the North Cape.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Herman, of Trinity College, is appointed Chairman of the Examiners for the Mathematical Tripos.

The University Lecturer in Geography (Mr. H. Yule Oldham) announces a course of lectures on the Elements of Physical Geography during the present term. The Royal Geographical Society's Studentship of £100 will be awarded at Easter. Candidates must be members of the University who have attended the courses of the University Lectures.

The Council of the Senate recommend that the University of Allahabad be adopted as an affiliated University on terms corresponding to those in force for the University of Calcutta.

The report of the Syndicate on the Higher Local Examinations shows that good results have been attained in the scientific subjects. The new laboratory examination appears to work well, and has had a wholesome effect on the candidates' training.

Mr. W. C. D. Whetham and Mr. J. W. Capstick have been recognised as Teachers of Physics, and Mr. R. H. Adie as a Teacher of Chemistry, for medical degrees.

Among the freshmen who have matriculated this term, there are over 150 students of medicine.

SLOWLY, but surely, the system of paying teachers of elementary science according to the examinational successes earned by their students—in other words, according to their ability to cram young students with a large assortment of scientific facts, dogmatically stated and imperfectly understood—is giving way to one more calculated to create and foster a desire for natural knowledge. Within the past few days a Minute has been issued to schools under the Department of Science and Art, stating that the Lords of the Committee of Council on Education have decided to try the experiment of making grants for instruction in science and art depend partly upon the attendance of the student and partly on payments on results as tested by examination. The Committees of Science and Art Schools and Classes which have been in the receipt of grants from the Department for two consecutive years, or which are conducted by a local authority under the Technical Instruction Act 1889, or the Technical Schools (Scotland) Act 1887, will be allowed to elect to receive their grants on the scheme under which the payments on results will be one-half those on the present scale, while attendance grants will take the place of the other half, provided that the Inspector of the Department reports that the teaching and equipment of the school are thoroughly satisfactory, and that the class or classes are not too large for instruction by the staff of teachers. The attendance grant will be 1*d*. for each attendance of at least an hour's duration in a day science class, and 2*d*. in a night science class, and of 3*d*. for each attendance of one and a half hours' duration given to practical work in chemistry, physics, metallurgy, or biology, in a properly equipped laboratory. Applications to receive grants under the new Minute must be received before December 1, 1896, and in subsequent years before November 1. But the sanction to be so treated may be withdrawn at any time should it appear from the results of the examination in May, or from the reports of the Inspectors, that the instruction is not efficient; and no school can receive grants partly under the new Minute and partly under the ordinary scale of payments on results. Organised science schools are exempted from these attendance grants; nor can the grants be claimed on behalf of students who are on the register of an elementary school. The principle of recognising attendance at classes as one of the tests of the efficiency of a school has common sense at the back of it, and it should do something to reduce the baneful influence of the examination fiend upon elementary scientific education.

Dr. A. Rothpletz has been appointed Extraordinary Professor of Geology and Palaeontology in the University of Munich; Dr. Ernst Lecher, Professor of Physics in Innsbruck University, has been nominated to succeed Prof. Machs at Prague; Dr. F. Mares has been made Ordinary Professor of Physiology in the Bohemian University at Prague; and Dr. J. E. Humphrey has been appointed Lecturer in Botany at the Johns Hopkins University, Baltimore.

The Calendar of the University College, North Wales, for the year 1895-96, has been received. The physical, chemical, and biological laboratories (plans of which are given in the Calendar) now cover an extensive area. Under Prof. Andrew Gray, the physical department has greatly developed; and the appliances and electrical installation with which it is equipped enable the College to offer a complete course of instruction in all branches of electro-technical education.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, October 14.—M. Janssen in the chair.—The decease of Baron Larrey, free member, was announced from the chair. He died on October 8. M. Emile Blanchard pointed out the great influence of the deceased in modern surgery.—The Prince of Monaco has sent to the Academy No. ix. of his publications concerning the scientific work done on his yacht: a contribution to the study of the Cephalopods of the North Atlantic, by M. Louis Joubin.—On a mechanical amplification of the horizontal component of the earth's rotation, by M. Jules Andrade.—On a hydraulic apparatus to show the movement of rotation of the earth, by M. Aug. Coret.—M. Aug. Fabre, in a memoir on "Integration of the equation to the derived partials of the first order, of a function x with n independent variables $x_1, x_2, x_3, \ldots, x_n$" gives a quick new method of arriving at the general integral of an equation $\mu(x, x_1, x_2, \ldots, x_n, p_1, p_2, \ldots, p_n) = 0$.—M. J. Janssen, in the name of the Bureau des Longitudes, presented the 1898 volume of "Connaissance des Temps." There has been added to the tables concerning the satellites of planets, a table giving the elements for the calculation of the position of Mars' satellites at any given moment. In the ephemerides of the fundamental stars, the brightness of those above the first magnitude has been given, taking Aldebaran as unit. The Perpetual Secretary announced to be printed in the Correspondence, "Theorie der endlichen Gruppen von eindeutigen Transformationen in der Ebene," by M. S. Kantor.—On a class of linear equations to the derived partials, by M. H. von Koch.—On the surfaces of which the lines of curvature form a network with equal tangential invariants, by M. A. Thybaut.—On the double elliptic refraction and the tetrarefringence of quartz near its axis, by M. G. Quesneville.—On the estimation of argon, by M. Th. Schloesing. An apparatus with circulating mercury pump is described, which allows of the absorption of nitrogen and measurement of the residual argon. The whole arrangement is a modified form of Ramsay's apparatus for isolating argon.—On the action of hydrochloric acid on copper, by M. R. Engel. Copper decomposes a saturated solution of hydrogen chloride at 15° C., with liberation of hydrogen. This interaction does not occur if the concentration be less than that shown by the formula $HCl.10H_2O$. The presence of cuprous chloride retards the reaction greatly.—Action of potash and potassium ethoxide on benzoquinone, by M. Ch. Astre.—On combinations of antipyrine with the diphenols, influence of the respective positions of the hydroxyl groups, by MM. G. Patein and E. Dufau. Pyrocatechol, resorcinol, and quinol (hydroquinone) behave differently with regard to antipyrine: the ortho- and para-diphenols combine with two molecular proportions, the meta- with one. The combination occurs through one of the nitrogen atoms and the phenolic hydroxyl, which loses this property when its hydrogen is replaced by a metal or radical.—Experiments on the reducing power of pure yeasts, means of measuring it, by M. Nastukoff.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Elements of the Mathematical Theory of Electricity and Magnetism: Prof. J. J. Thomson (Cambridge University Press).—Elementary Physiography: Prof. J. R. A. Davis (Blackie).—A Directory of Science, Art and Technical Colleges, Schools and Teachers in the United Kingdom: R. L. Lawrence (Chapman and Hall).—A Manual of Physiology: Dr. G. N. Stewart (Baillière).—Movement: E. J. Marey, translated by E. Pritchard (Heinemann).—Frail Children of the Air: S. H. Scudder (Boston, Houghton).—Darwin and after Darwin: Dr. G. J. Romanes, ii (Longmans).—Among Rhode Island Wild Flowers: Prof. W. W. Bailey (Providence, R.I., Preston).—Pagan Ireland: W. G. Wood-Martin (Longmans).—First Steps in Egyptian: Dr. E. A. W. Budge (K. Paul).—Birdcraft: M. O. Wright (Macmillan).—Fishes, Living and Fossil: Dr. B. Dean (Macmillan).—Science and Art Drawing: J. H. Spanton (Macmillan).—Great Astronomers: Sir R. S. Ball (Isbister).—Elektrophysiologie: Prof. W. Biedermann, Zweite Abthg. (Jena, Fischer).—Protobasidiomyceten: A. Möller (Jena, Fischer).—The Tallerman-Sheffield Patent Localised Hot-Air Bath (Baillière).—University College of North Wales, Bangor, Calendar for the Year 1895-6 (Manchester, Cornish).—Atlas d'Ostéologie: Prof. C. Debierre (Paris, Alcan).—Evolution and Effort: E. Kelly (Macmillan).—A Handbook of British Lepidoptera: E. Meyrick (Macmillan).—Surface Currents of the Great Lakes: M. W. Harrington, revised edition (Washington).—Annuario p.p. Observatorio do Rio de Janeiro, 1895 (Rio de Janeiro).—U.S. Geological Survey Report, 1892-93, 2 parts (Washington).

Pamphlets.—Neue Versuche zum Saison-Dimorphismus der Schmetterlinge: Dr. A. Weismann (Jena, Fischer).—Neue Gedanken zur Vererbungsfrage: Dr. A. Weismann (Jena, Fischer).—Cavendish Lecture on Dreamy Mental States: Sir J. Crichton-Browne (Baillière).—The People's Stonehenge: J. J. Cole (Sutton).—Iron and Steel Institute: Presidential Address: Sir D. Dale; Metal Mixers: A. Cooper; The Effect of Arsenic on Steel: J. E. Stead; The Mines of Elba: H. Scott; On the Manufacture of Steel Projectiles in Russia: S. Kern; Ternary Alloys of Iron with Chromium, Molybdenum, and Tungsten: J. S. de Benneville (Victoria Street).—The Siouan Tribes of the East: J. Mooney (Washington).—Archæologic Investigations in James and Potomac Valleys: G. Fowke (Washington).—Chinook Texts: F. Boas (Washington).

Serials.—Proceedings and Transactions of the Queensland Branch of the Royal Geographical Society of Australasia, Vol. x. (Brisbane).—Quarterly Review, October (Murray).—Journal of Anatomy and Physiology, October (Griffin).—Contributions from the U.S. National Herbarium, Vol. 3, No. 3 (Washington).—Jahrbuch der k.k. Geologischen Reichsanstalt, xlv. Band, 1 Heft (Wien).—Societa Reale di Napoli, atti della Reale Accademia delle Scienze Fisiche e Matematiche, serie second, Vol. vii. (Napoli).—American Journal of Psychology, Vol. vii. No. 1 (Worcester, Mass.).—Ethnologisches Notizblatt, Heft 2 (Williams and Norgate).—English Illustrated Magazine, November (198 Strand).—Transactions of the Academy of Science of St. Louis, Vol. vi. No. 18, Vol. vii. Nos. 1, 2, 3 (St. Louis, Mo.).—Transactions of the Wagner Free Institute of Science of Philadelphia, Vol. 3, Part 3 (Philadelphia).—Proceedings of the American Philosophical Society, January, 1895 (Philadelphia).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1895, Part 1 (Philadelphia).

CONTENTS.

THURSDAY, OCTOBER 31, 1895.

THE CENTENARY OF THE INSTITUTE OF FRANCE.

FROM the brief telegraphic reports published in some of the English newspapers, readers in this country may have observed that the hundredth anniversary of the foundation of the Institut de France was celebrated last week in Paris. These reports, however, convey but a feeble impression of the real character of the celebration. The Institute is an establishment of which Frenchmen of all classes and of every shade of political opinion are justly proud. They look on it as a living embodiment of the culture and intellectual power of France. It stands above and beyond politics. Forms of Government may come and go; kings, emperors, and republics may arise, flourish, and disappear. But the Institute remains unshaken, quietly pursuing its career, and sustaining with marvellous success the intellectual glory of the nation. No wonder, then, that amid the turmoil of parties, the strifes of Parliament, and the endless changes of Ministries, many men turn to the Institute as the only stable institution, which royalists, republicans, socialists, and anarchists seem to be alike agreed in respecting.

That Republicans especially should show an interest in this institution was natural. It was founded a hundred years ago during the first Republic. The idea of restoring the old Academies and combining them into one central institution was carried out by the Republican Convention, with the openly professed intention of promoting the literary, artistic and scientific labours which should best contribute to the general benefit and glory of the Republic. After all the transformations of the last hundred years, a Republican form of government is once more in power. It was only fitting, therefore, that the State, by its highest officials, should manifest its interest in this, the oldest and most illustrious child of the Revolution, by taking an active and prominent part in the Centenary of its existence.

An Englishman privileged to be present at the celebration could not fail to be struck by various features in it that stood out in marked contrast to anything that would have been possible in his own country. In the first place, of course, the Institute itself is unique, in the wide range of subjects with which it is concerned. We have many admirable learned societies at home, from the Royal Society downwards, and so far as scientific progress is concerned, they are possibly of at least as great service as any Academy of Sciences in the world. We have likewise our Royal Academy of the fine arts, which may, it is to be hoped, hold its own against any foreign competitor. We have, however, nothing that corresponds to the French Institute, and the question has often been discussed whether the creation of such an Institute amongst us would be possible or desirable. But what especially strikes a stranger at such a gathering as that of last week in Paris, is the catholicity of view which led to the union under one organisation of so vast a range of human culture and faculty. Prose-writers, poets, dramatists, antiquaries, mathematicians, physicists, astronomers, geographers, engineers, chemists, mineralogists, geologists, botanists, anatomists, zoologists, physicians, surgeons, painters, sculptors, architects, engravers, musicians, writers on philosophy, morals, law, political economy, and history—all meet as in a common home under the dome of the Institute on the banks of the Seine. Each of the five Academies has its own sphere of activity and its own independent organisation. But they confer mutual strength and dignity on each other by the common tie that binds them together as the Institute of France. And one cannot help feeling that in a country liable to such political vicissitudes as France has gone through during the last hundred years, it has been of unspeakably great advantage to the stability and progress of all the arts and sciences which elevate a people, that this solidarity of intellectual effort should have been established at the beginning of the long succession of political troubles.

Another feature which impressed a native of this country was the direct, hearty and effective part which the highest functionaries in the State played in the chief events of the celebration. The President of the Republic himself received the foreign members and correspondants one morning at the Elysée, shaking hands with each, and stopping every now and then to say some few appropriate words to one whose name or whose work was known to him. The whole ceremony was as simple and natural as it was pleasant. M. Faure likewise presided at the opening meeting at the Sorbonne; and on Friday evening he held a brilliant reception, to which all the members and correspondents of the Institute were invited, with their wives, together with a large assemblage of other guests, including the Ministry, the Diplomatic Corps, and representatives of the chief departments and institutions. In short, everything which the head of the State could do to testify officially the pride and interest of France in her Institute was done simply and heartily. One felt that the President, kindly and gracious as he was personally, represented a national feeling which would have demanded expression no matter what form of Government had been in existence, or what political party had been in power.

Nor was the action of the President the only manifestation of official interest in the celebration. The Prime Minister, the Ministers for Foreign Affairs, War, Marine, Public Instruction, and others found time to spend an hour or two at one or other of the gatherings. The Minister for Public Instruction, M. Poincaré, indeed, multiplied himself in the most astonishing way. Having the official control of the department under which such organisations as the Institute are placed, he evidently considered it to be his duty, as it seemed certainly to be a pleasure to him, to attend every gathering where his presence could testify the sympathy of the Government with the Institute and its objects. At one time he was to be seen at the Ministry of Public Instruction holding a reception of all the academicians and correspondants, with their wives, and a large company of representative men from outside. At another time he was on the platform beside the President, making a vigorous speech, and conveying to the Institute the appreciation which he and his colleagues had of the work which the various Academies had accomplished. Again he was in his place presiding at the banquet given to the Institute, ready once

more with eloquent words to wish prosperity to literature, art, and science. And as if all this were not enough in the midst of his other busy official engagements, we found him just after breakfast at the unveiling of the Meissonier statue in the Louvre Gardens, where he made an admirable speech, summing up the characters of Meissonier's work.

An Englishman might be forgiven if he ventured to express openly his opinion that such things as these could not, or at least would not, be done in his own country. We suppose our Vice-President of the Council is the Minister who most nearly corresponds here to the Minister of Public Instruction in France. But when had we ever a Vice-President who thought it worth his while to show, outside of his official duties, so much active interest in the cause of science, art, and literature?

While this recognition from the State and its functionaries was extended to the Institute, the latter showed in several ways how well it realised its representative character as the outward symbol of the higher intellectual progress of France. One was especially impressed by the way this feeling was exhibited at the opening gathering in the great hall of the new Sorbonne. Behind the academicians and correspondants, the best seats in the building were allocated to representatives of education, law, justice, &c. The chief schools and colleges had places allotted to them, legibly marked out by large labels affixed to them. Lawyers, judges, and professors came in their robes to take part in the proceedings. Every section of the programme appeared to have been most carefully thought out. There was a well-trained orchestra, which began by playing a composition of the first composer who became a member of the Institute of France, and afterwards gave a fragment of *Mors et Vita*, by Gounod the last composer who had passed away from the Academy of the Beaux-Arts. Good care, indeed, was taken in the celebrations to show that music and the drama were included within the range of the Institute's activities. An afternoon "gala" performance at the Théâtre Français included parts of Corneille's *Cid* and Molière's *École des Femmes* and *Femmes Savantes*, wherein the chief members of this incomparable company showed once more what perfect acting should be.

Lastly, a stranger could not but be pleased with the numerous facilities offered to him to meet his old friends, and to make new ones. At the evening receptions and dinners, at the daylight gatherings in the Institute buildings, and in the foyer of the Théâtre Français, but most of all in the excursion to Chantilly, and the rambles through the rooms and grounds of that princely chateau, he had opportunities of seeing everybody that he wished to converse with. No one who went to Chantilly will be likely to forget the success of that concluding day of the proceedings the autumnal woods with their long vistas, the magnificent castle, the endless treasures of art and literature within the rooms, but above all, and as the centre and soul of the whole scene, the figure of the Duke d'Aumale, who has gifted all that estate to the Institute. Sitting in his bath-chair wrapped up in black velvet, hardly recovered from his last attack of gout, he showed himself the most vivacious talker in the company, shaking hands with his guests, discoursing to them of pictures, travel, and incidents of his life with the urbanity and dignity of the old *grand seigneur*.

There was one special source of gratification to English visitors in the remarkable band of men who went to represent Great Britain at the Centenary. The French members of the Institute seemed to feel the compliment paid to them by the attendance of so many illustrious men of science, literature and art. And the strength of the English contingent drew forth the admiration of visitors from other countries. It was pleasant, in these days of political rivalry, to see human culture linking men in a brotherhood which stands above nationality and politics, and more especially to note that nearly the whole of the Englishmen who have been so generously recognised by the Institute of France should have attended its Centenary.

THE GOLD MINES OF THE RAND.

The Gold Mines of the Rand; being a Description of the Mining Industry of Witwatersrand, South African Republic. By F. H. Hatch and J. A. Chalmers. London: Macmillan and Co., 1895.

AFRICA is proverbially a land of surprises. It is not likely, however, that more startling surprises can be in store than those witnessed by the present generation. We have seen a great city spring up, in what, before the discovery of gold in the Witwatersrand, was a desert, a city with over eighty mines, the workings of which extend east and west from Johannesburg for 45·8 miles. The mines have been worked with regularity, and the augmentation of dividends has attracted the attention of capitalists in all parts of the globe, resulting in the Russian Government commissioning Mr. Kitaeff to report on the gold-field, and in the Prussian Government despatching Mr. Schmeisser for the same purpose. The output of gold from the Witwatersrand has risen from 23,000 ozs. in 1887 to 2,023,198 ozs., valued at nearly £7,000,000, in 1894, whilst the return for the first nine months of the current year was 1,711,337 ozs. The Transvaal now produces one-fifth of the world's supply. It is calculated that at the present rate of progress the output of the Witwatersrand mines will have reached by the end of the century a value of £20,000,000.

To the already ample literature relating to the Transvaal gold mines, this handsome and profusely illustrated volume of three hundred large octavo pages is the most valuable contribution that has yet appeared. The authors possess special qualifications for the important task they have undertaken. Mr. J. A. Chalmers is an Associate of the Royal School of Mines, and his brilliant career as a student has been followed by many years successful practice as a mining engineer in South Africa; whilst Dr. F. H. Hatch's scientific attainments and literary skill are well known from his important petrographical researches carried out previously to his retirement in 1892 from the Geological Survey of England and Wales, and from his useful manuals on mineralogy and petrology.

The authors divide their subject-matter into twelve chapters. The first deals with the history of the gold discoveries and of the development of the mining industry, whilst the subsequent chapters deal respectively with the geology, the auriferous conglomerates, the

Witwatersrand deposits, the development and prospects of deep-levels, mining practice, surface equipments of the mines, the metallurgical treatment of the ore, economics, mining law and statistics.

Unfortunately for students of South African geology, much confusion results from the fact that beds of an identical character often receive different names in different localities. The inconvenience of this want of uniformity in the classification of the rock systems will now, it is hoped, be obviated, as the authors' clear exposition of South African stratigraphy cannot fail to be generally accepted. The geology of South Africa is, it may be noted, comparatively simple. The main subdivisions are (1) recent deposits; (2) the Karroo formation; (3) the Cape formation, and (5) the South African primary formation. The sedimentary deposits are underlain by granites, gneisses, and crystalline schists, which constitute the greater portion of the formation of north-west central Africa. This primary formation occurs largely in Mashonaland, Matabeleland, and the Mozambique, and predominates in the northern and eastern parts of the Transvaal. Lying unconformably on these beds are the shales, sandstones, conglomerates and limestones of the Cape formation, which extend over the southern, western, and middle parts of the Transvaal. They appear to be of an age corresponding with the Devonian and Lower Carboniferous periods of European classification. The Karroo formation, which may possibly be correlated with European Lower Mesozoic formations, has a widespread occurrence in Cape Colony, Natal, the southern Transvaal and the Orange Free State. It derives its importance for the Transvaal from the fact that it carries the coal-seams that have rendered such valuable aid to the development of the auriferous deposits. Lastly, the recent deposits comprise those of alluvial and æolian origin, together with the curious surface material to which the authors apply the somewhat misleading name of "laterite." This material is widely distributed throughout the Transvaal. The gold of the Witwatersrand is obtained entirely from beds of conglomerate, known as "banket," carried by the Cape formation. These are composed mainly of pebbles of white or grey quartz embedded in a matrix consisting originally of sand, but now completely cemented to an almost homogeneous material by a later deposition of quartz. The pebbles as a rule do not carry any gold, the mineralisation being confined to the matrix. The average total yield of the conglomerate stamped last year was 13·16 dwts. of fine gold per ton. With regard to the origin of the ore-bodies, the authors enumerate the various hypotheses without giving their support to any one of them. They have, however, been unable to find any evidence in favour of the idea locally prevalent that the dykes met with, have acted benefically on the banket in their immediate neighbourhood in regard to gold contents. Petrologically the dykes belong to the group of dark-coloured greenstones, among which the authors have recognised the following types: diabase, olivine-diabase, bronzite-diabase, epidiorite, gabbro and olivine-norite.

One of the most interesting chapters in the book is that on the development and prospects of deep-levels. As the bedded character of the banket deposits became known, and as the persistency in depth and the uniformity in the gold-contents became established by deep boreholes, companies were organised to work the deep-seated portions of the beds. In discussing the depth at which the main bed will be found, the authors bring forward evidence to show that a very important flattening of the bed takes place. They therefore take a more optimistic view of the future of the gold-mining industry than that taken by other writers. The most important problem that presents itself is to ascertain the limit in depth to which mining may profitably be carried. The limiting factors are increased temperature, excessive initial expenditure, and increase of working costs. The rise in temperature with increasing depth must, the authors think, be ascribed almost entirely to secular causes. Unfortunately very few experiments have been made to gauge the rate of increase. Mr. Hamilton Smith in 1894 made some determinations of the water in the Rand Victoria borehole at a depth of 2500 feet, the results indicating an increase of 1° F. for every 82 feet. Some rough determinations, too, have been made by Mr. A. F. Crosse at the Ferreira and Crown Deep shafts. In view of the scientific interest and commercial importance of the matter, it is to be hoped that an accurate determination of the temperature will be made at the bottom of the borehole which is now being put down to intersect the main bed at a depth of 3500 feet. At present, experience in other countries is the only available guide, and it is to be regretted that such results collated by the authors are very incomplete. A table of temperatures in some deep European and American mines is given (p. 104), but this, being disfigured by gaps and misprints, such as St. Andre for St. Andreasberg, Prizebram for Przibram, Sanson for Samson, Lambert for Charleroi, does not carry much weight. Nor are the shafts of the Michigan copper mines fair illustrations to select, inasmuch as the coolness of the rock is undoubtedly due to the proximity of the cold waters of Lake Superior. The authors' statement that at the Calumet and Hecla shaft, Michigan, there is a rise of only 4° F. in a depth of 4400 feet, is certainly inaccurate. The temperature determination must have been influenced by the fact that compressed-air rock-drills are in use at that mine. The ice-cold exhaust would lead to erroneous results. The usual geothermic gradient is 50 to 55 feet for an increase of temperature of 1° F., and the lowest recorded is that of 100 feet to 1° F., at the Lake Superior copper mines. It would appear, therefore, that in assuming it to be somewhat less than this in the Rand, the authors are taking too optimistic a view, more especially as Mr. Crosse's determination (p. 103) of 66·7° F. at 825 feet, and 70·7° F. at 1030 feet, indicate the normal gradient of 50 feet to 1° F. In the discussion of this important subject, the authors might have referred with advantage to Koebrich's 387 determinations of temperature in the Schladebach borehole. These are of special importance, as they were taken at fifty-eight points at equal distances of 30 metres down to the greatest depth yet attained of 1716 metres. The result of this investigation was that the gradient was found to be 46·09 metres for 1° R.

The chapters describing mining practice, surface equipment, and the metallurgical treatment of the ore, occupy more than a third of the volume. Admirably illustrated by

excellent drawings and photographs, they give a clear idea of the vigorous manner in which the work is carried on. Additional authority is given to them by the fact that they contain contributions by Mr. L. I. Seymour, Mr. C. Butters, and other leading engineering experts. The volume concludes with valuable information regarding material and supplies, labour, working costs, mine accounts, mining laws and regulations, production and dividends. A good index, eighty illustrations, fourteen photographic plates, and seven folding maps and plans, complete a volume of which the authors may justly be proud. With the exception of a geological map, which would have been a useful addition, the only omission appears to be a bibliography of the existing literature relating to the subject. The authors appear to be unacquainted with the geological work of Mr. C. J. Alford (London, 1891), and with the engineering descriptions of Mr. T. Reunert (London, 1893). Indeed they regard the published information relating to the nature of the ore deposits and to the extraction of the gold as meagre and inadequate. Yet Mr. W. Gibson in 1892 published a list of sixty-seven works on South African geology, sixteen of which bear directly upon the geology of the Transvaal. Mr. Schmeisser in 1894 gave the titles of fifty such works, and Dr. K. Futterer in 1895 gave 156 titles. With the rapid development of the mining industry, literary productions become antiquated with remarkable rapidity. When the writer of this review visited the Witwatersrand in 1892, there were 1907 stamps running. Now, according to Dr. Hatch and Mr. Chalmers, there are 2642 (June 1895). Since 1892 work has been pushed on more vigorously than ever before, and from the sixty mines near the outcrop of the main bed 5,000,000 tons of ore have been extracted in 1893 and 1894. Numerous deep boreholes have been put down to the dip of the bed, and several shafts have been sunk, encountering the auriferous conglomerates at depths of 600 to 1000 feet. Five years hence there will be 8000 stamps running. The present average stamping capacity is over four tons per stamp per day, and it is probable that, owing to technical improvements, the average will be five tons. With a total extraction of 10 dwts. of gold per ton, the output should be 6,500,000 ounces. The ore reserves are estimated at 170,000,000 tons, equal at 45s. per ton to £382,000,000. It seems unlikely that the average cost of mining and treating this ore will exceed the present cost of 30s. per ton. The authors think, therefore, that they may safely forecast a production from the Witwatersrand within the next half-century of £700,000,000, of which £200,000,000 will be profit.

Bennett H. Brough.

STARCH.

Untersuchungen über die Stärkekörner. By Dr. A. Meyer. (Jena: Fischer, 1895.)

THOSE who are best acquainted with the laboured details of Naegeli's classical investigations into the nature and growth of starch-grains, and the controversy which followed regarding his astounding hypothesis, which so long dominated certain of our text-books under the name of the "intussusception theory," will best be prepared for another huge work of inquiry into the physical and chemical nature, growth and solution, and significance to the plant generally of those curious structures. The full appreciation of the magnitude and value of Meyer's task will depend on the reader's acquaintance with the bearing of numerous discoveries which have been made since Naegeli's day, and turned to criticism and the final overthrow of his hypothesis; and among these stand prominently, on the biological side, Schimper's demonstration of the significance of the various plastids to the stratification of the starch-grain, Sachs' brilliant work on the *rôle* of the starch-grain in assimilation, and Strassburger's severe criticisms in his researches on the structure and growth of the cell-wall; and, on the physical and chemical side, Émil Fischer's work on the synthesis of carbohydrates, and the splendid work of our own countryman Horace Brown—the latter, indeed, as much physiological as chemical in its methods and results.

Meyer's book, which contains over 300 large pages of closely-printed German in the dryest of styles, which would be hard to forgive if the matter were not so good and the spirit so enthusiastic, covers the whole range of the enormous domain now centred around this formerly so insignificant a structure, the starch-grain; and it is embellished with nine tables and ninety-nine illustrations, good, bad, and indifferent, for the quality of the figures varies much, suggesting periods of different powers or methods of delineation during the fifteen years or so the author has been occupied with this monumental monograph.

For it *is* monumental, in the sense that it has evidently been gradually built up as a big structure, bit by bit, with morsels of hard evidence dug with great labour from the difficult quarry of facts, only to be worked with the best powers of the microscope, and the best methods which modern technique puts at the disposal of the investigator.

The work may be regarded as divided into five parts. The chemistry, physics, and biological properties of the starch-grain as an object of research, form the subject-matter of three of these parts; the fourth is occupied with some extremely ingenious and careful comparative studies of the changes undergone by the grain in the different organs of various selected plants, at stated seasons, and under experimentally varied conditions; while the fifth part may be taken as the critical survey of the investigations and views of others scattered through the body of the work, and the copious literature collected at the end.

It is, of course, impossible to traverse a work like this in a review, and the following short summary must suffice for a glimpse at Meyer's views and results, some of which he has already published in short papers from time to time.

He regards the typical starch-grain as consisting of two substances, one of which, *a-Amylose*, can be obtained separately in the crystalline form, whereas the other—*β-Amylose*—cannot be isolated in crystals. The relations of these two constituents to each other, and to other carbohydrates found in modified starch-grains, are considered in detail; they occur in the grain itself as acicular crystals (*trichites*) arranged more or less radially, and the starch-grain is in effect nothing but a complex, mixed sphere-crystal composed of radiating branch-systems of these *trichites*, in different proportions, and more crowded in the denser layers than in the softer ones.

The cases where amylo-dextrine occurs, and the relations of all these substances to other carbohydrates, their behaviour in water of various temperatures, the action of diastase, and so forth, are discussed at great length, and we are glad to see that the author has paid attention to, and, it may be added, been considerably influenced by, the valuable work of Brown, Heron, Morris and Salomon, and there are points of discussion of interest to all these workers.

Of course a view like Meyer's must depend for its validity essentially on what experimental results can be got in the way of obtaining sphere-crystals of carbohydrates like amylose under known conditions: if the author's statements regarding the crystallisation into spherites of inulin and amylodextrin and other bodies in a viscous matrix can be extended to the case in point—where the protoplasm of the amyloplast acts as the viscous matrix—he has certainly made out a strong case, for all the ordinary physical properties of porosity, behaviour to polarised light, swelling, and the stratification, striation, and other structural peculiarities of the starch-grain are as easily explained if the unit of structure is a *trichite* as where it is assumed to be a *micella*.

Since it is as yet impossible to artificially crystallise the amylose composing the chief part of a normal grain, into the spherical shape, however, the war of discussion will no doubt rage around this point; in the meantime, Meyer has unquestionably marshalled his facts in heavy order and made out an ingenious case, the full significance of which can only be grasped by ploughing one's way through his heavy, but, in the main, logical German.

The phenomenon of swelling has always been a crux in hypotheses regarding the structure of organised bodies. Meyer explains it as due to the *trichites* of β-*Amylose*—the principal constituent of the normal starch-grain—absorbing water, and themselves swelling. In other words, the water dissolves in the crystals.

It should be noted, however, that Meyer distinguishes sharply and emphatically between *Porenquellung*, where water is merely imbibed between the crystals, and *Lösungsquellung*, where the water is taken up by the crystals; and he here emphasises what may be a useful distinction in questions of imbibition. It is, of course, *Losungsquellung* which initiates the disorganisation of the grain.

In the discussion of the question as to the growth of the starch-grain, the author points out that the latter may grow in chromoplasts, as well as chloro- and leucoplasts, and that the grain *never* impinges on the cytoplasm—it is always completely surrounded by a layer, however thin, of its plastid so long as the cell lives; he makes this seem probable, but it is impossible to prove it in some instances. In any case, the reader will find some pretty staining methods brought to bear on the point.

Of course the grain grows by apposition, and the thickness of the layer deposited depends on that of the protoplasm in contact at the place. On the whole, indeed, the laws of growth and stratification are those laid down by Schimper and Strassburger, though Meyer adds a good many facts as to the initiation and growth of both simple and compound grains, and has devised a new nomenclature and classification of the various kinds of starch-grains which, complete and exhaustive though it appears, we confess does not seem to meet the requirements of clearness and simplicity so fully as could be desired.

One of the most ingenious chapters in the book is that on the solution of the grains in the cell, and the significance of fissures and pores for the attack of the diastatic or other solvent.

Space is not available for detailed remarks on the author's methods of examining the changes which the starch-grains undergo in the various organs of *Adoxa*, *Hordeum*, *Dieffenbachia*, *Pellionia*, *Hyacinthus*, *Oxalis*, &c., at different times of the year and under different conditions; nor to give his views on the constitution of protoplasm—which we venture to think too much of the nature of a hastily-written note, moreover not necessary to the subject, and far from convincing in the six pages (with critical sentences on everybody from Naegeli and Wiesner to Bütschli interspersed) devoted to it. Put briefly, Meyer regards protoplasm as a peculiar emulsion, and therein agrees essentially with Berthold; whereas the elements of cell-walls and starch-grains are as truly crystallised out as is calcium oxalate.

The experiments showing that the position of the layers of the starch-grains can be altered by changing the position of the organ in which they are growing, and that the alternation of day and night is expressed in the thickness and density of the layers—that the layers are "diurnal layers"—in effect (pp. 268–271) are well worth attention, however, as indeed are very many others of the difficult experimental points brought out towards the end of the book.

That the questions centering around the starch-grain have not reached finality, is obvious, but that Meyer has contributed a valuable attempt to set some of them at rest, must be admitted by all who read his monograph. It bristles with debatable points, and there are some annoying faults—*e.g.* the frequent references to figures and titles in the text without sufficient clues, and to chapters ahead of the reader; but that does not weaken the fact that his results stimulate the reader to some close thinking, and his critical compilation of the history and literature of the subject alone makes the book necessary to all working botanists.

H. MARSHALL WARD.

APPLIED METEOROLOGY.

Weather and Disease. A Curve History of their Variations in Recent Years. By Alex. B. MacDowall, M.A., F.R.Met.S. London: The Graphotone Co., 1895.

THE systematic study of climatic conditions in connection with the fluctuation in the public health, is one which has only recently been undertaken, but which already promises results of a most interesting and important character. Apart from the inherent interest of the subject, which must indeed be apparent; the study offers, like observations in phenology, the prospect of great practical value. The work of weather forecasting is at present so wanting in accuracy, and there is so little promise of progress in this direction, that practical meteorologists might be tempted to despair, and the general public be led to imagine that the vast stores of records which have been accumulated were destined to

remain fruitless for an indefinite time. The application of meteorology to related subjects in general, and to hygiene in particular, may thus be considered doubly welcome.

Mr. MacDowall's primary object, in the publication before us, is to represent the variations which certain elements of the weather, and the mortality from certain common diseases have undergone during recent years, and it may be to find a connection between the two. The mode of representation which the author has adopted is the one now commonly in use of plotting curves on ruled paper, by adjoining points, the ordinates of which are determined by the two quantities to be related, one of which generally refers to date. These curves have as a rule been subjected to a process of *smoothing*, which, by recording the average of every five or ten (as the case may be) consecutive values, eliminates the fluctuations of short duration, while preserving the more gradual and lasting variations. The great advantage which this method possesses is, it is hoped, to enable the eye at once to detect the more salient features of a general tendency, without the mind being distracted by a mass of details which may be, for the purpose in view, absolutely useless. In this way, within the compass of some twenty curves, the author exhibits the general tendencies which have controlled the principal and most interesting features of the weather; while a further sixteen curves show the fluctuations which have taken place in the most important zymotic diseases.

If we have any fault to find with a very excellent purpose, on the whole admirably carried out, it would be to remark that the curves would be better if drawn on a larger scale. This would have increased the expenses of production, but the result would be clearer. It would have been of advantage, too, if the numerical details, from which the curves have been drawn, had been given; then any one interested in a particular inquiry could have easily constructed the curve to any desirable scale. This point is of particular importance if the reader wishes to know what is the "probable error" of any point on the smoothed curve, or, in other words, what is the degree of reliance to be placed upon the process of smoothing. For instance, a comparison is instituted, or at least suggested (p. 63), between the curves representing the mortality from diarrhœa and dysentery, and that showing the mean temperature for July at Greenwich. There is apparently some resemblance between the two, but the probable error of either curve may be greater than this apparent agreement. If the solution of a system of equations of condition, to which these curves may be compared, yield the quantity sought, accompanied by a probable error as large as the unknown itself, great hesitancy is experienced in accepting the result as a satisfactory solution.

Mr. MacDowall's aim is apparently a modest one: for the most part he is content to leave his graphic representations of both kinds of records to speak for themselves, and invite the reader to study them independently, and to follow up any point which they may suggest. The author's own notes are not copious, but they are clear, interesting, and concise. Some of the curves, too, are very instructive. The opponents to compulsory vaccination will not find much to support their views in the curve tracing the mortality from small-pox through the last two centuries. The steady and consistent improvement in the twenty years following the introduction of vaccination, in 1798, pleads eloquently in favour of the process. The great decrease shown in the number of deaths from scarlet fever may be misleading, if it be not compared with the sad and alarming increase in the mortality from diphtheria. Previous to 1859, these two diseases were not separately registered in the Registrar General's Reports; but if the two curves be combined, the mortality from neither has conspicuously varied.

The book, small as it is, appears to have been carefully compiled, and must have involved a considerable amount of labour in its production. It should certainly be consulted by those who are interested in the relations between meteorology and hygiene. W. E. P.

OUR BOOK SHELF.

Popular History of Animals for Young People. By Henry Scherren, F.Z.S. Pp. 376. (London: Cassell and Co., Limited, 1895.)

What would have been said a few years ago of a popular history of animals of which the opening chapters were devoted to man and his resemblance to other members of the Order Primates? In the days when it was the fashion to place man in a separate order of Bimana, while the man-like apes were called Quadrumana, the mere idea of including the human race in the animal kingdom would have raised a storm of indignation. Yet here we have a book, intended for a popular public, in which the principle of relationship is fully recognised, and man is assigned his proper place in nature. Thus do the scientific ideas which are anathema of one generation become the accepted truths of the next.

One of the features which distinguish this book from most of the legion of popular works on natural history published in recent years, is that common names of animals are used throughout, and no attempt is made to familiarise the reader with the nomenclature of scientific zoology. This fact will endear the book to all who like to learn a little about the habits of animals, but have no desire to know any details. For such readers the present volume is admirably suited; it is full of readable anecdotes about animals, and is illustrated with thirteen coloured plates, as well as numerous figures in the text. Most of the illustrations, both coloured and plain, are old friends, but a few have been reproduced from photographs. We think the volume will be successful as a prize-book and as a book for general readers.

Simple Methods for Detecting Food Adulteration. By J. A. Bower. Pp. 118. (London: Society for Promoting Christian Knowledge, 1895.)

The author describes a number of simple tests for detecting common adulterations in articles of food. In the main, the tests described can only be carried out by means of a fairly good microscope, so they are quite beyond the ordinary householder until he provides himself with such an instrument, and educates himself in the use of it. Of the thirty-six illustrations in the book, twenty-eight represent microscopic views of various substances, and it will be of little use for any one to set about detecting fraud until he is perfectly familiar with the varying appearances exhibited not only in the illustrations, but by actual specimens mounted on slides. Possibly the book will induce young people to determine specific gravities, and make other simple observations; and if it does that, it will justify its existence.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Introduction of a West Indian Frog into the Royal Gardens, Kew.

A SHORT time ago Mr. W. Watson, the Assistant-Curator of Kew Gardens, informed me that he had noticed for several years, in some of the hot-houses, specimens of a small frog, which, hiding away during the day among the pots and orchid-baskets, enlivened the quiet evenings with their shrill, whistling notes. Suspecting that this frog must be a foreign importation, I asked the Director to allow some of the specimens to be caught, and some days ago I had the pleasure of receiving three specimens in excellent condition.

The frog is *Hylodes martinicensis*, a small arboreal species, distributed over, and common in, many West Indian Islands (Martinique, Porto Rico, St. Vincent, Dominica, Barbadoes, &c., and possibly in Trinidad). Mr. Watson recollects that he observed it first some ten years ago, that he lost sight of it for some time, but that it reappeared about four or five years ago. Taking into consideration the few facts with which we are acquainted as to the reproduction of this frog, it seems most probable that several specimens of both sexes were, on more than one occasion, accidentally introduced in Wardean cases.

However that may be, it is evident that the frogs have freely propagated since their introduction. At present they are most numerous in the propagating houses, in which the temperature ranges between 80 and 100°, sinking in winter at times to nearly 60°. Accompanying Mr. Watson one evening, I heard from several points the call of the frogs, which somewhat resembled the piping of a nestling bird; and guided by the sound, I had soon the pleasure of seeing one of them clinging to the side of a glass-case.

There is nothing extraordinary in the accidental importation of individuals of a tropical species of frog into Europe; but it is an interesting experience, that the species should have permanently established itself. This is owing, in the first place, to the favourable conditions under which it found itself placed, and, secondly, to the peculiar mode of its propagation.

Hylodes martinicensis, and probably the majority of its congeners, does not spawn in water, but deposits from fifteen to thirty ova on leaves in damp places. After a fortnight the young frogs are hatched in a perfect form, having passed through the metamorphosis within the egg, thus escaping the vicissitudes and dangers to which they would have been exposed during the progress of the usual Batrachian metamorphosis.

This instance of the acclimatisation in Kew Gardens of the "Coqui" (as the frog is called in Porto Rico) is unique in Batrachian life at present. I trust that the little guest may long flourish where it has found such a congenial home, and where it usefully aids in the destruction of plant-eating insects and wood-lice, of which I found great numbers in the stomach of a specimen. If at a later period a nest with ova were discovered, Mr. Dyer would delight the heart of embryologists, to whom the opportunity of examining fresh ova of this frog would be most welcome. ALBERT GÜNTHER.

Kew, October 20.

The Cause of an Ice Age.

IT appears to me that the position taken up by Sir Robert Ball in his book, "The Cause of an Ice Age," is seriously misrepresented by Sir H. Howorth in one paragraph of the criticism which appears in NATURE of October 17. Sir H. Howorth says, that the fact of the invariability of the ratio of the heat received by our hemisphere in summer to that received in winter cannot be the cause of variability in climate; "if, as we are told in the book over and over again, this particular proportion (63 : 37) is the cause of the Ice age, we must be living in an Ice age now, and we must always have been living in an Ice age." Now it is nowhere asserted by Sir Robert Ball that the invariability or the magnitude of this ratio is the cause of an Ice age, but it is very clearly explained that he assumes the cause of an Ice age to be a *particular range of positions of the line of equinoxes combined with a high value of the eccentricity of the earth's orbit*, and that the fact that the above ratio is 63 : 37, and not unity, as appears to have been supposed to be the case, is relevant only so far as it inclines us to regard the changes of climate due to the causes just mentioned as much greater than we might otherwise have regarded them.

It seems obvious that a large value of the eccentricity contemporaneous with a favourable position of the line of equinoxes will correspond to *some change* in climate. Whether this cause is a dominant one, or even an important one, in its effect on climate, is of course an open question, and one upon which I express no opinion. Sir H. Howorth thinks that Sir Robert Ball has inadequately recognised the fact that the ratio of heat received in summer to that received in winter by one hemisphere has been calculated by Wiener. I find, however, on page 90 (second edition), the following reference to Wiener's work. "They depend on the mathematical calculation given for the first time, I believe, by Wiener in his work, "Zeitschrift der Oesterreichischen Gesellschaft fur Meteorologie," vol. xiv., 1879, p. 129. . . . My chief object is to emphasise the relation of these calculations made by Wiener to the astronomical theory." Wiener's work is also mentioned in the preface.

On the general question as to the adequacy of Croll's theory, with or without the fact which Sir Robert Ball adduces *solely* with a view of strengthening that theory, I express no opinion; it seemed to me, however, that in fairness, some of the remarks made by Sir H. Howorth required refutation.

Christ's College, Cambridge. E. W. HOBSON.

Green Oysters.

ONLY to-day I was able to read Prof. Lankester's letter (NATURE, May 9, 1895), and wish to reply briefly. My note in *Monitore Zoologico* was simply a preliminary communication; the proofs of my assertions will be given *in extenso* in a paper which will soon be published. My conclusions in that part which may interest the previous labours of Prof. Lankester may be briefly expressed as follows:—

(1) My observations have always been made on true *huitres de Marennes*.

(2) I believe that Prof. Lankester must have overlooked the recent works on the histology of Molluscs by Janssen, Rawitz and others, or he would have seen that his "gland cells" are the *becherzellen*, *cellules caliciformes* of the authors quoted; which are inside the branchial epithelium, and not on its surface, and never can be considered wandering, nor can they have amœboid movements. It would be strange, therefore, to consider such "gland cells" as similar to the amœbocytes of the blood!

(3) Prof. Lankester says that the "gland cells" contain green granules in the Marennes oysters, but this is entirely due to an optical illusion; if one examines a fresh piece of branchial lamella of the green *huitre de Marennes*, the "gland cells" appear green, but if these cells be separated from the epithelium, one finds that they are always colourless, and that they appeared green because they are surrounded with green matter. Making careful sections of the branchial lamellæ or the labial palps, one finds clearly: (*a*) that the gland cells are never green; (*b*) that the superficial epithelium is green; (*c*) that some amœbocytes and large masses included in the epithelium are also green. I am ready to furnish Prof. Lankester with microscopical preparations showing what I assert.

(4) The green of the Marennes oysters is not a hurtful substance which must be got rid of, and it is incorrect to imagine a defensive phagocytosis performed by amœbocytes. To me it is quite obvious that the green colouration is merely due to a true assimilation of nutritive substance which takes place through the agency of the epithelium in some portions of the intestine and in the branchial lamellæ. And no doubt it is the amœbocytes who carry the green substance, assimilated from the epithelium, to the liver. I am quite aware that these results of my researches are new, and it is for this reason that in communicating them to the *Monitore Zoologico* I noted that they are of some importance to our further knowledge of the physiology of mollusca.

(5) It is a mistake to believe that the oysters are green because they feed on *Navicula ostrearia*: the truth is that the alga is green for the same reason that the Marennes oysters are so, which is from the nature of the *parcs* and *claires* bottom. It is therefore the same substance, viz. the blue pigment "Marennin," which is found in both.

(6) The chemical part of my work is not concluded, and I

fear that I shall not be able to finish that most difficult task. But I may note that my assertion that "Marennin" contains principally iron, is based on the recent researches of Muntz and Chatin.
D. Carazzi.

Spezia, Italy, October 12.

Oxford Endowments.

I am surprised that my friend Dr. Hickson, whose past residence among us lends authority to his words, should so greatly misrepresent facts as to say, in Nature of October 3, that "the income of the [college] endowments is frittered away in the salaries of the heads, the stewards, the bursars, and the tutors of the pass-men," the fact being that these endowments do not provide the salaries of either the stewards or the tutors. It is further difficult to see how estates can be managed without bursars, and how bursars can exist without salaries; how complex institutions can work without heads, and how heads can live on nothing; and how the payments to bursars and heads—the latter at least with stipends fixed by statute—is in any way connected with "the [alleged] unfortunate competition that exists between colleges."
Christ Church, Oxford.
R. E. Baynes.

I am sorry that my friend Mr. Baynes should think that I have "misrepresented facts" in my article on the "Linacre Reports." I did not state, nor did I intend to imply, that the whole of the salaries of the tutors and stewards is derived from college endowments; but surely it is true that in the majority of cases these officers are fellows of their colleges, and as such receive a substantial sum of money annually from the college endowments.

I am quite competent to understand that college estates cannot be managed without bursars, and that bursars cannot live without salaries; but the estates of the Oxford colleges could be managed by less than half the number of bursars that now exist in Oxford—provided that they were chosen carefully from among those who have had some training or experience in their profession—and a large annual income would be saved from the endowments.

As to the heads. Speaking with every respect for these august persons, I still feel that with judicious amalgamation three or possibly four heads would be sufficient to carry on the official work, they now perform, with efficiency and dignity.

I should exceedingly regret if any remarks of mine should give offence to my friends in Oxford; but I never hesitated to express my opinion there or in Cambridge, that the independence of the colleges means a fearful waste of their endowments; and until, by Act of Parliament, a suitable amalgamation of these institutions is brought about, there will be little margin left for the endowment of research and the payment of those engaged in pure scholarship.
Sydney J. Hickson.

Late Leaves and Fruit.

Here, many of the roadside lindens have cast their summer foliage, and put forth a garniture of new leaves; these are fully grown, and bear the vivid tint of spring. In this city, on the 11th inst., well-grown open-air strawberries were on sale in the fruiterers' shops. The quantity altogether amounted to several bushels.
J. Lloyd Bozward.

Worcester, October 19.

THE CENTENARY FÊTES AT PARIS.

THE latter part of last week has witnessed the celebration of the first centennial anniversary of the foundation of the Institut de France. Paris was certainly not at its best, as far as meteorological features were concerned; the weather offered nothing "Queenly" or "Presidential" in its demeanour, and upon the whole was what it generally is at this time of the year—unpleasant, wet, and cold. But it hardly interfered with the proceedings and festivities, and we trust none of the generally aged guests of the Institute will be any the worse in health for their rapid visit to Paris.

A large number of foreign associates and corresponding members had promised to come; and the occasion was such a remarkable one, that we print in full the list of acceptations. Of the Académie des Inscriptions et Belles-Lettres, the Associés étrangers present were MM. Ascoli, Helbig, Max Müller, Whitley Stokes; while the Correspondants present were MM. Bailly, de Beaurepaire, Blade, Blancard, Champoireau, Chevalier, Comparetti, Delattre (le Père), De Smedt, Sir John Evans, Goeje, Gomperz, de Grandmaison, Ioret, Kern, Merlet, Millardet, Naville, Radloff, Saige, Sauvaire, Windisch. In the Académie des Sciences, the Associés étrangers who attended were Lord Kelvin and Dr. Frankland; and the Correspondants were MM. Amagat, Arloing, Backlund, de Baeyer, Bayet, Bergh, Bichat, Blondlot, Brioschi, Cannizaro, Considère, Crova, Engelmann, Sir Archibald Geikie, Gosselet, Grand Eury, Haller, Herrgott, Houzeau, Kovalewski, Laveran, Lépine, Lie (Sophus), Lockyer, Marès, Marion, Masters, Matheron, Ollier, Pagnoul, Ramsay, Raoult, Rayet, Retzius, Sir Henry Roscoe, Sabatier, Sire, Sirodot, Stephan, Sir G. Stokes, Suess, Sylvester, Général Tillo, Treub, Vallier. In the Académie des Beaux-Arts there were the Associés étrangers, MM. Alma Tadema, Da Silva, Gevaërt, Pradilla; and Correspondants, MM. Bertrand, Biot, Civiletti, Cui, Cuypers, Dauban, Deffés, De Geymuller, Gouvy, Guffens, Israëls, Lanciani, Le Breton, Loenhoff, Marionneau, Martenot, Perrin, Ronot, Salinas, Salmson, Sgambati, de Vriendt, Waterhouse, Wauters. In the Académie des Sciences Morales et Politiques, the Associés étrangers present were MM. Carlos Calvo and Castelar; and the Correspondants were MM. Aubertin, Babeau, Barkausen, Bodio, Caillemer, Ducrocq, Du Puynode, Ferrand, Lallemand, Lecky, Legrand, le Comte de Lucay, Molinari, Moynier, Sir F. Pollock, Polotsoff, Raffalovich, Stubbs, Villey Desmeserets, Worms.

At one time, it had been decided to choose the epoch of the centennial anniversary for the transfer of Pasteur's mortal remains from the vaults of Notre Dame to their final resting-place at the Pasteur Institute. The plan was not carried out, and it was better so. The frame of mind which is suitable for festivities is not so for a funeral, and it would not have been in good taste to mingle the one with the other. The plan was dismissed after short, but wise, reflection. The festivities were carried out in strict accordance with the announcements made, and published in Nature.

On the first day, the 23rd, a religious service was celebrated in Saint Germain des Près, in memory of all members of the Institute deceased since its foundation, by Monseigneur Perraud, Bishop of Autun, a member of the Académie Française, and a very distinguished writer and philosopher. It must not be thought that, even in the land of Voltaire, all men of science consider atheism as "the" proper form of philosophy. The Institute is very conservative, and whatever opinions most members may hold concerning religion and dogmas, every man has his own conception of the universe, more or less, and entertains "son petit religion à part soi," as a witty German princess put it, in her own barbarian French. This first ceremony was largely attended, although more national than international in character. The real general opening of the celebration took place the same day at 2 p.m., when the foreign associates and correspondants were received and entertained in the salon of the Institute by the members of the latter. Each *invité* was announced by the *huissiers*, and after having been introduced to the masters of the house, joined his own personal friends and acquaintances in pleasant conversation and numerous introductions to fellow-workers of every land. The masters of the house were M. Ambroise Thomas the author of *Mignon*, member of the Académie des Beaux-Arts, and for this year President of the Institute, assisted by MM. Maspero, Marey, Léon Say, Count Delaborde, delegates of the four other Academies. The last function of the day was a general reception of all members,

associates, and correspondants by the Minister of Public Instruction. The reception-rooms of the Ministry had been very elegantly adorned for the purpose. A whole series of tapestries—from the celebrated Gobelins manufactory—illustrating Don Quichotte's life, after the pictures by Coypel, decorated the walls of a large hall which had been built for the purpose, at the end of which a stage had been erected. M. Poincaré, the Minister, received most cordially his guests, who comprised, besides the members of the Institute, a large admixture of very different elements, among which political men were predominant. A very pleasant evening was provided by the singers and actors, among which were the best of the Opera and of the Théâtre Français, and by the excellent orchestra of the Opera.

On the next day (Thursday, 24th) a general meeting was held in the large hemicycle of the new Sorbonne, so splendidly decorated by the paintings of Puvis de Chavannes. The President of the French Republic was present with such Ministers as were not professionally detained at the Chambre des Députés, and after the overture of Méhul's *Joseph*—Méhul was the first composer who belonged to the Institute—three speeches were delivered. M. Ambroise Thomas began, and was short. M. Jules Simon came next, but, as his voice is weak, he could not possibly make himself heard in more than a small fraction of the hemicycle. M. Poincaré, the Minister of Public Instruction, spoke last, and very appropriately. This long ceremony ended with a fragment of *Mors et Vita*, of Gounod, played by the orchestra of the Opera.

In the morning a short reception took place at the Elysée, where the President of the Republic received the foreign members of the Institute. The foreign associates and correspondants, to the number of fifty-five, were presented to President Faure by the office-bearers of their respective Academies. The President welcomed them, and held a short conversation with each, and M. Gaston-Boissier presented him with three volumes containing the minutes of the Institute since its foundation.

In the evening a banquet took place at the Hôtel Continental; two hundred and fifty members were present. After two short "after-dinner" speeches by M. Ambroise Thomas and M. Poincaré, M. Max Müller, acting as spokesman for all the foreign members and associates, proposed the health of the Institute, "which, alone, remains unaltered and immovable in its renown and glory, while so many things have changed during this century," in very excellent terms. Most happily inspired was Lord Kelvin in his address. The very cordial and sympathetic expression which the Royal Society gave to its feelings in its address to the Institute, was received with much satisfaction, and the few words which closed the orator's speech went to the heart of all Frenchmen: "Personally, I cannot express how much I appreciate the great honour you have done me in electing me among the associates of the Institute. But I owe to France an even greater debt. She has been, truly, the *alma mater* of my scientific youth, and has inspired my admiration for the beauty of science, which during my whole life has kept me chained in her service. It was Laplace who initiated me into celestial mechanics, and a few years later the venerable Biot led me by the hand and introduced me to Regnault's laboratory. To Regnault and Liouville I shall eternally be grateful for their kindness towards me, and for the solid teaching they gave me, in 1849, on experimental physics and mathematics. M. President of the Institute, gentlemen, I thank you with all my heart. From what I have said, you will understand why I consider with perfect gratefulness France as the *alma mater* of my scientific life." Lord Kelvin spoke with his heart as well as with his reason, and the great applause which followed his speech must have told him that he had made no mistake in doing so.

The 25th was devoted to an afternoon in the Théâtre Français; the programme, to be sure, was of somewhat an austere character. The *Cid*, the *École des Femmes*, and the *Femmes Savantes* were exceedingly classical and sedate. . . . though, what might have been put in their place we could hardly decide, and classics were probably more suitable for an audience comprising a large number of foreigners than some modern play, where the *finesses* might have been a little too subtle and delicate. A very nobly-felt and worded poem by Sully Prudhomme—the most philosophical of French poets of the period—was read by Mounet-Sully, the *doyen*, the veteran of the French theatre. In the evening a reception was held at the Elysée by the President, who most graciously shook hands with the foreign members who had already been at the Elysée in the morning. The members of the Institute were all but lost in a crowd of political men, senators, deputies, officers, and functionaries who had been invited to meet them.

The last act was a visit to the magnificent residence of Chantilly, to the Duc d'Aumale. A special train left the Northern Railway Station at 11.15 a.m., carrying 239 members, and at Chantilly eleven large vehicles transported the whole assembly to the chateau, through part of the woods, the race-course, and the stables. The Duke, who had hardly recovered from an attack of gout, had to receive his guests sitting in a rolling-chair, and received them most cordially. Lord Kelvin and other members of the British contingent had some conversation with the Duke in English, and the afternoon was devoted to inspection of the residence itself, which has been splendidly enlarged and embellished by the present proprietor, and to the surrounding grounds. The whole of Chantilly and of its contents, as we have already said, has been bequeathed by the Duke to the Institute. This represents nearly £2,000,000, exactly 43,000,000 francs. As the Institute owns already some 25,000,000 francs (£1,000,000), at the death of the Duke the whole amount will be of some 70,000,000 francs (under £3,000,000). The whole Institute distributes over 725,000 francs in prizes each year.

And now the festivities are over, and most of the Institute's guests have gone back to their home or country—may their remembrances be pleasant. They have met some of their fellow-workers, and new friendships have been formed. Such meetings are profitable. While ill-feeling between nations are being daily suggested and excited by the incautious and ill-advised prose of a number of irresponsible men, it is well that occasionally the heads and lights of different countries should meet and mingle together. Knowing each other better, appreciating each other, united by a same bond to a same faith, they may, by their influence, help to further the advent of the reign of reason and goodwill. A great number of men, like Moses, have already expired in view of the Promised Land; and doubtless many more will do the same. The Promised Land seems very remote, and hardly "promised." But this is no reason for not doing what should be done, and international assemblies of the "best of the land" cannot fail to exert a useful influence.

HENRY DE VARIGNY.

This account of the *fêtes* would be incomplete if we did not give M. Jules Simon's discourse on the Institute, the delivery of which formed the central feature at the meeting in the Sorbonne. As M. Jules Simon is the foremost French orator, and his style is remarkable not only for its brilliancy but for its terseness, we give the whole oration as it was delivered.

MESSIEURS, Quand le général Bonaparte prit le commandement de l'armée d'Égypte, il signa aussitôt de la façon suivante ses proclamations et ses ordres: "Bonaparte, général en chef, membre de l'Institut." "bien sur, disait-il, d'être compris du dernier tambour."

L'Institut n'avait pas trois ans. Il a fait depuis ce temps-là quelque bruit dans le monde. Je ne puis donc me flatter d'apprendre à personne sa courte et glorieuse histoire. Je la résumerai en quelques mots pour nous réjouir en commun de ses grandeurs et non pour nous en instruire.

Les grandes assemblées qui prirent en mains le sort de la France à la fin du XVIII^e siècle eurent dès leur premier jour l'instinct révolutionnaire. Elles ne se proposèrent pas pour but de conserver les institutions existantes en les améliorant et en les purgeant de leurs abus: elles firent partout table rase, et quand elles eurent tout renversé, elles s'occupèrent, en liberté, de tout reconstruire.

Les académies avaient largement contribué à l'avènement de la Révolution. A peine eut-on passé de la théorie à l'action qu'elles trouvèrent qu'on allait trop loin. Elles avaient voulu réformer: on ne songeait plus autour d'elles qu'à détruire. La Révolution, de son côté, fit comme toutes les révolutions: elle oublia ce qu'on lui avait donné et s'irrita de ce qu'on lui refusait.

Elle se borna d'abord à des mesures malveillantes.

L'Assemblée constituante vota avec hésitation et provisoirement pour une année, en accompagnant son vote d'aigres reproches, les subventions que le Comité des Finances demandait pour les corps littéraires.[1] La Convention frappa les grands coups. Elle défendit d'abord de pourvoir aux sièges vacants, et enfin, en août 1793, elle supprima "toutes les académies et sociétés littéraires patentées par la Nation."

On a souvent remarqué que cette même révolution qui avait supprimé toutes les académies créa l'Institut, qui est une académie. Ce n'est pas versatilité dans les assemblées. La pensée de créer de toutes pièces une académie nouvelle était contemporaine de la résolution prise d'en finir avec les académies anciennes.

L'Assemblée constituante avait chargé Mirabeau de lui soumettre le plan d'une académie nationale. Mirabeau appela Chamfort qui était en querelle avec l'Académie française. Chamfort écrivit une violente diatribe et prépara un projet que Mirabeau n'eut pas le temps de lire à la tribune.

Les projets se multiplièrent sous la Convention. Condorcet, d'Alembert, Daunou, Talleyrand, tous ceux qui avaient le souci des grandes choses, apportèrent leur contribution. On dit que Talleyrand accepta la paternité d'un projet entièrement rédigé par l'abbé Desrenaudes, qu'il avait eu pour vicaire général à Autun et que nous avons connu membre du Conseil de l'Instruction publique. Talleyrand était de ceux qui peuvent se passer [illegible] taire; mais la tradition est ancienne et persistante.

Tous les auteurs de projets ont réclamé à l'envi le titre glorieux de fondateurs de l'Institut. La vérité historique exige que l'on [illegible] autre nom en tête de cette liste d'honneur, et ce nom est [illegible] de Richelieu, fondateur de l'Académie française.

Nous sommes plus justes aujourd'hui que ne l'ont été nos pères. Notre admiration pour les grandes œuvres de la Révolution ne nous cache pas les gloires de la monarchie, qui sont les gloires de la France. Nous fêtons le centenaire de l'Institut de France, mais il ne nous en coûte pas d'associer à l'honneur de cette journée le fondateur ou les fondateurs des académies dont l'Institut a reçu l'héritage, Louis XIII et Louis XIV, Richelieu, Séguier, Colbert. L'Institut existe depuis le 25 octobre 1795; [illegible] académies qui le composent remontent à 1635. Assurément l'Institut de France, depuis sa fondation, compte dans ses rangs un nombre considérable d'hommes illustres. J'en veux citer quelques-uns, avec le regret de ne pas les citer tous: Chateaubriand, Lamartine, Victor Hugo, Alfred de Musset, Alfred de Vigny, Guizot, Cousin, Thiers pour l'Académie française; Monge, Berthollet, Lagrange, Laplace, Lavoisier, Fresnel, Ampère, Arago, Cuvier, Geoffroy Saint-Hilaire, [illegible], [illegible], Claude Bernard pour l'Académie des Sciences; [illegible], Victor Le Clerc, Littré, Boissonade, Hase, Naudet, Burnouf pour l'Académie des Inscriptions; Louis David, Ingres, [illegible], Meissonier, David (d'Angers) pour l'Académie des Beaux-Arts.

J'avais arrêté là cette liste de nos gloires contemporaines pour obéir à la loi qui m'est imposée de ne prononcer le nom d'aucun vivant: faut-il que je doive aujourd'hui ajouter le nom d'un homme que j'ai connu il y a plus de cinquante ans, à l'École normale où il était élève, où j'étais professeur, qui était notre ami à tous, car on ne pouvait le connaître sans l'aimer, et qui était avant tout l'ami et le bienfaiteur de l'humanité: le nom immortel de Louis Pasteur? Les voûtes de cette salle gardent l'écho des acclamations qui l'accueillirent quand il vint, à cette place même, recevoir les hommages du monde savant. L'humanité, ce jour là, fut reconnaissante et juste.

Ainsi l'Institut de France a eu, dès son premier siècle, une magnifique floraison de grands hommes. Nous sommes fiers de nos gloires nouvelles: mais nous gardons pour nos gloires séculaires un culte reconnaissant et filial. Nous ne renonçons ni à Corneille et Racine, ni à Boileau, ni à La Fontaine, ni à Bossuet, ni à Voltaire, ni à Montesquieu, ni à Buffon, ni à Clairaut, ni à d'Alembert, ni à Huyghens, ni à Mariotte, ni à Mabillon, ni à Rollin, ni à Turgot, ni à Lebrun, ni à Mignard, ni à Lesueur, ni à Philippe de Champagne, ni à Mansart, ni à Soufflot.

Messieurs, le drapeau aux trois couleurs est toujours pour nous "le drapeau chéri"; c'est l'astre de la liberté et de la civilisation; mais nous suivons avec amour et orgueil le drapeau blanc fleurdelisé remontant les âges jusqu'au siècle qui fut le grand siècle et qui reste par excellence le siècle français.

C'est le 29 janvier 1635 que l'Académie française reçut sa consécration officielle. L'Académie des Beaux-Arts eut le même honneur en 1648, l'Académie des Inscriptions en 1663 et l'Académie des Sciences en 1666.

Il ne suffit pas d'avoir restitué la création des académies à Louis XIII et à Richelieu, il faut remonter jusqu'à Conrart. La première en date, l'Académie française, est, comme beaucoup de grandes choses, due à l'initiative privée. Conrart n'était rien. Il n'est rien devenu. Il n'est célèbre que par son silence: un genre de célébrité créé tout exprès pour lui par Boileau. C'est lui qui eut l'idée de donner un règlement à une compagnie qui se réunissait tour à tour chez chacun de ses membres pour parler de littérature. Ils étaient neuf en le comptant. De petits hommes, dit Voltaire, d'un ton dédaigneux. Des hommes obscurs, dit-il ensuite en parlant des premiers académiciens au nombre de vingt-huit qui reçurent ce titre après les lettres royales de 1635. Sans doute on n'eut pas sur-le-champ un Corneille ou un Racine à introduire dans l'Académie. Il fallut attendre douze ans pour Corneille, trente-six ans pour Bossuet, trente-sept ans pour Racine, quarante-neuf ans pour La Fontaine et Boileau. L'assemblée se garnissait de grands hommes peu à peu. Elle ne devait jamais avoir quarante grands hommes. Aucune assemblée en aucun temps et chez aucun peuple ne pourra en avoir à la fois qu'un nombre très limité. Ceux que Voltaire appelle de petits hommes ne sont peut-être pas aussi petits qu'il le croit. Ils semblent petits à la postérité; ils étaient grands pour leurs contemporains. Apprenons, ne fût-ce que par prudence, à respecter les hommes d'élite qui ne sont ni des Voltaire ni des Molière. On ne peut pas, et on ne doit pas se tromper sur les hommes de génie; on peut hésiter sur le choix entre les hommes vraiment supérieurs sans être grands, ceux que j'appellerai les hommes distingués dans le genre médiocre.

C'est un honneur pour la société éclairée du XVII siècle d'avoir su le-champ attaché de l'importance à cette réunion de quelques hommes de goût, qui ne s'occupaient entre eux ni de religion ni de politique, et parlaient uniquement des lettres et des ouvrages de l'esprit. L'amour des lettres est resté un des caractères de notre génie national. Dès que le public fut admis aux réceptions de l'Académie française, il y courut. Quand elle ouvrit en 1702 ses portes aux femmes pour ces jours-là, les femmes affluèrent. L'Académie n'a eu garde de renoncer à cet usage qui a pris avec le temps plus de solennité. Une réception à l'Académie est, par excellence, un événement parisien. Il faut y avoir assisté; il faut avoir son avis sur les deux discours. On attache moins d'importance aux séances les plus passionnantes de la Chambre. La fameuse coupole est un instrument de torture; on y étouffe, on y perd connaissance. Ces femmes évanouies sont un accroissement de succès pour les deux orateurs. Elles font penser aux corridas espagnoles, qui ne sont admirables, au dire de leurs ennemis, que quand un toréador a été tué.

On parla de la société de Conrart au cardinal de Richelieu. Il avait l'instinct du grand et du stable. Il jugea que cette compagnie pouvait devenir une institution. Il offrit aux amis

[1] [illegible] l'Académie française, [illegible] livres, plus 1,200 livres pour un prix à [illegible] pour l'Académie des Belles-Lettres, [illegible] livres; pour l'Académie des [illegible], [illegible] livres; ces deux Académies devaient aussi décerner [illegible] de [illegible] livres.

de Conrart de reconnaître officiellement l'existence de leur association. Ce fut à peu près tout ce qu'il offrit; "des privilèges honorables, dit Voltaire, aucun d'utile, son fondateur ne lui ayant même pas procuré une salle d'assemblée."

En réalité, il ne rendait à l'Académie d'autre service que de ne pas l'ignorer, mais il pensa, et tout le monde pensa avec lui, que puisqu'il ne l'ignorait pas, il la gouvernait. Plusieurs des amis de Conrart hésitèrent. Ce qu'ils avaient cherché, c'était la liberté, on leur offrait l'assujettissement. Cette résistance ne pouvait durer; on ne résistait pas au roi, ni au cardinal, qui était le roi. Refuser une grâce qu'ils offraient, c'était plus que résister, c'était désobéir. On céda, on remercia. On exalta le roi et le grand ministre Richelieu qui promettait de protéger.

Il y eut une autre difficulté à la création officielle de l'Académie. Le Parlement aussi eut la velléité de résister. On sait que l'enregistrement était alors nécessaire pour donner efficacité aux décisions royales. Le Parlement pouvait retarder, il pouvait faire des observations et même des remontrances. A la fin, dans les grandes occasions, on avait raison de lui par un lit de justice. On n'alla pas jusqu'à ces extrémités pour la transformation des réunions de Conrart en Académie royale; mais le Parlement manifesta sa mauvaise humeur par un retard d'un an. Le cardinal fut obligé de faire entendre qu'il voulait être obéi.

On a cherché la cause de cette mauvaise volonté du Parlement. Il ne s'agissait pas de la création d'une cour souveraine, mais "de simples peseurs de syllabes et de jurés fabricateurs de mots," comme disaient les mauvais plaisants de l'époque.

Le Parlement, suivant Voltaire, craignit que l'Académie ne s'attribuât quelque juridiction sur la librairie, et ajouta cette clause aux lettres patentes du roi: "L'Académie ne connaîtra que de la langue française et des livres qu'elle aura faits ou qu'on exposera à son jugement."

Je crois plutôt que le Parlement craignait pour l'autorité qu'il s'attribuait en matière religieuse et philosophique. La question des académies touchait à la question des écoles. La théologie était tout près; plus l'autorité du Parlement était contestée en matière religieuse, plus il s'en montrait jaloux. Il obéissait dans toute cette affaire au même esprit qui inspira plus tard la réforme de l'Université par le président Rolland.

Le roi, et je parle ici de Louis XV autant que de Louis XIV et de Louis XIII, fut constamment pour les académies un bon maître, mais un maître. Les élections durent être soumises a son approbation; c'est un droit qui a toujours été conservé au pouvoir public; il existe encore aujourd'hui. Louis XIV l'exerça une fois dans une occasion très éclatante. Il voulait l'élection de Boileau; l'Académie élut La Fontaine. Le roi refusa son approbation. L'Académie s'empressa d'élire Boileau à la première vacance. "A présent, dit le roi, vous pouvez procéder à la réception de La Fontaine."

Le roi intervint aussi, mais bien rarement, dans les travaux de l'Académie. C'est lui, ou plutôt c'est Richelieu, auteur de la tragédie de *Mirame*, qui prescrivit cet examen du *Cid* inventé pour exalter la gloire du cardinal et dont le résultat fut de montrer dans tout son éclat la gloire de Corneille. Voltaire, au siècle suivant, sous prétexte d'impartialité et en mêlant l'apothéose à la critique, essaya la même entreprise et aboutit au même résultat.

Les académiciens, un moment détournés de leurs travaux plus paisibles, revinrent au Dictionnaire. On ne manqua pas sous la Révolution de leur reprocher de n'avoir fait ni la Grammaire, ni la Poétique que le roi attendait d'eux et d'avoir mené trop lentement le travail du Dictionnaire.

L'Académie n'était pas si coupable qu'on le croyait. Des trois objets confiés a ses soins, elle avait choisi le Dictionnaire, qui rendait à la langue le double service d'en fixer les termes et d'en expliquer les règles par des exemples empruntés aux meilleurs écrivains.

Le Dictionnaire avançait lentement. Cette lenteur fait sa force. Les variantes qu'il enregistre ont toutes été jugées et consacrées par le temps, avant de recevoir cette confirmation officielle.

Le Dictionnaire est à lui seul toute l'Académie française. A notre langue essentiellement souple et vivante, qui exprime avec facilité les passions et les idées à mesure qu'elles se renouvellent et qui suffit, sans néologismes, à l'exposition et à la démonstration des découvertes scientifiques, il donne la solidité et la majesté des deux langues qui ont successivement incarné la Grèce et Rome.

Louis XIV voulait qu'il y eût une langue de Louis XIV comme il y avait une langue de Périclès et une langue d'Auguste, et il revendiquait pour lui-même l'honneur de cette pensée lorsqu'il disait: "Le soin des Lettres et des Beaux-Arts ayant toujours contribué à la splendeur des États, le feu roi, notre très honoré seigneur et père, ordonna en 1635 l'établissement de l'Académie française pour porter la langue, l'éloquence et la poésie au point de perfection où elles sont enfin parvenues sous notre règne."

Je n'ai garde d'insister; je dis la pensée de Louis XIV et de ceux qu'on appelait dès lors les Quarante.

Notre admiration pour nos chefs-d'œuvre et notre langue ne nous empêche pas d'admirer la gloire des autres nations. Nous nous sommes associés au centenarie de Shakespeare; Gœthe, Schiller, Cervantès sont populaires dans nos écoles. Nul n'entrera jamais sans une respectueuse et solennelle émotion dans cette église de Santa-Croce à Florence où sont réunis, autour du cénotaphe du Dante, les tombeaux de Galilée, de Michel-Ange, de Machiavel, d'Alfieri, de Cherubini.

Le XVIII siècle reprochait toujours aux académies et surtout à l'Académie française, qui portait le poids des querelles parce qu'elle avait porté celui de la gloire et parce que le public pouvait plus facilement suivre ses travaux, d'avoir élu des hommes médiocres et d'avoir laissé en dehors d'elle des hommes de génie.

Je connais deux hommes de génie qui n'ont pas été de l'Académie française, Descartes et Molière. Rousseau, dont on prononce quelquefois le nom à propos des omissions de l'Académie, était citoyen de Genève.

Deux erreurs en un siècle et demi! Les hommes se trompent ordinairement plus que cela. La plupart des ouvrages de Descartes sont écrits en latin. Le *Discours de la Méthode*, qui est un des grands monuments de la langue française, n'était connu que d'un petit nombre de savants et de philosophe. Le grand éclat de la renommée de Descartes n'a commencé qu'après sa mort, quand on a enfin compris qu'il avait émancipé la raison humaine. Molière avait contre lui sa profession; on se rirait aujourd'hui, avec raison, d'un tel obstacle. C'était quelque chose sous Louis XIV. Messieurs les tapissiers valets de chambre du roi n'auraient plus voulu être de l'Académie. Je ne sais pas ce que Molière lui-même aurait pensé de son élection. On était alors conservateurs du rang comme on l'est aujourd'hui de la propriété. Il fallut contraindre Catinat à se laisser faire maréchal de France.

Quant aux autres grands hommes dont la Convention regrettait si amèrement l'absence, ils apartenaient à la catégorie de ceux que nous appelions tout à l'heure des hommes distingués dans le genre médiocre. Ils étaient admirés, à juste titre, par leurs contemporains; la postérité a le droit de choisir entre eux. Dufresny, Raynal, Helvétius sont des grands hommes dont on blâmait en 1793 l'omission, et dont on blâmerait aujourd'hui l'élection si l'Académie les avait élus.

De tous les griefs dirigés contre l'Académie, le plus fréquemment invoqué était sa courtisanerie envers le roi. C'était une compagnie de courtisans qui pouvait, en ce genre, donner des leçons à tous les Dangeau. N'est-ce pas elle qui avait mis au concours cette question: "Quelle est celle des vertus du roi qui mérite le plus d'être louée?"

On était bien loin de ce style et de ces sentiments lorsque Grégoire, reprochant au "bon Fénelon" d'avoir fait un traité sur la direction de la conscience d'un roi, ajoutait: "Comme si les rois avaient de la conscience! Autant eût valu disserter sur la douceur des bêtes fauves."

Le tort des hommes aveuglés par la passion est de vouloir toujours juger sans tenir compte des temps et des milieux. N'en déplaise aux niveleurs de 1793, l'esprit libéral qui s'était manifeste dans le sein de l'Académie au moment de sa création officielle subsista pendant toute sa durée. Il s'associait chez elle à une admiration pour le roi dont nous ne comprenons plus la nature. L'Académie voyait la France dans le roi. A cette époque de l'histoire, on n'était puissant qu'à condition d'être dépendant. Ce qui est indiscutable, c'est que les académies entourées d'honneurs par la monarchie étaient devenues peu à peu de véritables aristocraties. Elles avaient aux yeux des republicains le double défaut d'être des corporations, et des corporations privilèges, très entichées de leurs privilèges. Un usage introduit par Colbert, ou plutôt par l'abbé Bignon, son neveu et son représentant dans le gouvernement des sociétés savantes, divisait les Académies des Inscriptions, des Sciences et des Lettres en trois classes d'académiciens: les honoraires, les pensionnaires et les élèves; ce qui constituait un privilège

dans le privilège. Seule l'Académie française avait énergiquement refusé de subir l'affront de ce règlement.

L'Académie française avait toujours eu dans son sein, depuis sa création, des ducs, des maréchaux, des évêques, des magistrats de cours souveraines. Ces grands seigneurs apprenaient à traiter les gens de lettres comme des égaux ; mais, en même temps, les gens de lettres apprenaient à se croire grands seigneurs. Ils se donnaient des compliments les uns aux autres, pour s'exercer à leur fonction principale qui était d'encenser le roi et le ministre. Les compliments sont devenus nos discours de réception : Voltaire n'était pas tendre pour eux : "Ce que j'entrevois dans ces beaux discours, dit-il, c'est que le récipiendaire ayant assuré que son prédécesseur était un très grand homme, que le cardinal de Richelieu était un très grand homme, le chancelier Seguier un assez grand homme, le directeur lui répond la même chose, et ajoute que le récipiendaire pourrait bien aussi être une espèce de grand homme, et que pour lui, directeur, il n'en quitte pas sa part" ; et plus loin : "La nécessité de parler, l'embarras de n'avoir rien à dire et l'envie d'avoir de l'esprit sont trois choses capables de rendre ridicule même le plus grand homme."

La Convention pouvait-elle souffrir l'existence d'un corps qui passait son temps à célébrer les vertus des rois, qui était lui-même un corps privilégié, et qui comptait dans son sein des membres investis d'un double privilège? C'était l'aristocratie de l'esprit, mais c'était une aristocratie. La Montagne et la Plaine étaient d'accord pour la renverser.

Il s'était pourtant passé vers le milieu du XVIII^e siècle un fait considérable qui aurait pu modifier les jugements des révolutionnaires. Voltaire était entré à l'Académie. Les académiciens s'étaient vaillamment défendus. Voltaire fut refusé deux fois. Enfin, il entra ; et dès ce jour l'Académie lui appartint. Il avait déjà son journal qui était l'*Encyclopédie*. L'*Encyclopédie* entra avec lui à l'Académie, qui fut ainsi transformée par anticipation en véritable Académie des Sciences morales et politiques. Il y fit nommer successivement Duclos, d'Alembert, Marmontel, Condillac, Morellet. Il échoua pour Diderot. Il s'en plaint vivement, et avec raison du reste, car si Diderot n'est pas précisément un génie académique, c'est sans conteste un homme supérieur. Voltaire écrit à l'abbé d'Olivet : "Tâchez, mon cher maître, de nous donner un véritable académicien à la place de l'abbé de Saint Cyr et un savant à la place de l'abbé Salier. Pourquoi n'aurions-nous pas cette fois-ci M. Diderot? Vous savez qu'il ne faut pas que l'Académie soit un séminaire et qu'elle ne doit pas être la Cour des pairs. Quelques ornements d'or à notre lyre sont convenables ; mais il faut que les cordes soient à boyau et qu'elles soient sonores."

Voltaire n'était pas accoutumé aux échecs et avait pris sa revanche. Il avait le gros de son armée à l'Académie française, il avait à l'Académie des Sciences Condorcet, d'Alembert, Fontenelle. L'Académie des Inscriptions était plus résistante, mais il avait pénétré partout. Il était l'oracle des cercles de précieuses dont l'influence avait remplacé l'influence décroissante de la cour. Mme. de Lambert, Mme. de Tencin, Mme. Du Deffand, Mlle. de Lespinasse, Mme. Geoffrin, Mme. Du Chatelet recevaient ses inspirations. Il était l'ami (intermittent) du roi de Prusse, le correspondant (et le flatteur) de la grande Catherine. Il avait traité Corneille de haut : il se croyait plus pathétique que Racine. En philosophie il tenait tête au clergé, tout en faisant ses pâques à Ferney et en dédiant au pape sa tragédie de *Mahomet*. Quand on le juge à présent, on ne peut s'empêcher de voir en lui un précurseur de la Révolution. Voltaire et toute l'armée qu'il commandait avaient, en effet, semé les idées révolutionnaires, mais ils avaient cru évoquer un génie ; et quand ils furent en face de lui (je parle des lieutenants de Voltaire, car il était mort en 1778), il leur sembla qu'ils avaient évoqué le diable.

Ils s'arrêtèrent en chemin, et devinrent, par cela même, les plus grands ennemis de leurs anciens amis. On pourrait ici parodier cette grande parole : "Il y a plus de joie dans le ciel pour un pécheur qui se repent. . . ." et dire : "Il y a plus de [illegible] dans l'armée révolutionnaire pour un ami qui s'arrête en chemin . . ."

Les [illegible], dont on oublia les services, eurent le sort des parlements et du clergé. Grégoire, dans un rapport ridiculement emphatique, proposa la suppression des académies, tout en demandant que "au milieu des décombres, le sanctuaire des arts, [illegible] le temple de la liberté, présentât la réunion organisée de tous les savants et de tous les moyens de science."

"[illegible], disait-il, la République française fera son entrée dans l'univers. En ce jour où le soleil n'éclairera qu'un peuple de frères, les regards ne doivent plus rencontrer sur le sol français d'institutions qui dérogent aux principes éternels que nous avons consacrés, et cependant quelques-unes, qui portent encore l'empreinte du despotisme ou dont l'organisation heurte l'égalité, avaient échappé à la règle générale : ce sont les académies."

Deux ans après avoir congédié les académies avec cette politesse, la Convention faisait une grande, une très grande chose. Elle les rétablissait, et en les rétablissant, elle leur faisait subir une modification profonde. Le rêve d'une assemblée unique des savants et des artistes, des poètes et des philosophes, déjà conçu par la Constituante, devenait une réalité. Jamais la fraternité des lettres, des sciences et des arts n'avait été affirmé avec cet éclat. La nouvelle institution réunissait en un faisceau toutes les forces de la passion et de la pensée. Elle créait au-dessus de la société vulgaire, occupée des soins de la vie, une sorte de monde à part d'où sortiraient sans cesse pour éclairer l'humanité, pour la fortifier et la charmer, des vérités et des chefs-d'œuvre. L'Institut ne participerait pas au gouvernement, il ne serait pas chargé de l'enseignement. Son action serait d'une nature plus haute ; elle s'exercerait par l'exemple. De même que le Dieu d'Aristote meut sans être mû et peut ignorer le monde auquel il donne la vie, il suffit aux savants et aux poètes d'être, et d'être connus. Leurs œuvres produisent le mouvement, et en même temps elles le règlent par l'admiration qu'elles inspirent.

Daunou parlant au nom de la Convention disait : "Nous avons emprunté de Talleyrand et de Condorcet le plan d'un Institut national, idée grande et majestueuse dont l'exécution doit effacer en splendeur toutes les académies des rois. . . . Ce sera en quelque sorte l'abrégé du monde savant, le corps représentatif de la république des lettres, un temple national dont les portes toujours fermées à l'intrigue ne s'ouvriront qu'au bruit d'un juste renommée."

Cette union majestueuse et féconde de tout ce qu'il y a d'éternel dans le sentiment et la pensée n'est pas la seule grandeur de l'institution nouvelle. Les académies jusque-là avaient été purement locales. Elles se recrutaient dans une seule ville et représentaient le mouvement scientifique ou littéraire de la ville où elles étaient nées. Mais l'Institut créé en 1795 pour remplacer les académies n'est pas un institut parisien, c'est un institut national, c'est l'Institut de France. La constitution de l'an III, dont la formule est fidèlement reproduite par la constitution de l'an VIII, le déclare en ces termes solennels : "Il y a pour toute la République un Institut national chargé de recueillir les découvertes, de perfectionner les arts et les sciences."

Pourrais-je oublier, en présence de cette assemblée, que la Convention nationale ouvrit les portes de son Institut non seulement à tous les Français, mais à tous les grands hommes quelle que fût leur origine? De même que Louis XIV récompensait le génie à quelque nation qu'il appartînt, la Convention créa dans le sein de l'Institut l'ordre des associés étrangers, qui nous permet d'inscrire sur nos listes d'honneur Huyghens, Newton, Leibniz, et plus près de nous Rossini et Meyerbeer.

L'œuvre de la Convention n'est donc pas la reproduction des anciennes académies déguisées sous des noms nouveaux et modifiées dans les détails secondaires de leur organisation. C'est bien une œuvre nouvelle. C'est une création, une puissante création. C'est l'Académie de France, représentant à la fois les lettres, les sciences et les arts. Elle contient les anciennes académies, mais en les enfermant dans une synthèse nouvelle et forte. C'est notre droit et notre devoir, en ce jour de fête, d'adresser également nos hommages aux anciennes académies qui ont préparé l'Institut et à l'Institut qui contient et complète les anciennes académies.

L'œuvre de la Convention est assez belle pour que nous puissions avouer maintenant que l'Assemblée avait été moins heureuse dans les détails d'exécution que dans la conception première. Elle avait tout exagéré : sa propre autorité sur l'Institut et l'autorité de l'Institut sur les membres qui le composaient. Elle ne connaissait pas la liberté. Elle disait comme Louis XIV : "L'État, c'est moi," et quand elle avait usurpé tous les pouvoirs, elle disait : "Nous voilà libres."

La première faute de la Convention, en ceci comme en bien d'autres choses, fut son amour immodéré de la table rase. Elle avait supprimé les académies qu'elle pouvait modifier en les conservant. Elle supprima jusqu'à leurs noms dans la réorganisation qu'elle fit ensuite. On a dit d'elle avec vérité qu'elle avait

peur des mots. Elle remplaça ces noms illustres par les appellations vulgaires de première, seconde, troisième classe, et ne réussit par ces changements qu'à voiler les traditions historiques. Elle effaça un autre nom qui aurait dû lui être particulièrement sacré. Ayant à placer la philosophie dans la classe des sciences morales et politiques qu'elle organisait pour la première fois, elle remplaça ce nom, qui pouvait rappeler les croyances spiritualistes, par celui d'Analyse des sensations et des idées, qui ne rappelait que Condillac. Chaptal, qui déjà en 1801 reprochait à l'organisation de l'Institut "de s'être beaucoup trop écartée de ce que l'expérience avait montré de perfection dans la composition de nos anciennes académies," fit en 1803 un nouveau projet où il se montra plus équitable et plus habile que la Convention. Il proposait même de rétablir le nom des anciennes académies, dont la France s'honorait depuis plus d'un siècle, et qui étaient devenues le modèle des institutions savantes et littéraires formées successivement dans tous les Etats de l'Europe. Le Conseil d'Etat ne voulut pas y consentir. Il approuva le fond de la proposition, mais il ne rendit pas leurs noms aux anciennes compagnies.

L'Académie des Sciences morales et politiques, fondée pour la première fois en 1795, et qui formait la seconde de l'Institut, eut une courte existence. Le Premier Consul avait dit un jour à M de Ségur : "Vous présidez la seconde classe de l'Institut ; je vous ordonne de lui dire que je ne veux pas qu'on parle de politique dans les séances. Si la classe désobéit, je la casserai comme un mauvais club." Fidèle jusqu'au bout à son aversion pour ceux qu'il appelait les idéologues, quand il procéda à la réorganisation de l'Institut en 1803, il supprima la deuxième classe par prétérition, en supprimant son nom et en répartissant ses membres dans les autres classes.

La première faute de la Convention fut donc de renoncer à des noms vénérables et à un passé illustre ; elle fit une seconde faut dans le mode d'élection qu'elle adopta. Les candidats furent présentés par la classe dans laquelle s'ouvrait une vacance, et l'Institut en corps fut chargé de choisir entre les candidats ainsi présentés. Jamais la compétence ne fut traitée avec un pareil mépris. Un comédien décidait de l'élection d'un mathématicien. Un peintre jugeait un philosophe. On reconnaît bien là une assemblée qui admettait les juifs au nombre des votants pour l'élection des évêques catholiques. L'élection par classe ou académie ne fut établie qu'en l'an XI, sur le rapport de Chaptal.

La Convention commit une troisième faute. Les deux premières avaient pour effet d'exagérer l'unité ; celle-ci exagérait et faussait le caractère national de l'Institut. C'était l'Institut de France ; on voulut qu'à ce titre il fût composé par moitié de Parisiens et de provinciaux. Il aurait suffi de dire que les choix pouvaient se porter également sur les hommes du premier mérite, qu'ils eussent leur résidence à Paris ou ailleurs. Non. Il sembla plus radical de partager par moitié. Cela cessait même d'être juste, car Paris ne comptait que 500,000 habitants et la province en avait 25 millions. Et cela n'était pas raisonnable : car un homme d'élite peut désirer le séjour de Paris à cause des bibliothèques, des musées, des amphithéâtres et de tous les autres moyens d'étude. On avait admis une section de l'art dramatique : trois comédiens parisiens, trois comédiens de province. Tout le monde sait que les grands comédiens peuvent se former en province, mais qu'ils ne peuvent y rester. Ils n'y trouvent ni les traditions, ni les écoles, ni les auxiliaires, ni le public dont ils ont besoin, ni les ressources materielles. On en peut dire autant des érudits, des artistes. La regle de résidence était sévère alors ; plus sévère qu'elle ne l'a été depuis. Un membre nommé pour représenter Paris et qui s'établissait définitivement en province était obligé de donner sa démission. Destutt de Tracy, qui habitait Auteuil, fut nommé membre non résident.

La plus grande erreur commise est peut-etre le règlement intérieur des travaux imposé par décret organique.

Le gouvernement s'attribuait dans ce règlement le droit de requérir l'avis des classes de l'Institut. C'est surtout à l'Academie des Sciences qu'il adressa ses réquisitions. Il la consulta sur les voitures couvertes destinées au transport des malades, sur le perfectionnement à apporter au régime des hopitaux, sur le système monétaire, sur la manière d'accorder l'ere de la République avec l'ère vulgaire, sur un nouveau boulet, sur un taffetas huilé propre à faire des manteaux pour les troupes, sur l'idée de fair établir plusieurs rangées de canons sur un seul affût, sur la conservation des eaux potables à bord des navires, sur la nservation des biscuits et des légumes en mer. Il y avait aussi des questions pour les autres classes, meme des questions philosophiques, ce qui tendait à faire une doctrine d'État. Rien n'est plus contraire à la philosophie et à la vraie politique, et rien ne peut nuire davantage aux progrès de la science et à l'éclat des académies. Dans un corps littéraire bien organisé, l'autorité de chaque membre s'accroît de celle de la compagnie, mais a condition qu'il n'en résulte aucune ingérence de l'académie ni des gouvernements sur le travail individuel. Quand le général Cavaignac, pour réfuter les socialistes de 1848, demanda a l'Académie des Sciences morales et politiques des petits livres populaires, l'Académie échoua, il faut le dire résolument, quoiqu'elle se fût adressée aux plus grands noms de la science. Un grand esprit ne se retrouve pas dans un travail fait sur commande : il faut au génie l'air de la liberté.

Ce droit de réquisition n'était pas seulement attribué au gouvernement, il appartenait aussi au public. Tout auteur pouvait exiger une analyse de son livre, tout inventeur un examen de sa découverte. Ainsi les académiciens n'étaient plus maitres de leur temps. Je ne m'étonne plus qu'on leur eût attribué deux costumes : un costume de cérémonie et un costume de travail. On ne voyait pas qu'assujettis au service de tout le monde, il ne leur restait plus de temps pour le service de la science.

Je ne veux pas tout énumérer. Je citerai pourtant la suppression des secrétaires perpétuels, remplacés par deux secrétaires semestriels : c'était ôter aux académies leur unité, leur vie. Chaptal, en 1801, parlant des anciennes académies, disait : "Le même homme suivait tous les details de l'Académie, en devenait l'historien, et attachait d'une manière toute particulière la gloire de son nom à celle du corps dont il etait l'organe ; il y avait plus de suite dans l'administration, plus de célérité dans l'exécution, plus d'ordre dans la marche, et on ne peut pas nier que le rétablissement d'un secrétaire perpétuel pour chaque classe de l'Institut, en rouvrant une carrière qui présente tant de grands hommes pour modeles, ne contribuât à la gloire de ce corps et aux progres des sciences." Et plus tard, en 1803, il revenait à la charge - "Le rétablissement de ces places, disait-il en parlant des secrétaires perpétuels, fera renaître une branche d'éloquence très négligée depuis dix ans et donnera aux travaux académiques cet esprit de suite, cet enchaînement de faits et de pensées qui, seuls, peuvent fixer l'époque des découvertes et tracer avec exactitude l'histoire des connaissances humaines."

Tout en déclarant qu'elle renonçait au passé académique, la Convention, par la force meme des choses, avait conservé à son Institut tous les avantages dont avaient joui les anciennes académies. Elle maintenait, la reconnaissance de l'Institut par l'État et l'intervention de l'État dans les règlements intérieurs de l'Institut. Elle laissait à l'Institut le local des academies, la bibliothèque, la participation à la nomination des professeurs dans les grands établissements littéraires et scientifiques. L'Institut a conservé cette prérogative et présente encore aujourd'hui des candidats pour le Collège de France, le Muséum, l'Académie, de Rome, les Écoles de Rome et d'Athènes, l'École des Chartes, l'École des Langues orientales vivantes, le Conservatoire des Arts et Métiers, l'Observatoire, l'École Polytechnique. Il a conservé les impressions gratuites et les prix connus sous le nom de prix du budget, auxquels s'ajoutent à présent des prix fondés par l'initiative privée, dont le chiffre annuel n'est pas inférieur à 524,500 francs. Le 29 messidor an IV la Convention donnait aux membres de l'Institut une indemnité annuelle de 750 myriagrammes de froment, et le 19 thermidor suivant, elle décidait que "sur cette indemnité, il serait distrait a l'égard de chacun des membres une somme egale à la valeur de 150 myriagrammes de froment, pour être répartie par forme de droit de présence entre les assistants aux séances, tant générales que particulieres, de chaque classe."

En 1803, sur le rapport de Chaptal, on permit aux membres de l'Institut d'être de plusieurs académies à la fois, et par conséquent de réunir plusieurs indemnités. "C'est, dit Chaptal, le moyen d'ouvrir aux hommes distingués plusieurs routes à la gloire et à l'aisance, et par conséquent le moyen de multiplier et d'agrandir les talents."

Le droit de cumuler les académies subsiste, mais on a enlevé celui de cumuler les indemnités. Nous en sommes restés aux 750 myriagrammes. Ceux d'entre nous qui font partie de plusieurs académies ne touchent l'indemnité qu'une seule fois. Nous nous vantons de n'être pas riches.

Les membres de l'Institut, quand on fixait à 750 myriagrammes de froment, c'est-à-dire, pour parler en langage intelligible, à 1500 francs, l'indemnité qui devait les delivrer de

… les soucis de la vie, n'imaginaient pas dans leurs rêves les plus ambitieux qu'ils auraient un jour à eux l'un des plus beaux palais du monde, avec une galerie de tableaux, une bibliothèque créée d'une seule venue par un grand écrivain doublé d'un érudit consommé, des bois, des eaux, et tout un monde de beaux souvenirs.

Peut-être est-il bon de rappeler ici, pour expliquer à la fois notre richesse et notre pauvreté, que tous les dons faits à l'Institut sont faits à la science ou aux pauvres. Les membres de l'Institut n'en profitent jamais. Une nouvelle donation n'est pour eux qu'un surcroît de travail. L'empereur Napoléon III voulut un jour élever à 5000 francs l'indemnité annuelle de 1500 francs, ce qui faisait une quantité froment fort respectable. L'Institut, consulté, exprima sa reconnaissance, et refusa.

On a dit quelquefois que tous les efforts de la Révolution pour transformer les académies n'avaient été qu'une illusion. Le 8 août 1793, on les supprime; le 25 octobre 1795, on les remplace par l'Institut. On s'aperçoit sur-le-champ que cet Institut, à force d'être nouveau, n'est pas viable. Dès 1803 on commence à le réformer; les réformes se multiplient d'année en année, et à quoi aboutissent-elles? à supprimer la plupart des innovations, à refaire les anciennes académies et même, en 1816, à leur rendre leur nom.

Ceux qui parlent ainsi ne voient pas qu'il reste à la Révolution la gloire d'avoir établi un lien étroit entre les académies, d'avoir compris la solidarité des lettres, des sciences et des arts, d'avoir mis les académies en communication plus intime avec le public et de leur avoir donné de nouveaux et sérieux moyens d'influence.

Des anciennes compagnies, des remaniements opérés sur les nouvelles est résulté l'Institut actuel, où la protection de l'État n'exclut pas la liberté des membres, où chacun répond seul de sa doctrine, où la solidarité d'honneur qui unit tous les membres rend impossibles les excentricités, où tous les travaux tendent à la manifestation de la vérité et aux triomphes de l'art, où tous les membres rassemblés sans être confondus se prêtent une mutuelle assistance sans jamais tomber dans la confusion: un corps enfin qui réunit dans une juste proportion l'autorité et la liberté, et qui mérite d'être proposé comme modèle à toutes les nations civilisées.

J'ose ajouter, Messieurs, que votre présence ici, celle du chef respecté de l'État, et l'éclat qui en résulte vont donner à l'Institut national de France une consécration nouvelle.

Le monde assiste depuis vingt-cinq ans à un singulier spectacle. D'une part les gouvernements multiplient avec une sorte de rage les préparatifs de guerre. Ils construisent des forteresses, ils coulent des canons, ils emplissent les arsenaux de projectiles; ils imposent le service militaire dans l'armée active à tous les jeunes gens sans exception, au point de vider les écoles, de désorganiser les services publics et particuliers, d'ôter à l'agriculture et à l'industrie les bras dont elles ont besoin. Ils retiennent les citoyens dans les liens du service militaire jusqu'à quarante-cinq ans. Il semble que la bataille doive se livrer demain.

En même temps tous les philosophes, tous les publicistes, les hommes d'État, les souverains eux-mêmes protestent à grands cris de leur horreur pour la guerre. Ils veulent la paix, il la leur faut pour rendre au travail la sécurité, à l'intelligence ses droits et à l'année son printemps. On fonde de toutes parts des ligues pour la paix, on assemble des congrès pour protester contre la paix armée, plus ruineuse et plus meurtrière que la guerre.

Hélas! ces congrès n'apportent que des vœux. C'est beaucoup et ce n'est rien. Ils apportent des vœux, je n'ose pas dire qu'ils apportent des espérances.

Ce qu'il faut à l'humanité, ce ne sont pas des paroles, ce ne sont pas des soupirs, ce sont des actes. Ce qui fera renaître la fraternité entre les hommes, ce sont grands travaux faits en commun, de grand services rendus à l'humanité.

Le voilà devant vos yeux, le congrès de la paix! Voilà le congrès où la vérité est aimée pour elle-même, quel que soit le pays où elle éclate, où la poésie est adorée dans toutes les langues, où les grandes découvertes excitent le même enthousiasme, quelle que soit leur origine, et où l'on ne connaît d'autre émulation que celle de bien faire. La patrie de l'éternelle vérité et de l'éternelle beauté est aussi la patrie de la paix.

Associés et correspondants de l'Institut de France, vous n'emporterez pas seulement d'ici le souvenir des chaleureuses sympathies qui vous ont accueillis. Nous emporterons tous, de cette réunion fraternelle, un redoublement d'amour pour la paix, pour les sciences qui la fécondent et pour les arts qui l'embellissent; et nous travaillerons, chacun dans notre coin préféré de l'atelier universel, à la prospérité de la maison, c'est-à-dire au bonheur de l'humanité.

"BARISÀL GUNS" AND "MIST POUFFERS."

In the delta of the Ganges, dull sounds, more or less resembling distant artillery, are often heard. These are called "Barisàl guns"; but I do not know the meaning of the term.[1] The object of this note is to draw the attention of the readers of NATURE to this mysterious phenomenon, and to the similar "mist pouffers" of the Belgian coast.

My attention was for the first time drawn to the subject some days ago by a letter from M. van der Broeck, Conservator of the Museum of Natural History of Belgium. He writes[2] of certain "curious aerial or subterranean detonations, which are pretty commonly heard, at least, in Belgium and in the north of France, and which are doubtless a general phenomenon, although little known, because most people wrongly imagine it to be the sound of distant artillery.

"I have constantly noticed these sounds in the plain of Limburg since 1880, and my colleague of the Geological Survey, M. Rutot, has heard them very frequently along the Belgian coast, where our sailors call them 'mist pouffers' or fog dissipators.

"The keeper of the lighthouse at Ostend has heard these noises for several years past: they are known near Boulogne, and the late M. Houzeau spoke of them to my friend M. Lancaster. More than ten of my personal acquaintances have observed the fact.

"The detonations are dull and distant, and are repeated a dozen times or more at irregular intervals. They are usually heard in the day-time when the sky is clear, and especially towards evening after a very hot day. The noise does not at all resemble artillery, blasting in mines, or the growling of distant thunder."

M. van der Broeck, after referring to the "Barisàl guns," says that he was disposed to regard the noises as due to some peculiar kind of discharge of atmospheric electricity. "But my colleague M. Rutot believes the origin to be internal to the earth. He compares the noise to the shock which the internal fluid mass might give to the earth's crust."

Mr. Clement Reed has informed M. van der Broeck that he believes similar noises are heard on Dartmoor, and in some parts of Scotland. I was not previously aware of anything of the kind in these islands.

Before any systematic observations are undertaken, it will be useful to form some general idea of the frequency of these sounds and of their geographical distribution.

Will any of the numerous readers of NATURE in various parts of the world give us an account of their experiences in this matter? G. H. DARWIN.

NOTES.

The Municipal Council of Paris have decided to erect a statue to Sir Isaac Newton. We cannot imagine the London County Council paying a similar graceful tribute to the greatness of one of France's renowned investigators, say Laplace or Lavoisier, but we dare to suggest that the action of the Paris Municipality ought to be reciprocated.

Munificent gifts to science and education continue to be reported from America. *Science* states that the Spring Garden Institute of Philadelphia has received £20,000 from the heirs of Samuel Jeanes, who supported the Institute with great generosity during his lifetime; Earlham College at Richmond, U.S., has

[1] T. D. La Touche, Brit. Assoc. Rep. 1890, p. 800.

[2] I give a free translation and abridgement of the letter.

received £5000 from Mr. M. H. White and Mr. F. T. White, in memory of their father; a new laboratory, built at a cost of £8000, is almost completed for the departments of bacteriology, histology, and pharmacy in the Medical College of the University of Minnesota; and by the will of Colonel W. L. Chase, £1000 is bequeathed to Harvard College to establish a scholarship in the medical school.

DURING the recent Zoological Congress, at one of the meetings of the Section of Comparative Anatomy and Embryology, Prof. A. Kovalewsky bore testimony to the greatness of Huxley in words of which the following is a translation: "In the list of men of science who expressed their intention to take part in our Congress will be found the name of Thomas Huxley; but death has prevented him from being among us. In the person of Huxley, science has sustained a great loss. We do not know any other investigators of our century who had the talent of foresight to such an extent as Huxley. It was he who, properly speaking, founded modern embryology by demonstrating the homology of the germinal layers of Vertebrates with the ectoderm and endoderm of Cœlenterates. It was he who supported Darwin in the publication of the fundamental work on the origin of species, and it was he who was the fervent propagator of the views therein contained. The two names of Darwin and Huxley have built up the story of the scientific world."

THE following gentlemen have been recommended for election as the Council and officers of the London Mathematical Society at the annual meeting to be held on November 14:—President, Major P. A. Macmahon, F.R.S.; Vice-Presidents, Prof. M. J. M. Hill, F.R.S., M. Jenkins, A. B. Kempe, F.R.S.; Treasurer, Dr. J. Larmor, F.R.S.; Secretaries, R. Tucker and A. E. H. Love, F.R.S. Other members—H. F. Baker, G. H. Bryan, F.R.S., Lieut.-Colonel A. J. Cunningham, Prof. Elliott, F.R.S., Dr. Glaisher, F.R.S., Prof. Greenhill, F.R.S., Dr. Hobson, F.R.S., Prof. W. H. H. Hudson, and F. S. Macaulay. It will be seen that Mr. Jenkins, after thirty years' service, has retired from the office of Secretary, on the score of his delicate state of health. The Society held its first meeting on January 16, 1865, and on the retirement of Mr. H. M. Bompas (November 20, 1865), Mr. Jenkins was requested to act as Secretary until the annual general meeting (January 15, 1866), when he and the late G. C. de Morgan were elected joint Secretaries.

NEXT Sunday will be Museum Sunday—the fourth arranged by the Sunday Society. On that day special sermons or discourses will be given by many leading men in London and the provinces, in support of the Society's object, viz. the opening of museums, art galleries, libraries, and gardens on Sundays. The cause is a righteous one, and deserves every support. A number of special exhibitions will be held in the afternoon of Sunday, and these, together with the museums and other places of interest which will be open, make a fairly extensive list of institutions opened in the manner advocated by the Society. The list clearly indicates that the public opinion of the country is really on the side of a rational observance of the weekly day of rest.

THE death is announced of Prof. H. Hellriegel, in his sixty-fourth year. His investigations in the domain of agricultural science produced many valuable results, and it was his researches that led to the discovery of the fixation of free nitrogen by leguminous plants, through the medium of micro-organisms in the root nodules.

THE death of Dr. Robert Brown deprives science of one of her most popular exponents. Dr. Brown was born at Campster, Caithness, in 1842. He studied in the University of Edinburgh, and afterwards in the Universities of Leyden, Copenhagen, and Rostock, receiving from the latter the degree of Doctor of Philosophy. In 1861 he visited Spitzbergen, Greenland, and the western shores of Baffin's Bay, and made a number of valuable observations. Between 1863-66 he travelled for scientific purposes in many of the least-known parts of America, and some of the Pacific Islands, from the West Indies and Venezuela to Alaska and Behring Sea Coast, as botanist of the British Columbia Expedition and commander of the Vancouver Island Exploring Expedition, during which he introduced various new plants into Europe, and charted all the interior of Vancouver, then unknown. In 1867 he visited Greenland, making, with Mr. E. Whymper, the first attempt by Englishmen to penetrate the inland ice, and formed those theoretical conclusions regarding its nature, afterwards confirmed by Nansen and Peary. Dr. Brown afterwards travelled extensively in the Barbary States of North Africa. Settling down in Scotland he was successively lecturer on geology, botany, and zoology in the Royal High School, Edinburgh, and Heriot Watt College, Edinburgh, the Mechanics' Institution, Glasgow, and elsewhere. He was an honorary or ordinary member of many learned societies in this country, in America, and on the continent. In 1876 he removed to London, in order to devote himself entirely to literary work, and for the greater part of the period, from that time to his death, was on the editorial staff of the *Standard*. He was the author, or part author, of about thirty volumes, and of a large number of scientific memoirs, articles, and reviews.

THE thirty-fourth annual meeting of the Yorkshire Naturalists' Union was held yesterday at York Museum, and the presidential address was delivered by Dr. R. Braithwaite, on "The Study of Mosses."

MR. ARCHIBALD DENNY, of Dumbarton, has accepted the presidency of the Institution of Junior Engineers, in succession to Mr. Alexander Siemens, and will deliver his presidential address on Friday evening, November 1, at the Westminster Palace Hotel; Prof. A. B. W. Kennedy, Past-President, in the chair.

THE Epping Forest Free Local Museum, established by the Essex Field Club in Queen Elizabeth's Lodge, Chingford, will be declared open next Saturday afternoon, by Mr. R. C. Halse, Chairman of the Epping Forest Committee of the Corporation of London. Short addresses on the subject of local museums will be given by Mr. A. Smith Woodward, and others.

THE Session 1895-96 of the Royal Geographical Society, for the evening meetings, will commence on November 11, when an account of the progress of the Jackson-Harmsworth Arctic Expedition will be given by Mr. A. Montefiore. On November 25, a paper on the Fœroe Islands will be read by Dr. Karl Grossmann; exploration in the Central Alps of Japan will be described by the Rev. Walter Weston on December 9; and movements of the earth's crust, by Prof. John Milne, F.R.S., on January 6. Other papers which may be expected after Christmas are the following: Journey across Tibet, by St. George R. Littledale; exploration in the Alps of New Zealand, by E. A. Fitzgerald; our knowledge of the oceans, by Dr. John Murray; the geography of the English lake district, by J. E. Marr, F.R.S.; the cañons of Southern Italy, by R. S. Günther; British Central Africa, its geography and resources, by Alfred Sharpe. The following subjects, among others, will be submitted for consideration and discussion at the special afternoon meetings:—The construction and uses of globes, by J. Y. Buchanan, F.R.S.; the struggle for life in the North Polar region, by A. Trevor-Battye; an attempt to reconstruct the maps of Herodotus, by J. L. Myres. Under the joint auspices of the Society and the London University Extension, Mr. H. J. Mackinder is giving a course of twenty lectures on the principles of geography, at Gresham College.

The *Weekly Weather Report* of the 20th inst. shows that the temperature over the British Islands during the week was abnormally low for the time of year, the deficit ranging from 4° in the Channel Islands, and 6° in the east of England and north of Ireland, to 8° in the north-west of England and the south of Ireland. The lowest shade readings were recorded towards the end of the week, and ranged from 18° in the south-west of England to 21° in the south of England and 22° in the Midland counties. The continuous occurrence of frost for several nights in the neighbourhood of London during the current month of October has exceeded any previous record in that month at Greenwich during the last fifty years.

A description of a luminous cloud, observed at Mojanga, Madagascar, on September 27, by Mr. Stratton C. Knott, H.M. Vice-Consul, has been forwarded to us by Mr. R. H. Scott, F.R.S. The phenomenon was seen at 8.20 p.m. as a narrow streak of what appeared more like mist than cloud. It came out of a cumulus cloud in the south, a few degrees above the horizon, and extended through the tail of Scorpio across two-thirds of the sky, which was quite clear excepting some cumulus on the southern and eastern horizon. The streak travelled at a rapid rate eastwards, but its base seemed to be stationary: as it crossed the moon, it caused a sort of double corona. As the cloud got lower on the eastern horizon, although always maintaining the same length, some cumulus passed under it, partly obscuring it, and a few minutes later the streak was lost altogether in the cumulus on the eastern horizon. At the time of the observations the weather was perfectly calm, but soon after this streak had passed, cumulus commenced to ascend from the eastward, and the sky soon became nearly overcast.

The polarisation of the light emitted by incandescent bodies has not yet been fully investigated. Arago, indeed, made some experiments on incandescent iron, platinum, and glass, but these were only qualitative, and did not extend to liquids. Mr. R. A. Millikan publishes, in the *Physical Review*, an account of some careful tests of light emitted by glowing solids and liquids with a view to discover the laws of its polarisation. This phenomenon is exhibited strongly by incandescent platinum, silver, and gold, and by molten iron and bronze. A somewhat feebler polarisation is shown by copper, brass, lead, zinc, and solid iron. The most significant result is that polarisation is minimum with rays emitted normally to the surface, and maximum at a grazing emission. This indicates that the vibrations take place in a plane at right angles to the emitting surface. To show the phenomenon at its best, a smooth surface is essential. Glass and porcelain also emit polarised light, but to a lesser amount. Fluorescent bodies do the same, so that evidently a high temperature is not necessary. In the case of uranium glass it is the green reflected light which is polarised, and not the blue incident light diffused from the surface.

The main facts of Lieut. Peary's work in North Greenland are described by Prof. R. D. Salisbury in *Science* of October 11. Prof. Salisbury was one of the party which relieved Lieut. Peary, the other members being Mr. Emil Diebitsch, Dr. J. E. Walsh, Mr. T. Boutillier, and Prof. L. L. Dyche. During his Arctic residence, Lieut. Peary mapped a considerable stretch of the coast of West Greenland from Cape Alexander on the north to Cape York on the south, and his results show a number of remarkable differences with earlier charts of the same region. His map locates the position of nearly one hundred glaciers, where but ten were represented on the published chart. In addition to the map, Mr. Peary kept a series of meteorological records, and made observations of the behaviour of [illegible], and in this way has come into possession of facts which are not without significance in connection with the problems of glaciology. He made careful measurements of the rate of motion of one of the most active glaciers of the region, and carried them through a sufficiently long period of time to give them especial value. He took back to the United States two large and choice meteorites from the coast east of Cape York, and these will undoubtedly prove of interest. His studies of the Eskimos of North Greenland will, when published, form an important contribution to ethnology.

So far as concerns the results accomplished by the members of the Peary relief party of this year, Prof. Dyche was successful in getting large numbers of birds and mammals at various points along the coast. He also secured an abundant supply of walruses, reindeer and seals, and a smaller number of narwhals, and saw much of the west coast of Greenland between latitude 64° and 78° 45′, at close enough range to study its geographic features to advantage. Stops were made near the parallels of 67°, 69°, 70°, and at many points between 75° 45′ and 77° 45′. At all these points geographical and geological studies were carried on. The eastern coast of America was also seen for a considerable distance, especially from Ellesmere Land south to 71° 30′, and most of the coast of the island of Disco. Prof. Salisbury, who accompanied the party in order to study glacial geology, observed in detail many glaciers between 75° 45′ and 77° 45′ on the Greenland coast, and made some determinations of significance concerning glacier motion. A considerable body of evidence was gathered touching the former extension of the ice cap of Greenland. Determinations were also made at several points concerning recent changes of level of the land.

A recent number of the *Pioneer Mail*, published in Allahabad, contains an interesting article on immunity from scorpion and snake venom. Much attention has been directed in India to the experiments, which have lately been so successfully carried out, on immunity to snake-bites artificially induced by the introduction of gradually increasing doses of the venom into the system. The writer of the article in question does not regard this achievement as any really new discovery, being convinced that the traditional immunity claimed to be possessed by the Indian snake-charmers is simply due to the fact that they have frequently been accidentally bitten by cobras and karits, and having survived the first attack experienced no evil effects from the subsequent bites. This he states as the result of his personal acquaintance with many Madari Jogis and Fakirs, some of whom he knew had been bitten as many as five times. It appears, however, that cases of reputed immunity to scorpion stings are also well known, and one of these he had the opportunity of himself carefully testing. Hearing of a Mahomedan Fakir who had established a reputation for himself in this respect, he determined to investigate the case, and banish, if possible, all chance of trickery and deception being practised. He therefore dug up the scorpions himself, and these formidable creatures he describes as being from 5 to 7 inches long, with claws on them like lobsters. These scorpions the Fakir was told to irritate (not by pinching the end of the tail, which is a well-known way of preventing them stinging!), but by touching them on the part of the body indicated: the result was that each one of them stung him strongly enough to draw blood, but the man was apparently none the worse. "There could be no doubt," he writes, "as to the perfect genuineness of the exhibition." This incident should encourage M. Calmette to continue his experiments on artificially inducing immunity to the sting of scorpions by means of gradual doses of the scorpion venom. It is to be hoped that the successful investigations which have so far been made on artificially procuring immunity to snake-bites, may obtain the official recognition which they deserve, and that such immunity may not in the future be confined to the selected few or so-called charmed individuals.

In a lecture recently delivered by Dr. W. J. van Bebber, at Lubeck, and printed in the *Annalen der Hydrographie und Maritimen Meteorologie* for September, he discusses the possible means of improving storm-warning signals. As Dr. v. Bebber has charge of the weather service at the Deutsche Seewarte, his views on the subject carry considerable weight. He points out that notwithstanding constant exertions to place weather prediction on a sound and trustworthy basis, the solution of the question remains in a somewhat unsatisfactory condition. He makes the following suggestions for the furtherance of the object in view, most, if not all, of which have already been discussed at various meteorological conferences, and have fallen through on the score of expense or other hitherto insuperable difficulty:—(1) Extension of telegraphic communication westward (Färoe, South Greenland, &c.). This proposal was advocated by the late Captain Hoffmeyer. (2) Acceleration of exchange of telegrams, by the introduction of the "circuit-system." By this means the telegrams in America are received, and warning messages despatched within two hours of the time of taking observations. (3) More frequent information, by means of telemeteorography, or the connection of self-recording instruments with central offices. The practicability of this method has been put to test in the Netherlands, and the subject was recently discussed by the International Meteorological Committee at Upsala. (4) Exchange of telegrams between neighbouring signal stations; this plan has been found to work successfully in Germany and America, and by its means more recent information is obtained by the seafaring community as to the sudden approach of stormy weather. (5) The popularisation of weather knowledge among the public by means of weather charts, and (6) the preparation of an atlas of types of weather. The number of charts required would be at least 500 or 600. This subject has been suggested by Mr. Abercromby and others.

A new method of measuring the resistance of an air-gap during the passage of a spark has been devised by M. Victor Biernacki, and is described in the current number of the *Journal de Physique*. In the case of a Hertzian resonator in unison with an exciter, the forced vibrations and the natural vibrations of the resonator (the presence of which, according to Poincaré and Bjerkness, explain multiple resonance) have the same periodic time, and according to Bjerkness's theory these two vibrations are in oppose phase. In order that these two vibrations may entirely destroy each other, it is necessary that they be equally damped—that is to say, that the resistance of the exciter and resonator should be equal. The author has verified this consequence of the real presence of these two sets of vibrations in the resonator, by steadily increasing the resistance of the resonator, starting with a resistance less than that of the exciter. In this way he has succeeded in entirely destroying the vibrations in the resonator, and according to theory at this moment the resistances of the exciter and resonator must be equal. Since these had the same dimensions, and were made of the same material, but the spark-gap in the exciter was replaced by a liquid resistance R, it follows that the value of R, which corresponds to the completed extinction of all vibrations in the resonator, is equal to the resistance of the spark-gap in the exciter. The resistance R consists of a glass tube filled with a solution of copper sulphate of various strengths. A Geissler tube or a bolometer is employed to indicate the presence of the vibrations in the resonator. As the dilution of the sulphate of copper solution is increased, the vibrations in the resonator decrease in intensity. These die out, and on further dilution reappear. For a spark-gap of 1 cm. the resistance R varied between 300 and 800 C.G.S. units. With a spark-gap 0·4 m.m. long, however, the resistance is found to be between 1200 and 1500 C.G.S. units. This increase of the resistance as the spark diminishes is very curious; but it is important to notice that the decrease in the length of the spark is accompanied by a change in other properties of the spark. When the terminals of the spark-gap are near together it is very difficult to obtain a straight and white spark, the spark generally being slightly violet in colour and ramified in appearance. With a longer spark-gap, however, it is much easier to obtain a spark which is white in colour and non-ramified, and which passes with a sharp noise. It is a spark of this latter character which Hertz found to be best suited to his classical experiments, and the fact established by the author that such a spark really offers less resistance than a short violet spark, affords an explanation of Hertz's observation.

With the title "The People's Stonehenge," a slim little pamphlet, by Mr. J. J. Cole, has been published by Mr. J. Doney, Sutton, Surrey. The pamphlet contains ten reproductions from photographs of the objects at Stonehenge; and these, with the short descriptive text which accompanies them, brings out the points of interest in the most wonderful of our archæological remains.

Astronomers should be grateful to Messrs. W. Wesley and Son for the excellent catalogue of works on astronomy just published as No. 124 of the Natural History and Scientific Book Circular. The classification is very elaborate, the books being arranged under no less than twenty-four headings. In each section the books follow the alphabetical order of authors' names. Both the arrangement of the sections and the divisions adopted are admirable, and reflect great credit upon the compilers. Bibliophiles well know that a bookseller's catalogue is a mine of information, and they will be joined by astronomers in appreciation of the efforts of Messrs. Wesley and Son to produce a full and accurate list of works on celestial science.

The *Proceedings* of the American Philosophical Society for January, 1895, reached us at the beginning of this week. Among other papers contained in it we notice a description (with four plates) of an old "Horologium Achaz," or Dial of Achaz, by Mr. J. F. Sachse; a paper on "The Significance of the Jugal Arch," by Mr. D. D. Slade; a note proving that thin leaves of gold, similar to those exhibited by Mr. J. W. Swan at the Royal Society in June 1894, were produced by Mr. A. E. Outerbridge seventeen years ago (on this matter, see Mr. Outerbridge's claim for priority in Nature, vol. li. p. 608, 1895); a paper by Dr. D. G. Brinton on the "Protohistoric Ethnography of Western Asia," and the "Fourth Contribution to the Marine Fauna of the Miocene Period of the United States," by Prof. E. D. Cope.

Messrs. Macmillan have just issued the first part of the "History of Mankind," by F. Ratzel, in which the learned author states what the task of ethnography is, and describes the situation, aspect, and numbers of the human race, together with a series of preliminary observations on the rise and spread of civilisation, religion, language, &c. Where possible he illustrates his remarks by pictures of genuine "savage" remains, and his theories have usually a good substratum of fact. It is, of course, too early to pass a final opinion on the work: but we believe that it supplies a want among the increasing number of people who need a popular history of the beginnings of the human race, and an intelligible account of the conditions under which our primitive ancestors lived. The part before us is printed in good type on excellent paper, and contains a coloured plate of a Bosjesman family, and a map of North and South America, besides several illustrations scattered throughout the text.

Dr. A. B. Meyer has sent us a memoir (*Abh. u. Ber. des K. Zoolog. u. Anthropol. Ethn. Museums zu Dresden* 1894–95) on a

Brown Chimpanzee. The Chimpanzee described and figured in it is a young female living in the Zoological Gardens at Dresden, remarkable for its reddish brown hair, projecting eyes, and very bright-coloured skin. Dr. Meyer discusses at some length the numerous species, sub-species, and varieties of the Chimpanzee that have been proposed by various authors, and decides that his "Brown Chimpanzee" can be referred to none of them. It may be quite true that no one has previously described such a brown form of the Chimpanzeee, but nearly all mammals, especially the Quadrumana, are subject to lighter variations in colour, and we see no reason why this should not be the case with the Chimpanzee. It would seem, therefore, that Dr. Meyer has done well in not giving his Brown Chimpanzee a new scientific name. It appears that nothing is known of the history of the specimen, nor of its exact locality.

The third and concluding portion of Kubary's monograph of the ethnography of the Caroline Archipelago is now published under the editorship of Herr J. D. E. Schmeltz, who has, as usual, spared no pains to bring out the memoir in a way which its value demands. The complete work consists of 306 pages and fifty-five plates; many of the latter are coloured. They are executed by Trap, which is a sufficient guarantee of their excellence. It is to the famous but ill-fated Museum Godeffroy that we owe the inception of this investigation. At a later date Kubary was connected with the Kgl. Museum fur Volkerkunde in Berlin. The present section, which deals with house- and canoe-construction in the Pelau Islands, maintains the level of conscientious care and minute detail which characterised the two former parts. The structure of the houses and canoes is illustrated to scale by drawings in plan, elevation, and section; and details of fastenings and joinery are given on a larger scale. We have thus all the information necessary to understand structural details, which latter are too often lacking in the descriptions and illustrations of travellers. Some houses are richly decorated with carved and painted ornamentation; but unfortunately Kubary was not impressed with the importance of this branch of ethnography, and so we are left in ignorance as to the significance of the figures and patterns. What a pity it is that the ethnography of our Possessions and Protectorates in various parts of the world is not investigated and published in such a manner as this!

We have received from Mr. J. Eliot, F.R.S., Meteorological Reporter to the Government of India, parts viii. and ix. of vol. v. of "Indian Meteorological Memoirs," containing the discussion of hourly observations made (1) at Deesa, a military station in the Palanpur State on the Banas River; and (2) at Kurrachee, the Port of Sind. The latter station is about three miles from the sea, and has a most complete exposure. The period embraced is 1875–93, and forms part of the proposed discussion of the observations recorded at twenty-five observatories. For each station the mean observed hourly values of the various elements, and the differences from the mean of the day, have been calculated, and from these the diurnal variations have been resolved into four component harmonic oscillations by the application of Bessel's formula, while the epochs and values of the diurnal maxima and minima have been computed by the method used by Dr. Jelinek, to the second approximation. The investigation of the materials at each station is of itself a most laborious and thorough piece of work, and the complete discussion will be probably unequalled in magnitude. The importance of the whole investigation can scarcely be over-estimated, and when the results are collated they cannot fail to throw much light upon the causes which underlie the periodic variations over this area, and their dependence on various physical and local conditions.

Xylose, like arabinose, gives two optically active stereoisomeric acids on treatment with hydrocyanic acid and subsequent hydrolysis. Of these, gulonic acid has long been recognised; the second, idonic acid, has recently been isolated, and its derivatives prepared by Emil Fischer and Irving Wetherbee Fay (*Berichte*, 1895, No. 14, p. 1975). The series is remarkable as containing the last missing members of the mannitol group of acids, sugars, and alcohols. The names—idonic acid, idose, iditol, and idosaccharic acid—assigned to these substances have been derived from "idem," and given on account of the symmetrical geometrical formulæ expressing their constitution. From the formula of l-idose,

$$\begin{array}{c} \quad\quad\;\; H \;\; OH \;\; H \;\; OH \\ CH_2OH\,.\,C\,.\,C\,.\,C\,.\,C\,.\,COH \\ \quad\quad\;\; OH \;\; H \;\; OH \;\; H, \end{array}$$

it is evident that hydroxyl and hydrogen are similarly related to each of the asymmetrical carbon atoms, and that only the *same* product, racemic acid, and no inactive tartaric acid can be produced by oxidation wherever the molecular chain is broken; in this respect a remarkable contrast to the other hexoses being shown. From the product of the action of hydrocyanic acid on xylose, gulonic acid was separated by repeated crystallisation of the lactones; the syrupy dark liquid resulting on evaporation of the mother liquor was diluted and treated with brucine. The product on evaporation and addition of a large quantity of alcohol gave a crystalline mass of brucine idonate. When purified and recrystallised from methyl alcohol it formed colourless prisms, or long rectangular plates, which melted with decomposition between 185° and 190° (corr.). The acid was prepared from the brucine salt by addition of barium hydrate and subsequent decomposition of the barium salt with sulphuric acid. Ultimately a relatively good yield of idonic acid and its lactone was obtained as a colourless syrup, which dissolved easily in water, and with difficulty in alcohol, and was insoluble in ether. 0·5 gram dissolved in 3·5 grams of water gave a rotation of 5·2° in a decimeter-tube. The normal idonates of calcium, barium, cadmium, and lead are amorphous and very easily soluble in water. A characteristic cadmium double salt, $(C_6H_{11}O_7)_2Cd.CdBr_2.H_2O$, crystallises in fine, colourless needles. The corresponding sugar, l-idose, was prepared from the syrupy mixture of idonic acid and its lactone by reduction with $2\frac{1}{2}$ per cent. amalgam after dilution with ten times its volume of ice-cold water. The sugar was isolated in the usual way as a syrup, which could not be completely purified through lack of material. A 10 per cent. sterilised solution did not ferment with yeast. The osazone, prepared as usual, could not be distinguished from gulosazone. The alcohol of this series, l-iditol, was obtained by the further reduction of idonic acid by sodium amalgam, first in acid, and finally in alkaline solution. It was purified by formation of the benzaldehyde compound, recrystallised from acetone in colourless needles of the composition $C_6H_8O_6(CH.C_6H_5)_3$. The purified compound, on treatment with sulphuric acid and alcohol, gave the alcohol as a colourless syrup very easily soluble in water. The idosaccharic acid was formed from idonic acid by treatment with nitric acid, and yielded crystalline calcium and copper salts.

The additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Sir Egbert Sebright, Bart.; a King Parrakeet (*Aprosmictus scapulatus*) from Australia, presented by Mr. George Cawson; two White Storks (*Ciconia alba*), European, presented by Sir Charles Payne, Bart.; an Ortalan Bunting (*Emberiza hortulana*), European, presented by Mr. H.

C. Martin: two Hybrid Widgeons (between *Mareca penelope* and *Anas boschas*), bred in England, presented by Mr. Wellesley Taylor; a Cape Viper (*Causus rhombeatus*), two Rufescent Snakes (*Leptodira rufescens*) from South Africa, presented by Mr. J. E. Matcham; a Great Kangaroo (*Macropus giganteus*) from Australia, deposited: two Hunter's Spiny Mice (*Acomys hunteri*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

RUTHERFURD'S STELLAR PHOTOGRAPHS.—The pioneer work of the late Dr. Rutherfurd in photographic star charting is gradually assuming a form which gives the results a high scientific value. In 1890, Dr. Rutherfurd presented his original negatives, many of them taken more than twenty years ago, to the Columbia College Observatory, New York, together with some thirty volumes of measures of certain star photographs, and Prof. J. K. Rees was authorised to arrange for the discussion of the photographs. After Dr. Rutherfurd's death in 1892, his son, Rutherfurd Stuyvesant, generously provided funds for continuing the reduction and publication of the measures. The results obtained for the stars of the Pleiades group, and for the stars about β Cygni have already been published, as well as an investigation of the parallaxes of μ and θ Cassiopeiæ. To these are now added two papers giving full details of an investigation of the parallax of η Cassiopeiæ, and of the reduction of positions of sixty-two stars in the neighbourhood (*Ann. New York Acad. Sci.*, vol. viii. 301, 381). Using three pairs of comparison stars, the parallax deduced for η Cassiopeiæ is 0″·443 ± 0″·043; or, taking six pairs, 0″·465 ± 0″·044 (see NATURE, vol. lii. p. 61). In view of the difficulty of getting comparison stars suitably situated either with respect to position angle, or distance, it was considered desirable to take a larger number than usual, and hence six pairs were reduced, being all that were sufficiently impressed on the plates in both seasons of the year. Only the three pairs which lead to the first-named value, however, are so situated with reference to the parallactic ellipse as to give good coefficients for the parallax.

RADIAL VELOCITIES OF SATURN.—The recent spectroscopic investigations of the velocities in the Saturnian system furnish an admirable illustration of the accuracy at present attainable in this department of astronomical research. Prof. Keeler, M. Deslandres, Prof. Campbell, and Dr. Belopolsky have each in turn directed their attention to the planet, and the following table brings together the different results obtained, and compares them with the computed velocities:—

	Equatorial velocity of planet.	Excess of velocity for inner edge of ring.
Keeler ...	10·3 km. per sec. ...	3·6 km. per sec.
Deslandres ...	9·4 ,, ...	4·7 ,,
Campbell ...	9·77 ,, ...	3·13 ,,
Belopolsky ...	9·4 ,, ...	5·5 ,,
Computed ...	10·3 ,, ...	3·9 ,,

It thus appears that in the hands of competent observers, the photographic methods now employed for the determination of velocities along the line of sight may be relied upon to give values which are correct to within one kilometre per second; while for results depending upon the measurement of more than one velocity, a little greater latitude must be allowed.

In reply to the objection of M. Deslandres and Prof. Seeliger, that the spectroscopic results do not strictly prove the meteoritic constitution of the ring, Prof. Keeler has pointed out that any other explanation which is consistent with them can only be regarded as artificial, or inherently improbable (*Ast. Nach.* 3313). If the ring were composed of concentric solid rings, a line in the spectrum would be made up of short straight lines, like an end view of a flight of stairs. Prof. Keeler does not consider his own photographs capable of showing more than ten such subdivisions, for if the number were greater than this, the step-like structure of the lines would be destroyed by unavoidable errors in guiding; but up to a certain point the effect would still be apparent in the widening of the lines. He finds, however, that the definition of the lines in the spectrum of the ring is less affected by guiding errors than that of the lines of the planet, as might be expected if the lines were smooth curves such as would be produced in the case of a meteoritic ring.

THE CAPE OBSERVATORY.—Dr. Gill's report of the work done at the Cape Observatory during 1894 has been distributed. It opens by pointing out that the chief desideratum in astronomy during the past decade has been an adequate provision for the study of astrophysics in the southern hemisphere. As the readers of NATURE are aware, Mr. Frank McClean, F.R.S., has given to the Cape Observatory a splendid equipment for such work, so the need has been met, and a harvest of results may be looked for as soon as the instrument is erected. With reference to this generous gift, the report says that the telescope will have a photographic object-glass of 24 inches aperture and 22½ feet focal length, and be provided with an objective prism of the same aperture having a refracting angle of 7½°. Mounted parallel to this there will be a visual telescope of 18 inches aperture and of the same focal length as the photographic telescope. The equatorial mounting will have complete circumpolar motion for within 30° of the zenith; and will be sufficiently elevated to allow of a slit spectroscope suitable for determining motion in the line of sight. Such a spectroscope will also be provided by Mr. McClean, together with an observatory of light construction. The instrument has been for some time under construction by Sir Howard Grubb, and will probably be completed before the end of 1896.

Among the work done with the astro-photographic telescope, we notice that, after rejecting all plates of insufficient exposure, or which are otherwise faulty, only 70 of the plates for the Catalogue, out of 1632 areas assigned to the Cape, remain to be done. Of the chart plates, 263 have been passed as satisfactory.

Measures of the diameters of the photographic discs of a variable star in Vela, together with those of nine comparison stars, prove the former to be a variable of the Algol type, its period being about 5d. 22h. 24m. 4s. A complete discussion of the light curve and period will shortly be undertaken.

The researches on the solar parallax have been carried forward, three sections of the work, on the observations of the minor planets Victoria and Sappho, having been passed through the press. The manuscript of the definitive discussion of the observations of Victoria has been sent to the printers; while the computations of the solar parallax from the observations of Sappho and Dr. Elkin's reductions of the observations of Iris are completed.

THE INSTITUTION OF MECHANICAL ENGINEERS.

AN ordinary general meeting of the Institution of Mechanical Engineers was held on the evenings of Wednesday and Thursday, October 23 and 24, at the Royal United Service Institution, Whitehall, the Council having lent their new theatre for the purpose. The building of the Institution of Civil Engineers, where the Mechanical Engineers have held their London meetings for years, is now in process of rebuilding. It is to be hoped, however, that the Institution of Mechanical Engineers will, before long, have their own premises.

There were three papers down for reading on the first day of the meeting:

"The Electric Lighting of Edinburgh," by Henry J. Burstall.

"Report on the Lille Experiments upon the Efficiency of Ropes and Belts for the Transmission of Power," translated by Prof. David S. Capper.

"Observations on the Lille Experiments upon the Efficiency of Ropes and Belts for the Transmission of Power," by Prof. David S. Capper.

The chair was taken on each evening at 7.30, by Prof. Alexander B. W. Kennedy, F.R.S. On the first evening Mr. Burstall's paper was read and discussed.

The electric lighting of Edinburgh is in the hands of the Corporation. It was decided upon in 1893, when the work of designing and superintending the scheme was entrusted to Prof. Kennedy, the President of the Institution. From an electrical point of view the city consists of two districts. In one the houses are close together, and the demand for light may be expected to be fairly concentrated; in the other it will be more scattered. Having regard to the different districts to be served, and taking into account all the local circumstances, it was decided, after comparison of the various systems of supply and distribution which could be used, to adopt a low tension three-wire system

for the central and northern district, and an alternating-current high tension system for the southern and eastern district, both systems being worked from one central station, and under the same control and management. A good site was found for the central station between the Caledonian Railway and Dewar Place.

The boiler-house is designed to contain seventeen boilers, of which at present only six are in place. They are of the dry-backed marine type, each 10½ feet mean diameter and 12 feet long, with two Purves flues 3¼ feet inside diameter, and 166 tubes of 3 inches internal diameter. The boilers are of steel with wrought-iron tubes. On the top of the boilers are fitted super-heaters, each having two nests of tubes enclosed between the top of the boiler shell and a fire-brick casing above. Each consists of thirty-two vertical flat coils of wrought-iron tube 1½ inches diameter. Sinclair's mechanical stokers are fitted to each boiler, and are driven by an electric motor. The main steam pipe forms a complete ring round the present boilers. This ring joins the engine-room main at two points, and is provided with valves, so that the failure of any one pipe will put only the corresponding boiler out of use. The pump-room contains at present one duplex steam pump and two three-throw pumps driven electrically, each pump specially designed to run with a large range of speed, and for this purpose can be connected with either the 230-volt or the 115-volt mains. A Kennedy water meter is connected with one range of feed pipes, so that the whole of the water going to the boilers can be measured. In the pump-room is placed the electric motor for driving the mechanical stokers with its counter shaft. The coal brought in the railway trucks is at present stored in the east end of the boiler-house; on the station being extended, the coal will be stored over the boiler-house, and let down through shoots to the mechanical stokers. In designing the plant at present in the boiler-house, provision for extensions has been kept in mind, and the arrangements are such that new plant can be added at any time.

The engine-rooms are side by side, forming really one room divided by a line of columns which carry the roofs and the beams for the travelling cranes. The engine-room next the boiler-house is reserved for the low-tension plant, the other contains the high-tension plant. A platform, raised 4 feet above the engine-room floor level, runs the whole way across the west end of both engine-rooms; and on this are placed the switchboards and regulating gear for both the low- and high-tension systems. The machinery at present in the low-tension engine-room consists of eight engines, four of 100 I.H.P., two of 250 I.H.P., and two of 360 I.H.P., with their dynamos: and provision is made for eight more engines of 360 I.H.P., in the future. All are Willans central-valve engines driving their dynamos direct. All the dynamos are two-pole shunt-wound machines with drum armatures, all wound to give 270 volts, except two which are driven by two 100 I.H.P. engines: these two are wound to give 135 volts, being used as balancing machines on the three-wire system. The steam-piping forms, with part of the boiler-house ring, a complete ring round the low-tension engine-room, and is connected with the boiler-house ring at two points. The main ring is 8 inches internal diameter throughout. The straight lengths are of steel, with thick flanges screwed and brazed on; the tee pieces and valve boxes are of cast-iron; and the bends of copper with steel flanges. All bends are of large radius, and no expansion joints are used or required. The engines are erected in pairs, and are connected with the main ring by two long copper bends. The pipes are slung by long rods from brackets fixed on the walls or columns, so as to allow free movement. The main exhaust pipes are of cast iron, and are led through a Berryman feed heater, in the boiler house to the chimney. Provision is made for three more heaters when required. The whole of the machinery is fixed on a concrete foundation block 7½ feet thick, which is separate from the foundations of the walls.

The main leads from the dynamos are drawn through curved wrought-iron pipes let into the concrete, into chases in the centre of the engine foundation block, along which they are carried to the channel under the switchboard platform. The leads from the field winding off the machines are also carried in the same manner to their regulating resistances; the switches for these resistances are fixed upon the handrail on the platform in front of the switchboard, the leads from the resistances being brought up through the posts of the handrail. The switchboard, and the whole of the apparatus for regulating the dynamos and the mains used for the distribution of the current, are placed on one platform, and are directly under the eye of the engineer in charge. The switchboard consists of seven slate panels, each about 7 feet high, and stands 4 feet from the west wall of the engine-room. The arrangement of the switchboard and conductors was next described.

The battery-room has a fire-proof floor covered with acid resisting asphalt. The battery consists of 132 cells of the new Crompton-Howell 31-plate type. It is divided up into two half-batteries, positive and negative, and is arranged in two tiers on four rows of stands, which are of cast-iron, with wooden longitudinal bearers carrying the cells; the eight hospital cells are arranged on separate stands. All the cells are similar, and have each a nominal capacity of 1000 ampere hours, the normal rate of discharge being 200 amperes. The battery has ample capacity to meet the whole of the load of the station from daylight till the evening; thus during the summer time it can do the lighting during more than half the twenty-four hours. The high-tension portion of the station consists at present of only two engines and alternators with their switchboard, and the rectifiers for arc-lighting with their regulating arrangements and switchboard; but in the immediate future, this plant will be considerably extended. Each of the alternators is driven direct by a Willan's three-crank engine of 150 I.H.P., on the same bed-plate. The alternators are of the "Portsmouth" type, with some modifications necessary owing to their increased speed of 450 revolutions per minute. Their armatures are stationary, and are of great strength; the core, consisting of sheet-iron segments, is solidly bolted into the framing of the machine, with the coils threaded through holes in the sheet-iron, well insulated, and completely enclosed in brass boxes. The field magnets revolve, and consist of two heavy cast-steel discs, having on their circumference claws projecting sideways alternately over the field winding, which is between the discs, and is well protected from injury. The exciting current is taken from the low-tension switchboard at 230 volts, and is only a few amperes. The alternators work at an electromotive force of between 2000 and 2200 volts with a frequency of 52½ figures complete alternations per second. Opposite to the alternators, and standing on the same foundation block, are placed the Ferranti rectifiers for the series arc-lighting. These are three in number, one for each of the two circuits, and one to spare.

In the three-wire system of distribution for the northern and central districts, the electromotive force between the two outer conductors, positive and negative, is 230 volts, while that between the middle wire and the positive or negative is 115 volts. The latter is the electromotive force of the lamps on the consumers' premises, no trouble being now experienced in obtaining glow-lamps to work at this electromotive force, or even higher. The feeders from the station are connected to the distributing mains at sixteen points. They consist of two conductors only, the positive and negative; the middle wire is inter-connected throughout as much as possible, and is brought back from three districts on the system. The cables are put in parallel at the station, and one connection only is made to the switchboard. The positive and negative sides respectively of all the feeders are put in parallel at the switchboard; but any feeder or feeders can be put on a separate machine if required. As far as possible, consumers in each street and district are balanced against one another by connecting them alternately between positive and middle wires, and between negative and middle wires; large consumers have all three wires taken into their premises, and their lights balanced against one another in a similar manner. But however carefully this balancing is done, it is impossible to get a really accurate balance; the "out of balance" current varies from hour to hour, and even from minute to minute, and is different on different days of the week. The amount out of balance is compensated for at the station by means of the balancing machines, one of which can be put on either side of the system; the balancing during the light load is done from the battery alone. Light wires, forming potential leads or pilot wires, are brought back from all three conductors at each feeding point, and are connected to the feeder volt meters on the switchboard; so that the pressure at the feeding points at any part of the system is directly known at the station, and the necessary regulation made for keeping the electromotive force constant. The distributing mains are brought back to the station, but are used only for the supply of light and power there; no regulation is done on the mains anywhere, except to the feeding points. No high tension feeders or distributing mains have yet been laid, but will be added later.

In regard to road-work, practically the whole of the distributing mains are laid as cable insulated with india-rubber

heavily braided, drawn into Doulton stoneware casing under the footways, and into either Crompton-Davis cast-iron casing or cast-iron pipes under the roadways. At all crossings, and at intermediate places on the foot-ways, brick junction boxes are built. Wherever sufficient space has been found under the footways, the feeders have been laid as bare copper strip, carried on stoneware insulators in concrete culverts. Across all roads, and where there has not been sufficient space for culvert, the feeders are laid in Siemens armoured cable, laid direct in the ground. All the feeders have been designed to have a total drop of 44 volts at full load Potential leads, by which each feeding point is connected back to the station, consist each of three sets of wires, insulated with specially prepared paper, laid up together and covered with the same material, a lead tube being drawn over the whole. All of the cable connections are made by cone connectors, sweated on to the cables, and fitting into gun-metal connecting blocks.

The author concluded by stating that he had endeavoured to describe the arrangement of the plant and mains, and any details in their design and construction which might be of interest, without discussing general principles or the advantages or disadvantages of any individual system.

In the discussion which followed the reading of the paper, the chief point raised was the advisability of using a dual system of supply; but the author very well disposed of the objections raised in this direction by pointing out that the area to be dealt with consisted of two districts differing widely in character. For one the high pressure alternating system was most desirable, and for the other a low tension system. It is easy, as Mr. Burstall said, to maintain that either system is wrong if the disadvantages of that system are given undue prominence, and the advantages of the rival system are brought prominently forward. Of course, the benefit of the high-tension alternating system consisted in the saving of copper, but that was a thing that perhaps would not work out in practice exactly in the same way as it was presented in theory. On paper, a larger main was required for low-tension transmission, but practically there was often no saving in copper As a matter of fact there is, however, an economy in material in the feeders. It becomes a question of balance of advantages, whether certain points shall be sacrificed to the saving of copper. Mr. Burstall stated that so far as Edinburgh was concerned, no less than six schemes were worked out in detail before it was decided to adopt the plans described in the paper. The discussion also ranged over the question of superheating steam, the efficiency of feed pumps, and various other engineering details, which, however, it is not necessary here to consider in connection with an electrical paper, especially as no new facts of importance were added to one's information upon these matters.

The second paper, on the Lille experiments with ropes and belts, does not need any extended notice at our hands. The Société Industrielle du Nord de la France had the question of transmission of power brought before them by a paper by M. V. Dubreuil, and it being considered advisable to obtain more information on this subject, a commission was appointed, and the Institution of Mechanical Engineers was invited to send a representative. Various trials were made with ropes and belts under different conditions. Owing to the want chiefly of dynamometer records, no very exact figures could be deduced from the experiments, so far as the actual power transmitted was concerned. An effort was made to make the experiments comparative as between ropes and belts, by keeping the experimental conditions in both cases constant. It is somewhat doubtful, however, to what extent these efforts were attended by success. Generally speaking, it may be said that the results arrived at showed the power absorbed by transmission to be equal, whether ropes or belts were used. This conclusion, however, must be accepted with some reserve.

Prof. Capper in his paper, commenting on the experiments, attempted to arrive at some conclusion as to the efficiency of the whole system. His figures were excellently worked out, but he himself acknowledged that the data upon which he based his calculations might be open to question Although, therefore, the experiments may not be of great use to future designers of machinery, the thanks of English engineers are none the less due to the Société Industrielle for their courtesy in inviting the Institution of Mechanical Engineers to send a representative to watch the proceedings. The Institution is also to be congratulated in being able to send so competent an observer as Prof. Capper as their representative.

A long discussion followed the reading of the paper, which occupied the rest of the evening. Perhaps the most interesting part of it were the remarks of Mr. Crompton, who stated that the question of transmission by ropes and belts did not possess any longer the importance it once did, as within a few years the silent, flexible connecting rod, called electricity, would supersede all other methods of transmission, so that ropes and belts would only be found exhibited in museums, as mechanical curiosities of a past era.

RECENT FISHERY LITERATURE.

The general report for 1894 of the Fishery Board for Scotland contains evidences of the revolution which is quietly but steadily effecting a complete change in the methods of the fishing industry. There is a further falling-off in the number of fishermen and fishing boats engaged in the herring and line fishing. The sailing craft continue to give way before steam trawlers and steam liners, and the competition for the best markets is bringing about an increased centralisation of the fishing industry. The smaller and healthier creeks and villages are being gradually depopulated, and the larger ports are becoming overcrowded. The summer herring fishing is being forsaken for line fishing, which can be prosecuted all the year round. Steam liners are consequently increasing rapidly in number, and during the past year have proved most successful. Indeed, in spite of the falling off in the means of capture, the decrease in the total quantity of fish landed, as compared with the returns for 1893, amounted to only 19,000 cwts. This state of things may be attributed to the fact that the steam trawlers and liners are able to proceed much further out to sea than sailing craft, and are able to fish over fresh grounds where large catches are frequently obtained. Being larger and stronger, moreover, these vessels are to a great extent independent of wind and weather, which seriously affect the movements of the smaller sailing boats. Herring were locally plentiful, and of a quality never excelled within modern times. They were especially abundant in the Orkneys and Shetlands—where the catch was double that of 1893—and in the Campbelltown area; but the herring fishery in the Hebrides was again a failure, and this is the more to be regretted as Stornoway, the most important centre for ling, also exhibited a large falling off in the returns of the latter fish. It is gratifying to notice a slight increase in the returns of flat fish, especially in view of the complaints of the depletion of grounds frequented by them. The increase may, however, be due to the hauls made by steam trawlers working on fresh and more distant grounds. For the first time in the Board's returns, a table is given of the number of persons engaged in Scotch fisheries on sea and land; there are more than 117,000 people taking some part in the various branches of the industry.

The report of the same Board on Salmon Fisheries shows that the season of 1894 was in most districts below the average. On the other hand, salmon disease appears on the whole to have been less prevalent during 1894 than in the previous year.

In an interesting and amusing article on the North Sea fisheries ("Journal of the Marine Biological Association," vol. iii., 1895), Mr. Holt devotes especial attention to the question of the destruction of immature fish. The fact that there has been a diminution of the fish supply during recent years seems to be thoroughly established, although the improved boats and methods of fishing render this decrease less striking than might otherwise be the case. The alleged cause of this diminution is over-fishing, that is to say over-trawling, but inshore trawlers, shrimpers, and other fishermen do not appear to be blameless in the matter. During some years' residence at Grimsby, Mr. Holt has collected statistics on this question. They are necessarily incomplete, as time and opportunity did not permit of wide investigation; they are, however, fairly complete in the case of the plaice, one of the most important of our flat fishes, and Mr. Holt's evidence concerning this fish is very striking. He states that during a whole year's trawling on the North Sea grounds 57 per cent. of the plaice brought to shore were sexually immature, and had thus never had a chance of reproducing their species, and so contributing to the maintenance of the supply. In the Conference of 1892 the size limit for plaice was made 10 inches; they are marketable at this size, although not sexually mature, for Mr. Holt finds that as a rule North Sea plaice are not mature until they attain a length of 17

[illegible] Fish vary in size under different conditions and in different areas, and on the south-west coast the limit of size for maturity in plaice is 13 inches according to Mr. Cunningham. An immense number of the small plaice brought to market are caught on the eastern grounds; and this area forms also a nursery and spawning haven for turbot, brill and soles. The number of plaice above 10 inches on these grounds is inconsiderable; and if a size limit of 13 inches for plaice brought to market were enforced, even during the spring and summer only, such a law would suffice to keep trawlers off these grounds, which would thus be left unmolested. In conclusion, Mr. Holt considers various remedial measures for checking the depletion of the North Sea grounds, and of enabling the fish supply to recover; but the only practicable method of attaining this end at present is by legislation based on the principle of the size limit.

THE FORMATION OF BACTERIAL COLONIES.[1]

The author has examined the details of development of the colony from a single spore, in numerous species, by employing microscopic plate-cultures, which can be kept under observation under a one-twelfth and even a one-twentieth oil immersion, or by making pure *Klat. hpraparate* of the growing colony on cover-slips covered with a thin film of gelatine.

He finds many factors of importance in affecting the form, extent, rapidity of growth, and other characters of colonies. The consistency of the gelatine, the presence of moist films on the surface of the gelatine, the rate of (slight) liquefaction, &c., all being of importance in explaining the shapes, &c., of submerged colonies—"whetstone shaped," moruloid, spherical, or lobed colonies—the mode of emergence and spreading over the surface of the gelatine, the formation of radiating fringes, iridescent [illegible], &c.

Exposure to light during the development of liquefying colonies may profoundly affect their shape and other properties, a phenomenon closely connected with the retardation of liquefaction and growth. Pigment bacteria may give rise to perfectly colourless races when cultivated under certain conditions, and the colour restored by again changing the conditions, a fact which the author has not only confirmed with red forms, but which he shows to be true of a violet bacillus. Species commonly described as non-motile show active movements under certain conditions, and the sizes of bacteria are not constant in different regions of one and the same colony. Details have been worked out for series of types, the extremes of which differ considerably in liquefying power, and essential difference in the appearance of a colony may depend on the amount of liquefying power evinced.

Some curious cases of travelling films, the lobes and contorted [illegible] of which move like amœbæ over the surface of the gelatine, were also examined.

The facts point to (1) differences in colonies even of one [illegible] may depend on much more subtle differences in cultures than are usually recognised; (2) varietal differences may occur [illegible] bacilli of the same species (isolated from the river), due to different vicissitudes the two individuals have been subjected [illegible] during their sojourn in the water; (3) the difficulties met with in diagnosing "species" of bacteria with the aid of works of [illegible] authority, are partly due to varieties of the same species [illegible] recorded by different observers under different names, and the author thinks some more consistent pre-arranged plan of [illegible] out the characters of such forms should be developed by [illegible] than at present exists.

A False Bacterium.

The author has isolated from the Thames a form which gives [illegible] ordinary reactions of a bacterium in plate-cultures and [illegible] gelatine, agar, potato, broth, milk, &c.

It is a rod-like form, 1 μ thick, and up to 2 or 4 μ long, stains [illegible], and cannot be distinguished from a true Schizomycete by the methods in common use.

On cultivating it under high powers—one twelfth and one-twentieth oil immersions—from the single cell, however, it is [illegible] to form small, shortly branched mycelia, the growth and segmentation of which are acropetal. This turns out to be a minute oidial form of a true fungus.

Its true nature can only be ascertained by the isolation and culture through all stages from the single cell, according to the original methods of gelatine cultures of Klebs, Brefeld, and De Bary, which preceded and suggested the methods employed by bacteriologists; and the facts discovered raise interesting questions as to the character of alleged "branching" bacteria on the one hand, and the multiple derivation of the heterogeneous group of micro-organisms, termed bacteria in general, on the other.

[1] [illegible] Section K of the British Association [illegible] H. Marshall Ward, F.R.S.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The following have been appointed Examiners in Natural Science for the current academical year:—Physics: Dr. O. J. Lodge, F.R.S., and Mr. L. R. Wilberforce. Elementary Physics: Mr. H. F. Newall and Mr. S. Skinner. Chemistry: R. Meldola, F.R.S., and Mr. W. J. Sell. Elementary Chemistry: Mr. F. H. Neville and Dr. S. Ruhemann. Mineralogy: Prof. N. Story-Maskelyne, F.R.S., and Mr. H. A. Miers. Geology: Prof. G. A. J. Cole and Mr. H. Woods. Botany: Dr. H. M. Ward, F.R.S., and Mr H. Wager. Zoology: Prof. S. J. Hickson, F.R.S., and Mr. S. F. Harmer. Elementary Biology: Mr. A. C. Seward and Mr. J. J. Lister. Anatomy: Prof. A. Macalister, F.R.S., and Prof. A. M. Paterson. Physiology: Mr. W. B. Hardy and Prof. W. D. Halliburton, F.R.S. Pharmaceutical Chemistry: Mr. A. Ivatt and Mr. R. H. Adie.

Dr. Glaisher, F.R.S., and Mr. R. T. Glazebrook, F.R.S., of Trinity College, and Prof. G. B. Mathews and Mr. A. E. H. Love, F.R.S., of St. John's College, have been appointed Examiners for Part II. of the Mathematical Tripos; and Prof. Ewing, F.R.S., Prof. Reynolds, F.R.S., and Mr. J. B. Peace, of Emmanuel College, have been appointed Examiners for the Mechanical Sciences Tripos.

The twenty-second annual report on the local lectures has just been issued. It touches upon a number of interesting questions. Of the work temporarily undertaken for County Councils three years ago, the only portion that remains vigorous is that carried on in connection with the Norfolk County Council in the preparation of teachers in elementary schools to teach science subjects in evening classes. These courses, given in the county of Norfolk, have been supplemented by practical laboratory work in Cambridge during the Long Vacation, which has been attended by teachers holding scholarships from the Norfolk Council. The Syndicate state in the report that they are persuaded that this is a work of great value, and that they believe that it is in this direction, rather than by the provision of ordinary technical courses for rural audiences, that they can now best aid the technical education work of County Councils. During the past session the scheme of certificates has been remodelled so as to encourage more continuous and systematic work, and has already begun to show good results. The most important part of the report is that in which the Syndicate announce their intention to appeal for funds to enable the University to develop and extend the work in a more systematic way by placing particular districts in charge of superintendent lecturers, who will form a direct link between the district and the University. Appended to the report is a special report by Dr. R. D. Roberts, the secretary for lectures, in which a large scheme for the future development of the work is sketched and practical proposals suggested.

A Directory of Science, Art, and Technical Colleges, Schools, and Teachers in the United Kingdom, by Mrs. R. S. Lineham, has been published by Messrs Chapman and Hall. The directory will undoubtedly prove of great value to all who are concerned with scientific and technical education. It contains a list of schools arranged alphabetically according to towns, with the names of secretaries, principals, and teachers, and the number of students taught in each subject. There is also an alphabetical list of names and addresses of teachers of science, art, and technology, arranged under the headings of subjects taught. Other information of particular use to teachers under the Science and Art Department, and needed now and then by all promoters of elementary scientific education, will be found in the volume. Complete the directory is

not, nor is it infallible; but it is a praiseworthy attempt to organise into one guild the teachers of a growing and most important section of educational work. The labour involved in getting together the facts which make up the contents must have been immense, and it is to be hoped that, now the work has been done, the support required to ensure the annual publication of the directory will not be lacking. If the book only makes teachers in technical schools and institutes realise that they are part of one organic whole, having for its object the extension of scientific knowledge, it will accomplish a much-desired end.

THE Report of the Technical Educational Committee of the Berks County Council is optimistic, but it is not distinguished by descriptions of any very noteworthy developments. Berkshire is an agricultural county, and that is tantamount to saying that little encouragement is given to scientific education. Such counties are not willing to be taught much about principles; what they will tolerate, are subjects like practical butter-making, laundry-work, poultry-keeping, hedging, and horse-shoeing; but to attempt to teach agriculturists anything much beyond manual dexterity, is to court opposition. However, the Technical Education Committees are doing something to educate the agricultural mind to a better appreciation of the benefits to be derived from science, though it must be confessed that the rate of progress is extremely slow. Berkshire, along with Oxfordshire and Hampshire, contribute towards the maintenance of the University Extension College at Reading, and, in recognition of the satisfactory development of the agricultural department of the college, the Board of Agriculture recently granted a sum of £500, and the money could not have been better bestowed. The various courses of study at the college are well arranged, and valuable field experiments are carried on. By paying over the sum of £400 to the college, the Berkshire Committee ensures efficient instruction for the students under their care, and that is a very important consideration, for the supply of good teachers, competent to teach science as it should be taught, is comparatively small, to say nothing of the laboratory accommodation essential for truly scientific instruction. In spite of the facilities thus offered, the lectures in elementary science arranged for teachers were not successful. It would be a great pity if the Committee had to discontinue this part of their work on account of the want of support by the teacher for whom the lectures are intended. The other ways in which the Committee disposes of the funds allocated to technical education are evening continuation classes, scholarships, dairying, farriery, and bee-keeping. Aid is also given to classes in the principles of agriculture, mensuration, botany, drawing, horticulture, chemistry, mechanics as applied to agriculture, and to manual instruction in woodwork and metalwork.

THE Brussels correspondent of the *Times* reports that the electrical and anatomical institutes founded by M. Ernest Solvay, and presented by him and other donors to the University of Brussels, were officially inaugurated on Monday, under the presidency of the Burgomaster, assisted by M. Graux, the Chancellor, and the entire body of professors. Delegations from the English and Continental Universities have responded to the invitation of the Brussels University to take part in the series of *fêtes* organised in celebration of the event.

IT was announced a few weeks ago that the Treasury has thoughts of reinstating King's College, London, in the enjoyment of its share of the grants made to University Colleges. In consequence of this decision, the Council of the College have adopted a conscience clause as a standing regulation.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, October 25.—Mr. Walter Baily, Vice-President, in the chair.—Prof. J. Perry read a paper, by himself and Mr. H. F. Hunt, on the development of arbitrary functions. During the discussion on Prof. Henrici's paper (April 13, 1894), one of the authors described a graphical method of developing any arbitrary function in a series of other normal forms than sines and cosines, such as Bessel's or zonal spherical harmonics. The method consisted in wrapping the curve which represents the function round a specially shaped cylinder, not circular, and projecting this curve on to a certain plane. Many months were wasted in finding with great exactness a sufficient number of coordinates of the trace of the cylinder suitable for a Zeroth Bessel development. The labour, however, was unnecessary, since the coordinate most troublesome to calculate is not really needed, the projection only taking place in one direction. To develop any arbitrary function of x (say y) in normal forms, the real difficulty consists in finding the value of an integral such as $\int_0^a y.\ Q(x).\ dx$ where $Q(x)$ is some tabulated function. If now z is another tabulated function which is the integral of $Q(x)$, the required integral is $\int y dz$. If the values for y for 25 equidistant values of x are known, from $x = 0$ to $x = a$. Let the corresponding values of z be tabulated, and let a curve be drawn with the values of y as ordinates and the values of z as abscissæ; the area between the axis of z and this curve gives the value of the integral required. The authors give four tables containing the abscissæ for the four first terms in the development in Zeroth Bessels. They have tested the method by applying it to the calculation of a known function in terms of zonal spherical harmonics, and the agreement between the true value of the coefficients and those found is very satisfactory. Prof. Henrici said the method was a new departure, since in the place of an instrument of complicated design the authors only used a planimeter and pencil and paper, and obtained the same degree of accuracy. The fact that the series employed to test the method consisted of a finite number of terms seemed to him an objection. Prof. Karl Pearson had in a recent conversation informed him of a method for the development of functions which he (Prof. Pearson) had recently discovered. This method was not, however, so simple—at least in most cases—as that of the authors. Prof. Minchin thought it would add to the intelligibility of the paper if it were stated that the method was similar to that employed when expanding in terms of a Fourier series or in spherical harmonics. In these cases you have a function which, when multiplied by other functions of different orders, kills all the terms except one. Graphic methods ought, in his opinion, to be very much oftener employed, and he considered that there was no problem in physical mathematics of which the solution could not be obtained by graphic methods. He would also like to know if Prof. Perry had obtained a graphic method of calculating Bessels. Mr. Trotter agreed with Prof. Minchin as to the neglect of graphic methods. He regretted that Prof. Perry did not continue to consider the method as the projection from a cylinder, as he had found the method of wrapping curves round a cylinder most useful. Prof. Perry in his reply said he had adopted the expansion they had employed, under the impression that the test was a particularly severe one. He had not discovered a graphic method of calculating Bessels. The reason they gave up the cylinder was the immense labour involved in calculating the y coordinates of the trace, which would afterwards be of no use in the development of the function.—Mr. F. W. Lanchester read a paper on the radial cursor, a new addition to the slide-rule. The ordinary form of slide-rule enables calculations to be made which involve multiplication and division, also involution and evolution where the indices are integers. The radial cursor allows of the solution of problems in which fractional indices occur; for example, in questions involving the adiabatic expansion of a gas, where an expression of the form $pv_\gamma = \text{const.}$ has to be dealt with, and where γ is not an integer, nor is it constant for all gases. In this case it is necessary to provide some ready means of dividing the scales on the rule and slider proportionally to the value of γ, which corresponds to the division and multiplication of the respective logarithms of the quantities dealt with in the proportion of the indices of p and v, *i.e.* 1 and γ. This proportionate division of the scales is effected in the new cursor by a radial index-arm which is arranged to swing about a stud fixed to a sliding-bar running in guides at right angles to the rule. All readings are taken at the points of intersection of a line on the radius arm and the edges of the slide. The distance of the pivot, on which the radius arm turns, from the scale, and therefore the value of the index employed, is read off on a scale fixed to the transverse bar. Mr. C. V. Boys said that owing to the kindness of the author he had been able to try the cursor, and had found it of great service in dealing with questions of adiabatic expansion. The new addition to the slide-rule suffers under the same disadvantage as the rule itself, namely that a verbal or written description seems so very much more complex than is the actual operation when using the rule. The author's device might be described as an india-rubber slide-rule, for it performed the function of a slide-rule in which the graduations

[illegible] so [illegible] rather so that the ratio of the length [illegible] the rule is the length of the scale on the [illegible] at will, and thus involution and evolution [illegible] performed. Mr. Blakesley asked how [illegible] were dealt with. Prof. S. P. Thompson [illegible] Mr. [illegible] expressed their admiration for the author's [illegible] "stretching" the scale. Mr. Burstall said he had [illegible] apply a similar method to the Fuller rule, but did not [illegible], since in this scale there was only one scale. He [illegible] author's method could be applied in a form such that a greater accuracy than one in 500 could be obtained. Mr. [illegible] thought the fact that the point of intersection of two lines [illegible] acute angle had to be read was likely to limit the [illegible]. The author having replied, the Society adjourned [illegible] November 8.

PARIS.

Academy of Sciences, October 21.—M. Marey in the chair.—The decease of M. Hellriegel (Correspondent of the [illegible] Section), at Bernburg, Anhalt, on September 24, was announced to the Academy.—A study of graphite extracted from a pegmatite, by M. Henri Moissan. The author concludes, from the impressions of markings on the graphite crystals transferred to surrounding quartz and felspar, that the graphite existed before the pegmatite was formed. This graphite much resembles graphite formed in the electric furnace in fused metals, and may have been formed under somewhat similar conditions.—A study of some varieties of graphite, by M. Henri Moissan. Graphites found in nature may be divided, as recommended by M. [illegible], into intumescent and non-intumescent graphites. The former appear to have been produced in fused [illegible], the latter by the action of a raised temperature on [illegible] amorphous carbon.—On the Mounier Observatory, by M. [illegible]. Details are given concerning observations on the surface of Venus. The lack of alteration in the [illegible] part of the surface viewed during a con[illegible] supports Schiaparelli's contention that the planet [illegible] with great slowness.—M. Mascart presented an "Atlas of the 'isanomals' and secular variations of terrestrial magnetism," by M. M. de Tillo. The general conclusions able to be drawn from a study of the lines of secular variation are: (1) the changes of the elements so occur that in one hemisphere they are positive, and in the other negative; (2) there is a great similarity between the trend of the isanomals and that of the lines of equal secular variation.—Prof. Norman Lockyer presented [illegible] spectra taken with an objective and [illegible] by the Brothers Henry. The lines in the spectrum [illegible] correspond to those of helium. The absorption due [illegible] of stars showing few lines is due mostly to [illegible].—The following articles, by M. Cruls, are [illegible] Correspondence: (1) Posiçoes geographicas. The [illegible] determined are those of Rodeio, Entre-Rios, [illegible], and Barbacena, along the Central [illegible]. (2) [illegible] de Rio. Given by [illegible] from 1851 to 1890. (3) Eclipses de Soleil [illegible].—On a long period inequality in the longitude of [illegible]. An empirical correction proposed by [illegible] to the Le Verrier tables of geocentric longitude, and [illegible] want of sufficient precision in the determination of the value of a coefficient, is shown by the [illegible] by an independent method not to be [illegible] error in the tables, as his results agree exactly [illegible].—On the deformation of surfaces, by M. [illegible].—A correction to be applied to readings of [illegible], by M. Scheurer Kestner. The correction [illegible] applied to the Walferdin or Beckmann [illegible] for the mercury in the upper reservoir, [illegible] regards expansion shown [illegible].—Study on the latent heats of vaporisation of fatty [illegible], and of diethyl and dimethyl carbonates, by M. W. Longuinine. With regard to Trouton's formula $\frac{M l}{T}$ = constant (where M is the molecular weight of the substance, l its latent heat of vaporisation, and T its absolute boiling point), the results so far obtained warrant the general conclusions: (1) For each of the groups that have been studied $\frac{M l}{T}$ is very nearly constant. (2) It varies notably for different groups of substances. Latent heats may be calculated by the [illegible] value given to the constant within 1·5 per cent., and by [illegible] obtained from a determination by means of a substance of the same type within 1·5 per cent.—Peroxidised potassium derivatives of benzoquinone, by M. Ch. Astre. Benzoquinone contains only two atoms of hydrogen in its molecule replacable by a metal. The diketonic nature of benzoquinone is supported by the author's results.—On the composition of rice imported into France, by M. Balland.—On the toxicity of acetylene, by M. N. Grehant. Acetylene is poisonous when it occurs in air to the extent of 40 to 70 per cent., but is not nearly so injurious as ordinary lighting gas. M. H. Moissan added that pure liquefied acetylene possesses an agreeable ethereal odour, and causes no inconvenience when breathed in small quantity.—Serotherapy in the treatment of cancer, by MM. J. Hericourt and Ch. Richet.—On a new Lamellibranch (*Scioberetia australis*) commensal with an Echinoderm, by M. Felix Bernard.—On the age of the lignite formation of Southern Chili, the Arauacaria group, the Chilian equivalent of the Laramie and Chico-Tejon group of North America, by M. A. F. Nogues.—On the daily variations of relative humidity, by M. D. Eginitis.—On new observations in the Padiriac chasm (Lot), by M. E. A. Martel.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Elementary Physics: J. Henderson (Longmans).—The People of the Moon: T. Carter (Electrician Company).—An Account of Palmyra and Zenobia: Dr. W. Wright (Nelson).—Rambles in Japan: Dr. H. B. Tristram (R.T.S.).—Technical Educator, Vols. v. and vi. (Cassell).—Birds of Berwickshire: G. Muirhead, Vol. 2 (Edinburgh, Douglas).—Practical Trigonometry: H. Adams (Whittaker).—The Valley of Kashmir: W. R. Lawrence (Frowde).—Atlas of the Fertilization and Karyokenesis of the Ovum: Drs. Wilson and Leaming (Macmillan).

PAMPHLETS.—Anleitung zur Molekulargewichtsbestimmung: Dr. G. Fuchs (Leipzig, Engelmann).—Ueber den Zusammenhang Zwischen der Erdmagnetischen Horizontalintensität und der Inclination: Dr. H. Fritsche (St. Petersburg).—Compte Rendu des Travaux de la Société Helvétique des Sciences Naturelles reunie a Schaffhouse 1894 (Genève).—Congrès de la [illegible] Atmosphère, Anvers, 1894 (Anvers).—Communications: A. [illegible] (Anvers).

SERIALS.—Tufts College Studies, No. 4 (Tufts College, Mass.).—Zeitschrift für Wissenschaftliche Zoologie, lx. Band, 1 Heft (Leipzig, Engelmann).—Studies in Biology from the Biological Department of the Owens College, Vol. 3 (Manchester, Cornish).—Good Words, November (Isbister).—Sunday Magazine, November (Isbister).—Longman's Magazine, November (Longmans).—Journal of the Royal Microscopical Society, October (Williams).—Mittheilungen der Naturforschenden Gesellschaft in Bern, Nr. 1335-1372 (Bern).—Verhandlungen der Schweizerischen Naturforschenden Gesellschaft, 1894 (Schaffhausen).—Scientific Transactions of the Royal Dublin Society, Vol. v. series 2: The Papillary Ridges on the Hands and Feet of Monkeys and Men: D. Hepburn (Williams).—Humanitarian, November (Hutchinson).—Astrophysical Journal, October (Wesley).—Natural Science, November (Rait).

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